PONTIFICIAE ACADEMIAE SCIENTIARVM SCRIPTA VARIA

28

SEMAINE D'ETUDE

SUR

LE ROLE DE L'ANALYSE ECONOMETRIQUE DANS LA FORMULATION DE PLANS DE DEVELOPPEMENT

7 - 13 octobre 1963

Ie PARTIE

PONTIFICIA
ACADEMIA
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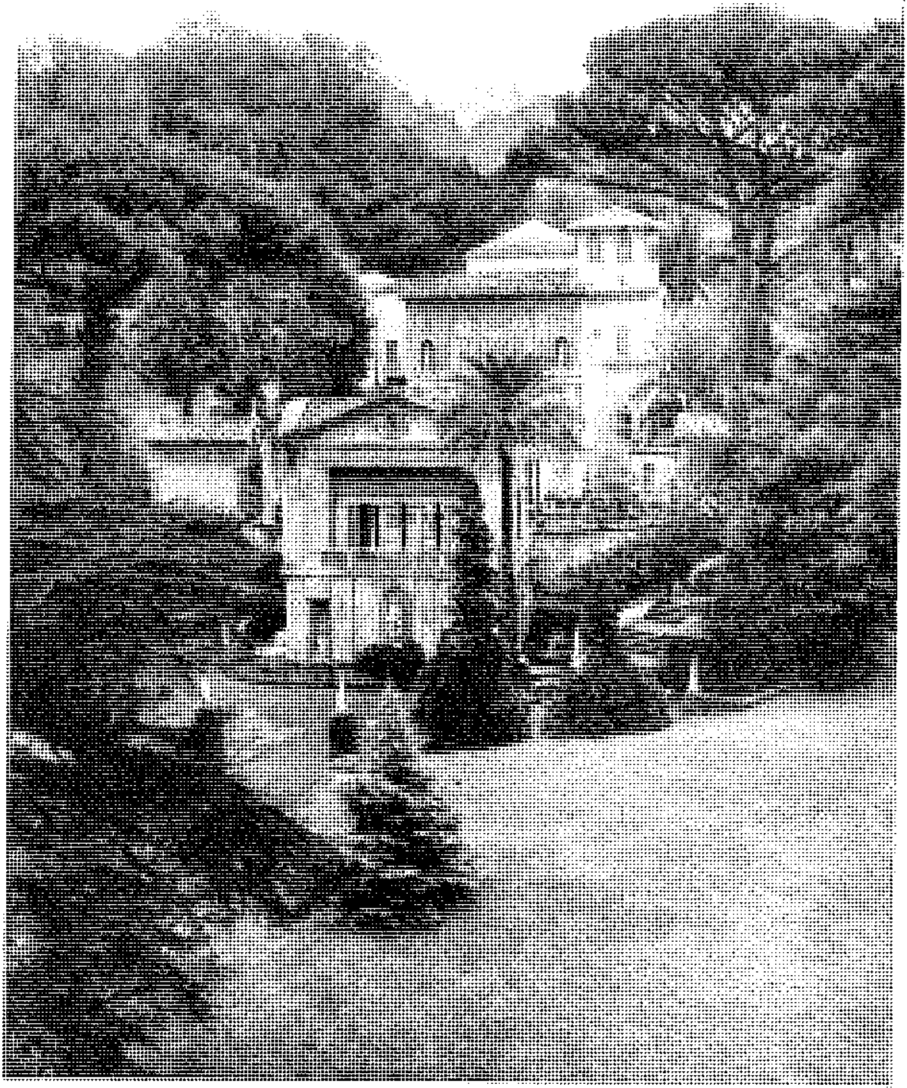
MCMLXV



PONTIFICIA ACADEMIA SCIENTIARVM CITTÀ DEL VATICANO

PONTIFICIAE ACADEMIAE SCIENTIARVM SCRIPTA VARIA

28

STUDY WEEK

ON

THE ECONOMETRIC APPROACH TO DEVELOPMENT PLANNING

october 7 - 13, 1963

FIRST PART

PONTIFICIA
ACADEMIA
SCIENTIARVM

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Adresse:

"Pontificia Academia Scientiarvm"
Casina Pio IV CITTÀ DEL VATICANO

LE ROLE DE L'ANALYSE ECONOMETRIQUE DANS LA FORMULATION DE PLANS DE DEVELOPPEMENT

Les économies modernes sont extrêmement complexes et la théorie et l'expérience démontrent que le libre jeu des choix individuels n'assure pas, comme on le croyait par le passé, des résultats favorables pour la collectivité.

Ceci posé s'ensuit la nécessité de préétablir des instruments appropriés de connaissance et de contrôle, et de fixer les objectifs vers lesquels l'activité économique doit être orientée.

De ces exigences a pris son départ l'Econométrie, qui emploie la méthode statistico-mathématique tant dans l'étude théorique des phénomènes économiques que pour formuler des directives de politique économique et des plans de développement.

L'Econométrie est une discipline relativement récente et, au sujet de sa structure, de ses méthodes et de ses finalités, s'est engagée une discussion animée entre les savants spécialistes du monde entier.

L'intérêt de cette discussion est accru du fait que l'on voit se répandre le recours à des plans de développement et à des politiques de contrôle de la conjoncture.

La méthode économétrique représente un progrès considérable sur les systèmes non mathématiques d'étude des phénomènes qui se rattachent à l'activité économique.

Elle a permis de créer les structures d'une nouvelle discipline ayant toutes les caractéristiques des sciences naturelles traditionnelles, parce que, bien qu'elle traite une matière substantiellement diverse de celle des disciplines physiques et biologiques, elle suit des procédés logiques et techniques qui la rendent strictement analogue à celles-ci.

La Semaine d'Etude que l'Académie Pontificale des Sciences a tenu en son siège dans les Jardins du Vatican et qui a réuni quelques-uns parmi les plus illustres spécialistes du monde en Econométrie, à cherché à étudier la contribution que l'analyse économétrique a apportée ou peut apporter à la connaissance des problèmes du développement et des fluctuations économiques.

PIETRO SALVIUCCI
Chancelier de l'Académie

LA SEMAINE D'ETUDE

SUR

LE ROLE DE L'ANALYSE ECONOMETRIQUE DANS LA FORMULATION DE PLANS DE DEVELOPPEMENT

Le but des « Semaines d'Etude » de l'Académie Pontificale des Sciences a été ainsi défini par son premier Président, S.E. le Rév.me Père AGOSTINO GEMELLI O.F.M. :

« Tandis qu'on fixait, après sa fondation, les travaux de l'Académie, un problème se présenta bien vite avec évidence : les sciences posent chaque jour des problèmes nouveaux qui donnent lieu d'ordinaire à divers essais de solution, souvent contradictoires. Il arrive ainsi constamment que parmi les représentants les plus autorisés d'une science et, en particulier, entre ceux qui se sont consacrés à l'étude d'une même question, on rencontre des opinions opposées. De pareilles divergences se maintiennent parfois pendant de longues périodes et constituent à la fois une grave difficulté pour l'enseignement des sciences et fréquemment aussi un obstacle considérable à leur développement. D'ailleurs, l'expérience montre que les méthodes actuellement pratiquées dans la discussion des problèmes scientifiques n'ont qu'une efficacité limitée au point de vue de l'établissement d'une unité de doctrine. Il serait hautement souhaitable de promouvoir tout ce qui pourrait favoriser une entente sur les points en discussion.

« Un tel procédé semble devoir être particulièrement utile sous ce rapport : savoir établir des contacts personnels prolongés entre quelques représentants d'opinions différentes au sujet d'une question déterminée ».

Dans ce but, l'Académie Pontificale des Sciences a réalisé une nouvelle « Semaine d'Etude » ayant pour titre: « Le rôle de l'analyse économétrique dans la formulation de plans de développement » (1).

Bien que ces derniers temps un travail intense ait été fourni sur les divers aspects de ce problème, il restait cependant quelques questions de détail à résoudre, et de nouvelles questions s'étaient de plus posées pendant ces dernières années.

Etant donné qu'on n'avait pas encore provoqué un débat approfondi à ce sujet et que le moment semblait propice pour le faire, l'Académie Pontificale des Sciences s'est proposée de réunir un nombre restreint de savants, spécialistes de la question. Son but était de recueillir, au cours d'une discussion approfondie, les synthèses des nombreuses recherches effectuées dans ce domaine; de formuler clairement l'état des différents problèmes qui s'y rapportent; et par là

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(1) Cette « Semaine d'Etude » sur « Le rôle de l'analyse économétrique dans la formulation de plans de développement » est la septième de la série.

La première « Semaine d'Etude » a eu lieu du 6 au 13 juin 1949; elle a été dédiée au « Problème biologique du Cancer », et a été présidée par l'Académicien Pontifical S. E. PIETRO RONDONI, Professeur de Pathologie générale et expérimentale à l'Université de Milan, y ont participé personnellement 15 savants tandis que 3 autres ont envoyé des mémoires. Les comptes-rendus de la « Semaine d'Etude » ont été publiés dans le 7ème volume des « Scripta Varia » de l'Académie; ils représentent un volume de 364 pages.

La deuxième « Semaine d'Etude » a eu lieu du 19 au 26 novembre 1951; elle a été dédiée au « Problème des Microséismes », et a été présidée par l'Académicien Pontifical S. E. FRANCESCO VERCELLI, Directeur de l'Institut Thalassographique et de l'Observatoire Géophysique de Trieste; y ont participé personnellement 15 savants tandis que 4 autres ont envoyé des mémoires. Les comptes-rendus de la « Semaine d'Etude » ont été publiés dans le 12ème volume des « Scripta Varia » de l'Académie; ils forment un volume de 466 pages.

La troisième « Semaine d'Etude » a eu lieu du 24 avril au 2 mai 1955; elle a été dédiée au « Problème des Oligoéléments dans la vie végétale et animale », et a été présidée par l'Académicien Pontifical S. E. JOSÉ MARIA ALBAREDA HERRERA, Directeur de l'Institut de Pédologie et de Physiologie de pouvoir fixer les directives de recherche les plus logiques, les plus persuasives et les plus prometteuses, étant donné l'état actuel de la science.

A cet effet ont été invités par l'Académie des experts qualifiés en économétrie, en économie, en économie politique et en statistique, qui, grâce à leurs études spécifiques, ont contribué à eclaircir le rôle fondamental de l'analyse statistico-mathématique dans la formulation de plans de developpement.

La présidence de cette « Semaine d'Etude » sur « Le rôle de l'analyse économétrique dans la formulation de plans de développement » a été confiée par le Président de l'Académie Pontificale des Sciences, S.E. le Rév.me Monseigneur GEORGES LEMAÎTRE, à l'Académicien Pontifical S.E. MARCELLO BOLDRINI, Professeur de Statistique à l'Université de Rome, et l'organisation générale au Chancelier de l'Académie Pontificale des Sciences Prof. PIETRO SALVIUCCI.

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végétale de l'Université de Madrid, Secrétaire Général du Conseil Supérieur des Recherches Scientifiques d'Espagne; y ont participé personnellement 19 savants tandis qu'un autre a envoyé un mémoire. Les comptes-rendus de la « Semaine d'Etude » ont été publiés dans le 14ème volume des « Scripta Varia » de l'Académie; ils forment un volume de 630 pages.

La quatrième « Semaine d'Etude » a eu lieu de 20 au 28 mai 1957; elle a été dédiée au « Problème des Populations stellaires » et a été présidée par l'Académicien Pontifical Surnuméraire le Rév.me Père DANIEL J. K. O'CONNELL, Directeur de la « Specola Vaticana » de Castelgandolfo; y ont participé personnellement 21 savants. Les comptes-rendus de la « Semaine d'Etude » ont été publiés dans le 16ème volume des « Scripta Varia » de l'Académie; ils forment un volume de 615 pages.

La cinquième « Semaine d'Etude » a eu lieu du 23 au 31 octobre 1961; elle a été dédiée au « Problème des macromolécules d'intérêt biologique avec référence spéciale aux nucléoprotéides », et a été présidée par l'Académicien Pontifical S.E. ARNE TISELIUS, Professeur de Biochimie à l'Université de Uppsala; y ont participé personnellement 28 savants. Les comptes-rendus de la « Semaine d'Etude » ont été publiés dans le 22ème volume des « Scripta Varia » de l'Académie; ils forment un volume de 544 pages.

La sixième « Semaine d'Etude » a eu lieu du 1 au 6 octobre 1962; elle a été dediée au « Problème du rayonnement cosmique dans l'espace inter-

Ont été invités à la réunion les savants suivants:

Prof. Dr. MAURICE ALLAIS, Professeur d'Economie générale à l'Ecole Nationale Supérieure des Mines et d'Economie théorique à l'Institut de Statistique à l'Université de Paris - *Paris* (France).

S.E. Prof. MARCELLO BOLDRINI, Académicien Pontifical, Professeur de Statistique à l'Université de Rome - *Rome* (Italie).

Prof. Dr. ROBERT DORFMAN, Professeur d'Economie politique à la Harvard University - *Cambridge, Mass.* (U.S.A.).

Prof. Dr. FRANKLIN M. FISHER, Professeur associé d'Economie politique au Massachusetts Institute of Technology - *Cambridge, Mass.* (U.S.A.).

Prof. Dr. RAGNAR FRISCH, Professeur d'Economie à l'Université de Oslo et Directeur de l'Institut des Recherches économiques à la même Université - *Oslo* (Norvège).

Prof. Dr. TRYGVE HAAVELMO, Professeur d'Economie politique à l'Université de Oslo - *Oslo* (Norvège).

Prof. Dr. WALTER ISARD, Professeur d'Economie politique à l'Université de Pennsylvanie et Président honoraire de l'Association de Science Régionale - *Philadelphia, Penn.* (U.S.A.).

Prof. Dr. D. GALE JOHNSON, Professeur d'Economie politique et Doyen de la Division des Sciences sociales à l'Université de Chicago - *Chicago, Ill.* (U.S.A.).

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planétaire », et devait être présidée par l'Académicien Pontifical S.E. VICTOR FRANCIS HESS, Professeur émérite de Physique à l'Université Fordham de New York; malheureusement l'Académicien VICTOR FRANCIS HESS n'a pas pu, en raison de son état de santé, être présent et la Semaine d'Etude a été présidée par S.E. l'Académicien GEORGES LEMAÎTRE, Professeur de Mécanique et de Méthodologie mathématique à l'Université de Louvain et Président de l'Académie. Y ont participé personnellement 24 savants. Les comptes-rendus de la « Semaine d'Etude » ont été publiés dans le 25ème volume des « Scripta Varia » de l'Académie; ils forment un volume de 574 pages.

Prof. Dr. TJALLING CHARLES KOOPMANS, Professeur d'Economie politique à la Yale University et Directeur de la Cowles Foundation for Research in Economics - *New Haven, Conn.* (U.S.A.).

Prof. Dr. WASSILY W. LEONTIEF, Professeur d'Economie politique à la Harvard University et Directeur du Harvard Economic Research Project - *Cambridge, Mass.* (U.S.A.).

Prof. Dr. PRASANTA CHANDRA MAHALANOBIS, Directeur honoraire de l'Institut Indien de Statistique, Conseiller honoraire pour la Statistique du Gouvernment Indien, Membre de la Commission de Statistique des Nations Unies - *New Delhi* (Inde).

Prof. Dr. EDMOND MALINVAUD, Directeur de l'Ecole Nationale de la Statistique et de l'Administration Economique - *Paris* (France).

Prof. Dr. MICHIO MORISHIMA, Professeur d'Economie politique à l'Université d'Osaka - *Kobe* (Japon).

Prof. Dr. LUIGI PASINETTI, Professeur d'Economie politique, Membre et chargé de cours d'Economie politique au King's College - *Cambridge* (Grande-Bretagne).

Prof. Dr. ERICH SCHNEIDER, Professeur d'Economie politique et Directeur de l'Institut für Weltwirtschaft à l'Université de Kiel - *Kiel* (Allemagne).

Prof. Dr. JOHN RICHARD NICHOLSON STONE, Professeur de Finance et de Comptabilité à l'Université de Cambridge, Membre du King's College - *Cambridge* (Grande-Bretagne).

Prof. Dr. HENRY THEIL, Professeur d'Econométrie et Directeur de l'Institut d'Econométrie à l'Ecole néerlandaise d'Economie - *Rotterdam* (Pays-Bas).

Prof. Dr. JAN TINBERGEN, Professeur d'Economie à l'Ecole néerlandaise d'Economie de Rotterdam - *Rotterdam* (Pays-Bas).

Prof. Dr. HERMAN O. A. WOLD, Professeur de Statistique à l'Université d'Uppsala, Membre de l'Académie des Sciences de Suède - *Uppsala* (Suède).

Tous les invités, à l'exception du Prof. JAN TINBERGEN, qui n'a pas pu intervenir, ont participé à la Réunion.

Le Président de la « Semaine d'Etude » S.E. l'Académicien Pontifical MARCELLO BOLDRINI à appelé à faire partie du Secrétariat scientifique le Prof. ANTONINO GIANNONE, le Prof. GIANCARLO MAZZOCCHI, le Dr. PIERO GIARDA, le Dr. PIERCARLO NICOLA, le Dr. GIACOMO VACIAGO.

Le « Règlement des Semaines d'Etude » prescrivant que le nombre des Participants doit être rigoureusement limité, a malheureusement empêché d'inviter d'autres illustres savants.

Ont aussi participé à la réunion: en qualité d'interprète et chef de Secrétariat Mme VALENTINE PRÉOBRAJENSKI; en qualité de sténographes polyglottes de séance Mlles MAURA BALOCCO et PAMELA SUTTON; en qualité de sténo-dactylographes polyglottes chargées des Procès-verbaux: Mlle VALERIA CRAJA, Mlle JOSÉPHINE LUCAS et Mme PAULETTE ROSSALDI; en qualité de technicien pour l'enregistrement et la projection, Mr MAURO ERCOLE, assisté par des opérateurs de Radio-Vatican. Le Bureau de Presse était confié au Dr. FRANCESCO SALVIUCCI, Coadjuteur du Chancelier de l'Académie.

Le Comité de Réception pour les Dames dirigé par Mme HÉLÈNE LOTTI, était composé de la Comtesse KARINA CALVI DI COENZO, la Comtesse ISABELLA CALVI DI COENZO et Mlle MARIA LUISA LOTTI.

Le dimanche 13 octobre tous les Participants ont été reçus en Audience Solennelle par le Souverain Pontife qui leur adressa un discours et après l'Audience a eu lieu, au Siège de l'Académie Pontificale des Sciences, une séance extraordinaire de l'Académie, à laquelle ont été invités également les Participants à la « Semaine d'Etude ».

Tous les participants à la Semaine d'Etude ont reçu toutes les communications à l'avance. Durant la Semaine d'Etude chaque communication a été discutée tant par un groupe de spécialistes [1] qu'en une session plénière, exception faite pour deux communications (STONE et FRISCH) qui onté tét directement discutées en session plénière. Les communications ont été mises ici dans une succession aussi rapprochée que possible à leur ordre de présentation et de discussion.

Les séances se tenaient deux fois par jour, le matin de 9 h. 30 à 12 h. 30 et l'après-midi de 16 h. à 19 h.

La réussite de la « Semaine d'Etude » a pleinement satisfait les illustres Participants qui, à la fin de leurs travaux, ont tenu à exprimer au Saint Père leur profonde gratitude et leur très sincère admiration pour cette manifestation scientifique si réussie, en envoyant à l'Auguste Pontife, animateur et mécène de l'Académie, le télégramme suivant:

« *Sa Sainteté le Souverain Pontife Paul VI, Cité du Vatican. — Les participants de la Semaine d'étude sur le rôle de l'analyse économetrique dans la formulation des plans de developpement et l'étude des fluctuations économiques prient Sa Sainteté de daigner accepter l'expression de leur respect et de leur gratitude. Grâce au milieu idéal offert par son Académie des Sciences, ils ont eu la possibilité*

[1] 1er groupe: STONE (Président), DORFMAN, JOHNSON, KOOPMANS, MAHALANOBIS, MALINVAUD, MORISHIMA, PASINETTI, SCHNEIDER. 2e groupe: LEONTIEF (Président), ALLAIS, FISHER, FRISCH, HAAVELMO, ISARD, THEIL, WOLD.

de discuter dans une atmosphère de sérénité et d'indépendance intellectuelle des problèmes de grande actualité. Ils espèrent que leurs travaux ajouteront, serait-ce dans une mesure modeste, au progrès de la Science et à l'amélioration de la condition humaine. — ALLAIS, BOLDRINI, DORFMAN, FISHER, FRISCH, HAAVELMO, ISARD, JOHNSON, KOOPMANS, LEONTIEF, MAHALANOBIS, MALINVAUD, MORISHIMA, PASINETTI, SCHNEIDER, STONE, THEIL, WOLD ».

A ce télégramme d'hommage et de remerciement, le Saint-Père a daigné répondre par le message suivant, signé par Son Eminence le Cardinal Secrétaire d'Etat:

« *Professeur Salviucci, Chancelier Académie Pontificale des Sciences - Casina di Pio IV - Città del Vaticano. — Sa Sainteté très touchée délicat Message participants Semaine étude sur rôle analyse économétrique dans formulations plans développements se réjouit contribution illustres savants progrès de la Science et amélioration condition humaine renouvelle sentiments paternelle bienveillance dans récent discours invoque sur continuation recherches distingués Académiciens abondantes faveurs divines.* — Card. CICOGNANI ».

Dans les pages qui suivent, après le compte-rendu de l'Audience du Saint-Père et le « Règlement des Semaines d'Etude », sont imprimés les rapports originaux présentés à la Réunion, et les discussions qui les ont suivis, mis en ordre et publiés par les soins du Dr. PIERO GIARDA et du Dr. GIACOMO VACIAGO.

Les « Conclusions » de la « Semaine d'Etude » se trouvent à la fin du présent volume.

Pendant la Semaine, les Participants ont visité les Musées Pio-Clementino, Chiaramonti, Etrusque, Egyptien; le Braccio Nuovo; les Galeries des tapisseries, des cartes géographiques; les Chambres et les Loggia de Raphaël, la Chapelle de Fra Angelico, la Chapelle Sixtine, l'Appartement Borgia et la Pinacothèque Vaticane avec l'assistance du Prof. Comm. FILIPPO MAGI et du Dr. DEOCLECIO REDIG DE CAMPOS de la Direction Générale des Monuments de la Cité du Vatican.

Les Participants ont aussi visité la Bibliothèque Apostolique Vaticane et les Archives Secrètes Vaticanes sous la conduite des Rév.mes Préfets Mons. MARTINO GIUSTI et Père ALPHONSE RAES, S.I. et la Station de Radio-Vatican qui leur fut présentée par le Directeur, le Rév.me Père ANTONIO STEFANIZZI S.I.

Enfin, le soir du même samedi 12 octobre, un dîner d'adieu a été offert par l'Académie, selon la coutume, aux savants participant à la « Semaine d'Etudes ».

L'AUDIENCE

ET

LE DISCOURS DU SAINT-PERE

Le matin de dimanche 13 octobre, le Saint-Père a accordé dans la Salle du Consistoire du Palais Apostolique Vatican, une Audience Solennelle à l'Académie Pontificale des Sciences à l'occasion de la « Semaine d'Etude » sur « Le rôle de l'analyse économétrique dans la formulation de plans de développement » tenue par l'Académie même. Ont participé aussi à l'Audience de nombreux hauts personnages.

Etaient présents Leurs Eminences les Cardinaux: EUGÈNE TISSERANT, Président honoraire de l'Académie; AMLETO GIOVANNI CICOGNANI, Secrétaire d'Etat, GIUSEPPE PIZZARDO et ANSELMO ALBAREDA, Académiciens honoraires; GIACOMO LUIGI COPELLO, JOSEPH FRINGS, PAUL EMILE LÉGER, FRANZISKUS KÖNIG, JOSEPH LEFEBRE, BERNARD JAN ALFRINK, EFREM FORNI.

De nombreux Académiciens Pontificaux sont intervenus, et spécialement Leurs Excellences: le Rév.me Monseigneur GEORGES LEMAÎTRE Président, JOSÉ MARIA ALBAREDA-HERRERA, MARCELLO BOLDRINI, GIOVAMBATTISTA BONINO, HERMANN ALEXANDER BRÜCK, CARLOS CHAGAS FILHO, GUSTAVO COLONNETTI, EDWARD JOSEPH CONWAY, EDUARDO CRUZ-COKE, GEORGE CHARLES DE HEVESY, JOHN CAREW ECCLES, JOSÉ GARCIA-SIÑERIZ, ALESSANDRO GHIGI, GIORDANO GIACOMELLO, WALTER RUDOLF HESS, CORNEILLE HEYMANS, CYRIL NORMAN HINSELWOOD, BERNARDO ALBERTO HOUSSAY, ALBERTO HURTADO, LOUIS LEPRINCE-RINGUET, DOMENICO MAROTTA, SAN-ICHIRO PAULO MIZUSHIMA, ANTONIO PENSA, ENRICO PISTOLESI, MANUEL SANDOVAL-VALLARTA, GEORGE SPERI-SPERTI, HIDEKY YUKAWA; les Académiciens Pontificaux Surnuméraires: Rév. P. DANIEL JOSEPH KELLY O'CONNELL S.I. et Mgr. MARTINO GIUSTI; le Chancelier de l'Académie Prof. Dr. PIETRO SALVIUCCI et le Coadjuteur du Chancelier Dr. FRANCESCO SALVIUCCI.

Parmi le groupe des Académiciens assistaient les savants spécialistes « Participants » à la Semaine d'Etude sur « Le rôle de l'analyse économétrique dans la formulation de plans de développement » MM. les Professeurs: MAURICE ALLAIS, ROBERT DORFMANN, FRANKLIN M. FISHER, RAGNAR FRISCH, TRYGVE HAAVELMO, WALTER ISARD, D. GALE JOHNSON, TJALLING CHARLES KOOPMANS, WASSILY W. LEONTIEF, PRASANTA CHANDRA MAHALANOBIS, EDMOND MALINVAUD, MICHIO MORISHIMA, LUIGI PASINETTI, ERICH SCHNEIDER, JOHN RICHARD NICHOLSON STONE, HENRY THEIL, HERMAN O.A. WOLD et les Secrétaires Scientifiques: Prof. Dr. ANTONINO GIANNONE, Prof. Dr. GIANCARLO MAZZOCCHI, Dr. PIERO GIARDA, Dr. GIACOMO VACIAGO.

Etaient également présents: Son Excellence Rév.me Monseigneur ANTONIO SAMORÉ Secrétaire de la Sacré Congrégation des Affaires Ecclésiastiques Extraordinaires, Son Excellence Rév.me Monseigneur ANGELO DELL'ACQUA Substitut de la Secrétairerie d'Etat; Son Excellence Rév.me Monseigneur CARLO GRANO Nonce Apostolique en Italie; un groupe d'Assesseurs et de Secrétaires des Sacrées Congrégations, ainsi qu'un groupe d'Archevêques et d'Evêques, parmi lesquels LL.EE. Rév.mes Nosseigneurs DIEGO VENINI, PIETRO CANISIO VAN LIERDE, PIETRO SIGISMONDI, PRIMO PRINCIPI, et autres personnalités de la Curie et de l'Etat de la Cité du Vatican.

Au complet le Corps Diplomatique accrédité près le Saint-Siège, dont les Membres furent reçus par le Gr. Uff. Dr. MARIO BELARDO.

Le Saint-Père a fait son entrée dans la Salle du Consistoire à 10 heures, accompagné par sa Noble Antichambre avec LL.EE. Nosseigneurs FEDERICO CALLORI DI VIGNALE Majordome et MARIO NASALLI ROCCA DI CORNELIANO Maître de Chambre, par son Secrétaire Particulier Monseigneur PASQUALE MACCHI, ses Camériers Secrets Participants et sa Garde Noble.

Une déférente manifestation d'hommage a accueilli l'arrivée du Saint-Père.

Après avoir gagné le trône, le Saint-Père donna son assentiment au Président LEMAÎTRE qui s'adressa alors au Souverain Pontife en ces termes:

« Très Saint-Père, il y a un an, Sa Sainteté le Pape JEAN XXIII daignait recevoir les Membres de l'Académie Pontificale des Sciences réunis en Séance Plénière, ainsi que les participants à la Semaine d'Etude sur "Les Rayons Cosmiques dans l'espace interplanétaire".

« Nous ne nous doutions pas que c'était la dernière fois que nous pouvions recevoir ses paternels encouragements pour nos travaux et lui exprimer notre profonde vénération.

« Je ne pouvais pas ne pas évoquer avec émotion ce souvenir au moment où son vénéré Successeur nous accueille à son tour avec la même paternelle bonté à la clôture de nos travaux de la Session Plénière et de la Semaine d'Etude sur "Le rôle de l'analyse économétrique dans la formulation des plans de développement".

« Il nous est impossible d'exprimer adéquatement tout ce que nous devons déjà à Sa Sainteté le Pape PAUL VI.

« Nous savons qu'à côté de ses illustres prédécesseurs, il a eu une part des plus actives dans l'érection et le développement de l'Académie Pontificale des Sciences et nous connaissons la profonde et affectueuse amitié qu'il témoignait pour son regretté Président, le Père AGOSTINO GEMELLI.

« Nous ne pouvons nous empêcher de penser que notre Académie, par sa large ouverture à toutes les formes de la Science et à toutes les personnalités du monde scientifique sans égard pour leur appartenance philosophique ou religieuse, a pu faire présager et inaugurer dans un domaine particulier ce large souffle de liberté et de respect de toutes les valeurs humaines qui anime en ce moment l'Eglise et qui, sous la direction éclairée de ses vénérés Pontifes, lui fait aborder un des plus grands tournants de son histoire.

« Je prie Sa Sainteté de daigner agréer le respectueux hommage de son Académie des Sciences ».

Le Saint-Père daigne répondre par le discours que nous reproduisons plus loin.

A la fin de l'Audience le Souverain Pontife daigna remettre au Prof. Dr. AAGE BOHR, Professeur de Physique theorique à l'Université de Copenhague, la grande médaille d'or qui porte le nom auguste de Pie XI, Fondateur de l'Académie Pontificale, et adressait au Prof. BOHR, qui était accompagné du Président et du Chancelier de l'Académie des paroles de satisfaction et des félicitations.

Le Saint-Père s'entretint ensuite, après avoir reçu l'hommage des Cardinaux, avec le Président LEMAÎTRE, les Académiciens Pontificaux, le Chancelier SALVIUCCI et les savants « Participants » à la « Semaine d'Etude », trouvant pour chacun d'aimables paroles de félicitations et de souhaits, pour eux, leurs familles et leur activité scientifique.

L'assistance exprima enfin ses remerciements au Saint-Père, sa reconnaissance émue et sa profonde gratitude, et le plus chaleureux hommage se manifesta de nouveau au moment où, l'Audience terminée, le Souverain Pontife quitta la Salle du Consistoire.

Messieurs,

Nous n'avons pas l'intention de vous faire un discours. Ce n'est pas que Nous n'aurions bien des choses à vous dire: cette rencontre avec l'Académie pontificale des Sciences éveille en effet dans Notre âme toutes sortes de thèmes, de questions, de sentiments, qui mériteraient que Nous leur donnions expression. Mais ce n'est pas le moment. En ces jours, absorbés par le Concile et par les problèmes qu'il soulève, le temps Nous manque. Ce ne sera donc qu'une brève salutation que Nous vous adressons, salutation pleine de cordialité pour les personnes que Nous avons le grand honneur de rencontrer,

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Gentlemen,

We do not propose to deliver a discourse. Not that we should not have plenty to say to you; this meeting with the Pontifical Academy of Sciences in fact calls to mind all sorts of topics, questions, feelings, which it would be worth while to express, but this is not the moment. In these days, absorbed as they are by the Council and the problems to which it gives rise, We have no time to spare. This will be merely a brief greeting that we address to you, a greeting full of cordiality for the persons that We have the great honour of meeting, full of respect for this institntion that We are happy to see here once again.

As you have just said, Mr. President, an esteem of long standing and a sincere friendship bind Us to your Academy. We are glad *pleine de déférence pour l'institution que Nous avons l'heureuse occasion de revoir.*

Comme vous venez de le dire, Monsieur le Président, une estime datant de loin et une sincère amitié Nous lient à votre Académie. Il Nous est agréable de refaire aujourd'hui connaissance avec elle et de saluer d'abord en votre personne, Monsieur le Président, le digne successeur du regretté et inoubliable Père Gemelli.

C'est une joie pour Nous de retrouver l'Académie, dans la plénitude de ses effectifs, appliquée à poursuivre fidèlement ses activités traditionnelles.

Et à ce propos, Nous Nous faisons un devoir de confirmer aux anciens académiciens Nos sentiments dévoués, et de souhaiter une joyeuse bienvenue à ceux des nouveaux que Nous n'avons pas eu encore le plaisir de saluer comme membres de cette illustre société.

Nous voulons aussi exprimer Notre reconnaissance aux personnalités qui ont accueilli l'invitation de Notre Académie et sont venues prendre part à cette semaine d'études, y apportant la précieuse con-

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to be able to-day to renew acquainstance with it and to greet, first of all, in you, Mr. President, the worthy successor of the lamented and unforgettable Padre Gemelli.

It is for Us a joy to find the Academy, and all its members, dedicated to the faithful carrying out of its traditional activities. We take this opportunity to express to the veteran Academicians our devoted esteem and to bid a happy welcome to those whon We have not previously had the pleasure of greeting as members of this illustrious society.

We wish also to express Our gratitude to those scientists who have accepted the invitation of Our Academy and who have come to take part in this semaine d'études, bringing to it the valuable contribution of their learned research and honouring it with their presence.

tribution de leurs savants travaux et la flatteuse adhésion de leur présence.

Nous voulons ainsi confirmer à ceux qui appartiennent à l'Académie pontificale des Sciencse et à ceux qui participent à ses travaux ou l'honorent de leur sympathie Notre haute estime pour cette institution et, en conséquence, la résolution qui Nous anime de lui accorder l'appui et l'honneur capables d'assurer sa stabilité et de favoriser son développement.

Elle est solennelle, à Nos yeux, la responsabilité qui Nous vient du Pape fondateur de votre Académie; profonde, l'estime que Nous nourrissons pour ceux qui en sont les membres et les promoteurs; aiguë est en Nous la conscience de l'importance et des besoins de la haute culture scientifique de notre temps; vivant et agissant dans notre âme, le sentiment du devoir, de l'intérêt, et, dans un certain sens, de la nécessité, pour l'Eglise catholique, d'entretenir les rapports les plus sincères avec le monde scientifique contemporain. Disons enfin que Nous Nous sentons stimulé par la certitude que notre

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To those who belong to the Pontifical Academy of Sciences, and to those who participate in its work or honour it with their friendly interest, we wish to reaffirm our high esteem for this institution, and the resolution we have taken to grant it the support and honour which will ensure its stability and favour its development.

We have inherited a solemn responsibility from the Pope who founded your Academy, for whose members and promoters We cherish a profound esteem, We have a keen appreciation of the importance and the needs of modern science and a lively sense of the duty, the interest, and in a way the necessity, for the Catholic Church to maintain the most sincere relations with the contemporary scientific world. Finally We may say that We feel ourselves stimulated by the certainty that our religion not only does not oppose any real objection to the study of natural truths, but that, without *religion, non seulement n'oppose aucune objection réelle à l'étude des vérités naturelles, mais qu'elle peut, sans sortir des limites de sa propre sphère, ni franchir celles du domaine de la science proprement dite, aider la recherche scientifique, honorer ses résultats, favoriser leur meilleure utilisation pour le bien de l'humanité.*

La religion que nous avons le bonheur de professer est, en effet, la science suprême de la vie: elle est donc la plus haute et la plus bienfaisante maîtresse dans tous les domaines où la vie se manifeste. Elle pourra sembler absente quand non seulement elle permet, mais ordonne au savant de n'obéir qu'aux lois de la vérité; mais à y regarder de près, elle sera encore près de lui pour l'encourager dans sa difficile exploration, en lui assurant que la vérité existe, qu'elle est intelligible, qu'elle est magnifique, qu'elle est divine; et pour lui rappeler, à chaque pas, que la pensée est un instrument apte à la conquête de la vérité et qu'il faut l'utiliser avec un tel respect pour ses propres lois que l'on sente continuellement la référence à une responsabilité qui l'engage et la transcende.

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crossing the bounds of its proper sphere or transgressing those of the domain of science properly so-called, it can promote scientific research, honour its results and help them to be better used for the good of humanity.

The religion which we have the happiness to profess is, in fact, the supreme science of life. It is thus the highest and most beneficent mentor in all those domains where life is manifested. It might seem to be absent when it not merely permits, but directs, the scientist to obey only the laws of truth. When, however, it is looked at more closely, it will be seen to be still beside him, to encourage him in his difficult task of exploration, assuring him that truth exists, that it is intelligible, splendid, divine; and also to remind him at every step that thought is an instrument for the conquest of truth and that it should be used with such respect for its own laws that one feels continually the transcendent responsibility that it imposes.

C'est vous dire, Messieurs, avec quel sérieux et avec quelle faveur Nous considérons cette institution dans laquelle Nous Nous plaisons à voir une représentation du monde scientifique, auquel Nous envoyons, à cette occasion, et par le moyen des interprètes autorisés que vous êtes, Notre salut respecteux et Nos encouragements.

Ce salut peut être symbolisé par la médaille d'or « Pie XI », que Nous avons le plaisir de remettre au professeur Aage Bohr. fils d'une nation dont Nous apprécions les insignes mérites, le Danemark —, savant célèbre pour ses études sur la structure nucléaire et sur l'analyse théorique des mouvements des noyaux atomiques. Que la remise de cette récompense soit une marque d'admiration et d'encouragement, tant pour la digne personne de ce jeune professeur, que pour la noble phalange, devenue aujorud'hui une véritable armée, des savants engagés dans la moderne et merveilleuse exploration du microcosme physique.

Que, venant de Nos mains sacerdotales, ce prix soit une cha-

This will show you, Gentlemen, how seriously and with what favour We regard this institution, which We like to consider as representative of the scientific world, to which We send through you, its authoritative interpreters, Our respectiful greetings and encouragement.

A symbol of this greeting is the Pius XI Gold Medal which We have the pleasure of presenting to Professor Aage Bohr, son of Denmark, a nation whose signal merits are appreciated by Us, a scientist celebrated for these studies of nuclear structure and for the theoretical analysis of the motions of atomic nuclei. May the granting of this award be a token of respect and encouragement, both for the worthy person of this young professor as well as for the noble company, nowadays become a whole army, of scientists devoted to the exploration of the marvels of the physical microcosm.

Coming from Our priestly hands may this award constitute a *leureuse invitation, un appel évangélique à tous les responsables: qu'ils ne fassent jamais de la science, ou plutôt de ses multiples applications pratiques — en particulier de la science nucléaire et de ses formidables emplois possibles —, un péril, un cauchemar, un instrument de destruction pour la vie humaine. Déjà un autre de Nos sages prédécesseurs, Pie XII, dès 1943, et encore en 1948, mettait en garde, devant cette même Académie, contre la terrible et menaçante possibilité que l'énergie atomique pût devenir fatale pour l'humanité. Et récemment encore, le Pape Jean XXIII, d'heureuse mémoire, dans son Encyclique* « Pacem in terris », *désormais célèbre, formait le voeu de la prohibition des armes atomiques.*

Nous voulons faire Nôtre leur cri paternel et, avec tous les hommes pleins de bonté et de sagesse qui sont dans le monde, souhaiter que soit conjurée une telle menace au salut et à la paix de l'humanité.

Dans votre pacifique assemblée, grâce à Dieu, vous êtes loin de ces perspectives si ténébreuses. Vous y parlez du « rôle de l'ana-

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warm invitation, an evangelical appeal, to all those in authority — that they may never make science, or rather its multiple practical applications, in particular those of nuclear science and its terrible possibilities — that they may never make it a peril, a nightmare, an instrument of destruction for human life. Another of Our wise predecessors, Pius XII, already in 1943 and again in 1948, addressing this same Academy, warned against the terrible and menacing possibility that atomic energy might become fatal for humanity. And, still more recently, Pope John XXIII, of happy memory, in his now famous Encyclical « Pacem in Terris », expressed the wish that atomic weapons be banned.

We wish to make Our own their fatherly appeal and to hope, with all good and wise men everywhere in the world, that this threat to the safety and peace of humanity may be averted.

lyse économétrique dans la formulation des plans de développement ». C'est là le thème de votre semaine d'études, un thème qui tend à rassembler les résultats modernes d'une branche scientifique nouvelle, l'économétrie, et à les présenter à la politique économique, pour l'aider à formuler ces plans de sécurité mieux assurée et de plus grand développement qui peuvent tant apporter pour le bien-être et la paix des peuples.

Nous ne voulons pas aborder ce thème ni y ajouter de commentaires. Mais Nous sommes heureux que des personnes si éminentes soient venues l'exposer devant cette Académie, et Nous les remercions de cette haute contribution qu'ils apportent ainsi au progrès de la science et à la bonne renommée de cette même Académie. Nous tenons à vous exprimer Nos félicitations pour le choix, la manière de traiter et les buts d'un thème aussi riche pour la recherche scientifique que fécond en applications pratiques. Nous sommes sûr aussi que ces études d'économétrie, intégrées aux autres connaissances

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In your peaceful assembly you are, thank God, far removed from these sombre prospects. You will be speaking of « The econometric Approach to Development Planning ». This is the theme of your study week, a theme which seeks to gather together the latest results of a new branch of science, econometry, and to present them to political economists in order to aid them in formulating those plans for a more stable security and for greater development which can contribute so much to the well-being and peace of nations.

We do not intend to enter upon this theme or to comment on it, but We are happy that such eminent men have come to treat of it before this Academy, and We thank them for this important contribution which they are making to the advance of science and to the reputation of this Academy. We are happy to congratulate you *des phénomènes humains, y compris dans le domaine économique, seront vraiment de grande utilité au progrès ordonné de la civilisation humaine.*

Et, en vous saluant paternellement, Nous implorons sur vos personnes et sur vos travaux la protection de Dieu, en vous donnant à tous Notre Bénédiction Apostolique.

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on the choice, the method of treatment and the aim of a theme as fruitful for scientific research as it is rich in practical applications. We are sure also that these econometric studies, integrated with the rest of our knowledge of human phenomena, including those in the field of economics, will truly prove of great utility in the ordered progress of human civilisation.

We give you a fatherly greeting and beg the divine protection for you and for your labours, bestowing on you all Our Apostolic Blessing.

LES « SEMAINES D'ETUDE »

ET

LEUR REGLEMENT

Lorsque l'Académie Pontificale des Sciences fut fondée par le Souverain Pontife Pie XI, de vénérée mémoire, par son « Motu Proprio » du 28 octobre 1936 « In multis solaciis », cette initiative suscita dans les milieux scientifiques un mouvement général de sympathie et d'admiration. Cette institution unique au monde, qui groupait en une même assemblée des représentants de toutes les nations civilisées était appelée, en effet, à de hautes destinées dans le développement de la pensée scientifique.

D'autre part, cette oeuvre de coopération fut accueillie avec un véritable soulagement par tous ceux que plongeait dans le désarroi le plus profond la période qui suivit la guerre 1914-18. On voyait, en effet, s'altérer profondément les caractères d'objectivité et de désintéressement propres au travail scientifique, et s'affirmer même une tendance à asservir la science à des fins pragmatiques.

Tout au contraire, dans l'immortel « Motu Proprio » du 28 octobre 1936, le Pape Pie XI proclamait solennellement la dignité de

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A general movement of sympathy and admiration was aroused in scientific circles when, in 1936, the Pontifical Academy of Science was founded by His Holiness Pope Pius XI, of venerable memory, by means of his « Motu Proprio » of October 28, « In Multis solaciis ». This institution, the only one of its kind in the world, which brought the representatives of all civilized nations into touch with each other, was, in fact called upon to play a leading role in the development of scientific thought.

This work of cooperation was, moreover, welcomed with a sense of real relief by all those who were plunged in a deep state of confusion in the period following the 1914-18 war.

Signs of drastic changes were, in fact, discernible in the objective and disinterested nature of scientific work and even a tendency to make science subject to pragmatic aims.

la recherche de la vérité pour elle-même (*), et, élevant sa pensée au-dessus de toute préoccupation utilitaire, affirmait qu'il ne demandait rien d'autre aux nouveaux « Académiciens Pontificaux » que de se consacrer, avec une ferveur toujours plus grande, au progrès de la science et, par là, au culte de la vérité: « C'est Notre souhait ardent et Notre ferme espérance que par cet Institut, à la fois Nôtre et leur, les "Académiciens Pontificaux" contribuent toujours plus et mieux au progrès des sciences. Nous ne leur demandons pas autre chose; car en ce dessein généreux et ce noble labeur consiste le service, qu'en faveur de la vérité, Nous attendons de leur part ».

La consécration pratique de cette idée, par la nomination d'un certain nombre de non-catholiques parmi les nouveaux Académiciens Pontificaux, a fait une profonde impression sur beaucoup d'esprits, comme l'ont montré les réactions de la presse internationale de l'époque et de nombreux témoignages individuels d'hommes de science et des plus grands savants du monde.

(*) « Nobis autem in votis expectationeque est, fore ut "Pontificii Academici" vel per hoc Nostrum suumque studiorum Institutum, ad scientiarum progressionem fovendam amplius excelsiusque procedant; ac nihil praeterea aliud petimus, quandoquidem hoc eximio praeclaroque labore famulatus ille nititur servientium veritati, quem ab iisdem postulamus ».

In his immortal « Motu Proprio » of October 28, 1936, Pope Pius XI, on the contrary, solemnly proclaimed the dignity of the search for truth for its own sake (*) and, raising his thoughts above all preoccupations of an utilization nature, asserted that all he asked of the new « Pontifical Academy » and its members was that they should dedicate themselves, with increasing fervour, to the furthering of the progress of science and, consequently, to the cult of truth: « It is Our ardent wish and firm hope that, by means of this Institute, which is both Ours and theirs, the "Pontifical Academicians" will contribute to an increasingly great extent to the progress of science. We ask nothing more than that from them because the service in favour of truth that We expect from them consists in this generous intention and noble work ».

By including a certain number of non-Catholics amongst the new Pontifical Academicians, the practical application of this idea made a deep impression on many persons, as is proved by the reaction of the interna-

Beaucoup de préjugés à l'égard de l'Eglise ont été fortement ébranlés par ce geste du Souverain Pontife qui a obligé à reconnaître la place éminente réservée aux valeurs purement intellectuelles dans l'Eglise Catholique.

Pour toutes ces raisons, la fondation de l'Académie Pontificale des Sciences a été hautement appréciée dans le monde scientifique et y a fait naître de grands espoirs quant aux possibilités d'action d'une institution si opportune.

Le Saint-Père Pie XII, qui avait collaboré avec son Prédécesseur au projet et à la fondation de l'Académie et qui l'avait représenté comme Légat personnel lors de l'inauguration solennelle, ne s'est pas borné à maintenir à son égard ses sentiments de haute estime par sa présence à de solennelles séances académiques, où il daigna prononcer ses discours d'une haute portée scientifique; il a tenu, en outre, à lui donner un nouveau témoignage de son auguste satisfaction en accordant à ses membres le titre d'Excellence par le Bref Apostolique du 25 novembre 1940.

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Les sciences posent chaque jour des problèmes nouveaux qui donnent lieu d'ordinaire à divers essais de solution, souvent contra-

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tional press of the time and by the innumerable individual tributes paid by scientists and by the greatest scholars of the world.

Many prejudices against the Church were very deeply shaken by this gesture on the part of the Sovereign Pontiff, since it called attention to the lofty place reserved for purely intellectual values in the Catholic Church.

For all these reasons, the foundation of the Pontifical Academy of Science was greatly appreciated by the scientific world and aroused high hopes as to the prospects open to such a timely institution.

His Holiness Pope Pius XII, who had helped his predecessor to draw up the plan and to found the Academy, and who had represented Him as His personal Legate at the time of its solemn inauguration, did not confine himself to the expression of lofty sentiments when attending solemn academic gatherings, where he deigned to make speeches of great scientific importance, but he also afforded proof of his august satisfaction by granting the title of Excellency to the members of the Academy, by an Apostolic Brief of November 25, 1940.

dictoires. Il arrive ainsi constamment que parmi les représentants les plus autorisés d'une science, et, en particulier, parmi ceux qui se sont consacrés à l'étude d'une même question, on rencontre des opinions opposées. Pareilles divergences se maintiennent parfois durant de longues périodes et constituent à la fois une grave difficulté pour l'enseignement des sciences et fréquemment aussi un obstacle considérable à leur développement.

Par ailleurs, l'expérience montre que les méthodes actuellement pratiquées dans la discussion des problèmes scientifiques n'ont qu'une efficacité limitée au point de vue de l'établissement d'une unité de doctrine.

Il serait dès lors hautement souhaitable de promouvoir tout ce qui pourrait favoriser un accord sur les points en discussion.

Un procédé semble devoir être particulièrement utile sous ce rapport: à savoir, l'établissement de contacts personnels prolongés entre quelques représentants d'opinions différentes au sujet d'une question déterminée.

En effet, le contact personnel entre hommes de science constitue, sans aucun doute, le moyen le plus efficace de résoudre les controverses scientifiques.

Dans ce but, l'Académie Pontificale des Sciences a décidé d'organiser de pareilles rencontres scientifiques. L'organisation de ces ren-

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Every day science raises new problems, which usually give rise to various, and often contradictory, solutions. Consequently it often happens that amongst the most authoritative representatives of a given branch of science, and particularly amongst those who are engaged in studying the same question, one meets with contrasting opinions. Divergences of this kind often exist over long periods of time and are a serious obstacle not only to the teaching of science but also to its development.

Experience shows, moreover, that the methods at present in use in the discussion of scientific problems have only a limited efficacy in so far as concerns doctrinal unity.

It would, therefore, be highly desirable if everything that could favour agreement on controversial points were to be promoted.

contres qu'on a appelées « Semaines d'Etude » a été établie de la manière suivante :

RÈGLEMENT DES SEMAINES D'ÉTUDE

1. - L'Académie invite quelques illustres savants, parmi ceux qui, ayant étudié spécialement une question déterminée, sont arrivés à des conclusions différentes, à se rencontrer à Rome, à son siège, la « Casina di Pio IV », à l'intérieur de l'Etat de la Cité du Vatican, afin d'y procéder en commun, en dehors de toute autre préoccupation, à un examen général de toutes les données du problème.

2. - Le but essentiel de ces discussions est de chercher à formuler de façon précise les raisons qui sont à la base de la divergence des opinions. Les savants conviés aux réunions s'engageraient d'avance à concentrer leurs efforts dans cette direction.

3. - Un examen critique de ces raisons aboutira soit à un accord sur une solution déterminée, soit à la constatation qu'à l'état actuel des connaissance il est impossible d'établir une unité de doctrine au sujet du problème envisagé.

One process that would seem to be particularly useful from this point of view would be the establishment of prolonged personal contacts between some of the representatives of different trends of thought on a given subject.

Personal contacts amongst scientists are, in fact, the most efficacious means of solving scientific controversies.

With this aim in mind, the Pontifical Academy of Science decided to organize scientific meetings of this description. These meetings, known as « Study Weeks », were planned on the following lines:

STANDING RULES FOR « STUDY WEEKS »

1. - The Academy invites a number of illustrious scholars — comprising those who have especially studied a given question and have arrived at different conclusions — to meet in Rome at its headquarters, the « Casina di Pio IV », situated in the Vatican City, so as to make a joint examination, free from all other preoccupations, of all data concerning the problem.

2. - The chief aim of these discussions is to endeavour to formulate precisely the reasons which are at the root of the differences of opinion.

Dans ce dernier cas, les savants invités auront pour tâche:

a) de préciser les motifs pour lesquels un accord s'avère présentement irréalisable;

b) de définir le genre de recherches qu'il serait souhaitable d'entreprendre en vue de résoudre la question.

4. - L'invitation ne sera adressée par l'Académie qu'à un très petit nombre de représentants de chaque science: ceux-ci seront choisis parmi les personnalités étrangères à l'Académie, auxquels se joindront, dans la discussion, les Académiciens versés dans la même discipline. Cette invitation, de plus, ne se rapportera qu'à l'étude d'une question déterminée, pour chaque science.

5. - Les discussions auront un caractère strictement privé; elles prendront la forme de conversations particulières, sans autre assistance que celle de quelques membres de l'Académie Pontificale des Sciences particulièrement compétents dans la matière.

Des interprètes polyglottes, des sténographes, des rapporteurs, etc., seront mis à la disposition des savants réunis.

6. - Les « Conclusions » des discussions seront publiées sous la

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The scholars invited to these meetings undertake in advance to concentrate their efforts on this.

3. A critical examination of these reasons should lead, either to agreement on a given solution or else to the conclusion that, on the basis of the information actually available, it is impossible to establish doctrinal unity on the problem envisaged.

In the latter event the scholars concerned will be called upon:

a) to define the reasons why agreement appears to be impossible for the present;

b) to specify the kind of research work it would be desirable to undertake with a view to solving the problem.

4. - The invitation will be addressed by the Academy to only a small number of representatives of each branch of science: these will be selected from amongst those who are not connected with the Academy. They will be joined during the discussions by Academicians versed in the same discipline. This invitation, moreover, will apply only to the study of one precise problem in each branch of science.

forme d'une « Note Collective Finale » (à laquelle pourront éventuellement être jointes des annotations individuelles), mentionnant:

a) les points sur lesquels un accord aurait été réalisé;

b) les points sur lesquels un accord n'aurait pas paru réalisable;

c) les raisons pour lesquelles l'accord n'aurait pu être réalisé:

d) des suggestions relatives aux recherches paraissant les plus aptes à résoudre les difficultés.

7. - Les « Conclusions » seront aussitôt imprimées et communiquées, par les soins de l'Académie Pontificale des Sciences, à tous les centres scientifiques qu'elles seraient de nature à intéresser.

8. - Tous les frais de voyage et de séjour à Rome des personnalités invitées seront à la charge de l'Académie Pontificale des Sciences. L'hospitalité sera assurée dans l'un des principaux hôtels de Rome.

L'Académie se fera un plaisir d'offrir la même hospitalité aux épouses des savants invités, à l'exclusion toutefois des frais de voyage.

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5. - The debates will be strictly private and will take the form of personal talks, in the presence only of a few members of the Pontifical Academy of Science with special knowledge of the subject under discussion.

Polyglot interpreters, stenographers, reporters, etc. will be placed at the disposal of the participants.

6. - The « Conclusions » arrived at will be published in the form of a « Collective Note » (to which may eventually be added individual notes) mentioning:

a) the points on which agreement was reached;

b) the points on which it was impossible to reach agreement;

c) the reasons why it was not possible to reach agreement;

d) suggestions regarding the research work which appears most suitable for arriving at a solution of the difficulties.

7. - The « Conclusions » reached will be immediately printed and transmitted, by the Pontifical Academy of Science, to all the scientific centres which might be interested therein.

8. - All travelling expenses, and accommodation in one of the best hotels in Rome of the persons invited to the meetings will be borne by the Pontifical Academy of Science.

The Academy will be pleased to offer similar accommodation to the wives of the scholars who are invited, but not their travelling expenses.

TRAVAUX SCIENTIFIQUES
ET
DISCUSSIONS

PUBLIÉS PAR LES SOINS
DU DR. PIERO GIARDA
ET DU DR. GIACOMO VACIAGO

All participants to the Study-Week have received all the papers in advance. At the Study-Week each paper has been discussed both in a group of specialists (*) *and in a plenary session, except for two papers* (STONE *and* FRISCH) *which have been discussed directly in plenary session. The papers have been put here in a succession which is as close as possible to the chronological order of presentation and discussion.*

(*) 1st group: STONE (Chairman), DORFMAN, JOHNSON, KOOPMANS, MAHALANOBIS, MALINVAUD, MORISHIMA, PASINETTI, SCHNEIDER.
2nd group: LEONTIEF (Chairman), ALLAIS, FISHER, FRISCH, HAAVELMO, ISARD, THEIL, WOLD.

THE ANALYSIS OF ECONOMIC SYSTEMS

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I

MODELLING ECONOMIC SYSTEMS

1. The Background

It has long been a commonplace that an economy is a system, an exceedingly complex probabilistic system. According to the classical theory, the information flowing in this system consists of price signals. Constrained principally by the law and public opinion on the one hand and by the state of technology on the other, individuals and groups respond to these signals mainly in terms of self-interest. First, the productive part of the system produces as efficiently as possible what the consuming part wants to consume. Second, in every part of the system strong tendencies exist to check any departure from equilibrium. Third, not only is every firm efficiently operated but new ideas and inventions are adopted as soon as they become profitable and so the standard of living grows as fast as human ingenuity can make it. In other words, the system is efficient, stable and progressive. It gives the maximum scope to individual desires and initiatives and, like physical or biological systems, is regulated by means of viable governors set in motion by the system itself and not by any outside organ of control. To its other virtues, therefore, a final one is added: compatibility in the highest degree with human freedom.

There was a time, not so long ago, when the rulers of the world wished to believe this story and, generally speaking, succeeded in doing so. But the imperfections of *laissez faire* as a mode of economic organisation are so glaring that it has been either thrown out altogether, as in the socialist countries, or modified out of all recognition by state intervention even in countries devoted to the principle of free enterprise. So angry have men been at the abuses, injustices and waste of resources around them, so strong has been their desire for change, that they have shown very little appreciation of the good points of the system they were destroying and so have made very little effort to incorporate them in the system that was to take its place.

The purpose of this paper is to discuss how economic models might help us to reconcile the advantages of central planning with those of individual initiative. The basic ideas are simple: first, whether we consider a private firm or a government agency, sensible decisions cannot be reached unless there is an adequate amount of information flowing within the system and available in the right place at the right time; and second, since some kinds of information are expensive, if not impossible, to transfer from one decision centre to another, it makes a great deal of difference which decisions are taken at which centre.

As to the first point, private firms and government agencies usually try to enrich the flow of information in their neighbourhood by making special surveys, projections and so on. But just because the economy is a system, because, that is, its different parts are interdependent, the task is very difficult for any institution acting in isolation. Without a centralised service of consistent projections which concerns itself both with social objectives and with practical possibilities, individual forecasts are likely to be unduly conservative. For example, a realisable plan might require the output of a particular industry to double in a decade, but if no such plan exists the industry in question can hardly be expected to try to double its output in the next decade if in the past its market has been expanding at a slower rate.

As to the second point, in operating a system that is not deterministic, adjustments of one kind or another are constantly needed at all stages. There are costs, not to say dangers, in requiring all decisions to be referred to a central authority. Accordingly, we should examine carefully the sources of information in the system, the costs of transferring information, and the rules needed to ensure that centralised and decentralised decisions will not conflict.

2. Models and their Environment

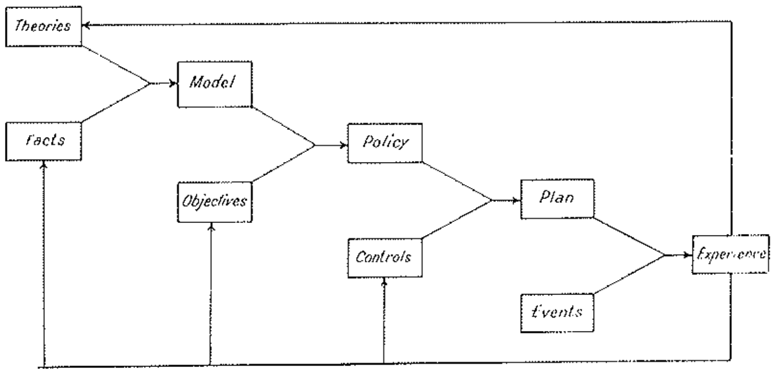
The main discussion of this paper will centre round a computable model of the economic system. Before we come to the general features of such models and to the particular example that I shall give, which is a revised version of that described in [7], it may be useful if I try to set an economic model in its environment, that is, relate it to the objectives it is intended to serve, the administrative arrangements needed to make it work and the general experience of economic life which it modifies and by which in turn it is modified.

Following the treatment in [37], the essence of the situation as I see it can be represented graphically as in diagram 1 below.

The combination of theories with facts gives rise to a model. Since economics is largely concerned with quantities, the model must be quantitative; and since we need to know not only how

DIAGRAM 1

A Model in its Environment



the system would behave if left to itself but also what would be the problems and results of trying to change it in various respects, the model must be readily computable. Model-building is a scientific activity but I do not think that a usable model of an economic system can be built by a group of scientists working in isolation, for the simple reason that, however well-endowed the group may be, it cannot possibly know as much as it needs to know about all aspects of the economy without the cooperation of many people engaged in a variety of practical tasks. The model-builders can of course build a prototype, but they will be very unwise if they do not seek a great deal of practical advice before they go fully into production.

The combination of a model with a set of objectives gives rise to a policy. This is where the politician comes in: he formulates the objectives. He may specify objectives in detail or he may delegate his responsibility, as when, with whatever safeguards he thinks desirable, he accepts the principle of consumers' sovereignty: this means that consumers may spend their money as they please, and the economic system must try to provide the goods they want and not some other goods. Thus, just as the initiative in model-building lies with the scientist, so the initiative in formulating the objectives lies with the politician. But neither works on his own; each is dependent on social acceptance and the cooperation of others.

The combination of a policy with a set of administrative procedures, or what I have called controls, gives rise to a plan. This is where the administrator, both public and private, comes in. But he too may delegate his authority and indeed in many cases must do so if the cost of basing all decisions on sufficient information is to be minimised. The relationship between centralised and decentralised decisions has been the subject of recent papers by MALINVAUD [23] and by KORNAI and LIPTÁK [20], [21].

When the plan is put into practice it comes up against events and this gives rise to our experience of the part of life with which the plan deals. As a result every element that led to the plan may to some extent be modified. It is inconceivable that initially we should have succeeded in building a perfect model, specifying a perfect set of objectives or designing a perfect set of controls. The possibility of these modifications is indicated in diagram 1 by the feedbacks from experience to theories, facts, objectives and controls, from which the effects of experience spread to the model, the policy and the plan.

Of course this is a very simplified presentation of a very complicated process. In fact, each stage in the process throws light on the preceding stages as well as on the succeeding ones, and each part of the system interacts to a greater or less degree with most of the other parts, so that actually there is much more feedback between the boxes than is shown in the diagram. For example, the model is to some extent determined by the objectives it is designed to serve, and the objectives are likely to be modified in the light of calculations made with the model. Again, the policy is bound to be affected by the structure of the existing control mechanism, while this is likely to be modified in the interests of the policy. And so on. In the next chapter we shall examine these connections in greater detail.

II

MODELS, POLICIES AND PLANS

1. MODELS

The purpose of building a model of anything is to understand how the thing works and, if possible, to make it work better. As BALL has argued [2] in reviewing the model of economic growth that I and my colleagues are working on in Cambridge, the great thing is to make a start and to follow this up by intensive work on the less satisfactory components of the initial version. It is a mistake to try to perfect all the components at the outset. Such an attempt may easily result in no model at all, for one never achieves initially a degree of perfection in the components that makes one entirely happy about leaving them alone. Furthermore, as he says, the adequacy of particular types of relationship can never be properly judged in isolation but should be evaluated in terms of their role in the complete model. Our motto, therefore, is *solvitur ambulando*.

The decisions to be taken in building a model can be grouped under four main beadings: *a*) the variables with which the model is concerned; *b*) the relationships by means of which the variables are connected and the precise form that these relationships are to take; *c*) the statistical and other methods by which the parameters in the relationships are to be estimated; and *d*) the methods of numerical analysis to be used in calculating the parameters and the unknown elements in the system. Let us now look at each heading in turn.

a) *Variables*. The basic concepts of economics are production, consumption and accumulation, to which may be added, since national economic systems are never isolated, foreign trade. The variables relevant to these concepts can, as everyone knows, be set out in a system of national accounts. These accounts have the merit of all accounts: their entries satisfy certain arithmetic and accounting identities.

The entries in accounts are values: sums of money. If an entry relates to a product, whether final or intermediate, or to a primary input, it can be decomposed into a quantity and a price. Thus values, quantities and prices have their places in an accounting system. Consistency requires, therefore, that we set up at least the main variables in our model within an accounting framework.

The technique of doing this is called social accounting and consists essentially in subdividing the national accounts. For example, in input-output analysis we subdivide the national production account so as to provide a separate account for each group of commodities that the economy produces. When we start doing this we find that different parts of the economy habitually use different systems of classification. Our system of social accounts should accommodate these different classifications and show how they are reconciled. In this way we can establish behavioural or technical relationships for different parts of the economy which reflect the categories habitually used by each part. For example, private consumers' expenditure is usually expressed in terms of a classification of goods and services which follows the lines of a consumer's shopping list; and public consumers' expenditure is usually expressed in terms of various purposes, such as education, health and defence. Neither of these classifications has a one-to-one correspondence with the industrial classification of products, and these products, in turn, are not in one-to-one correspondence with the industries in which they are produced. Conseqnently, if we wish to be able to establish relationships for the components of each of these categories we must make sure that each of them is properly represented in our accounting system. Some thoughts on this subject are given in [8], [36].

b) Relationships. The general nature of the relationships connecting production, consumption, accumulation and foreign trade was defined long ago by the great economists of the second half of the nineteenth century, such as JEVONS, WALRAS, MARSHALL and PARETO. Their contribution was, precisely, to formulate an economy as a system. These general ideas were set out fully and explicitly by BOWLEY [4], who remarked in his introduction that « there seems to be no book in existence, at least in English, that presents in a coherent form the mathematical treatment of the theory of political economy which has been developed during the past eighty years or more. » This was said in 1924.

But these general ideas provide no more than a guide. In trying to formulate the economy as a system, the great writers of the past naturally made many simplifying assumptions. For example, in the theory of consumers' behaviour, they assumed that an individual tries to maximise the utility of his consumption subject to a fixed set of prices and a budget constraint. In this theory individual preferences are assumed to be fixed. If, like these writers, we are concerned with formulating economic relationships in a general way, this is a legitimate assumption which leaves us free to concentrate on the constrained-maximum problem whose solution is the outcome of the theory. But if we are interested in empirical demand analysis we cannot make such an assumption since we know that individual preferences change, partly because the circumstances of the individual change with age and partly because, with time, new commodities appear and social attitudes and fashions change. Thus is studying the pattern of consumers' expenditure over time, it may be more important to find a way of allowing for changes in preferences than to have a very sophisticated means of allowing for responses to income and prices.

The earlier writers were aware of such complications, but their first concern was to realise their vision of the economy as a system, not to work out its operating characteristics. MARSHALL said in 1896: « the nineteenth century has in great measure achieved *qualitative* analysis in economics; but it has not gone farther. It has felt the necessity for *quantitative* analysis, and has made some rough preliminary surveys of the way in which it is to be achieved: but the achievement itself stands over for you. »

During the last fifty years or so, the development hoped for by MARSHALL has slowly gathered momentum and has been accompanied by a great deal of useful theorising. Our first task now is to perfect, in the light of the vast mass of quantitative information available, the relationships sketched out by our predecessors: production functions, consumption functions and so on. But just as the simplifying assumptions of the earlier theorists restrict particular relationships to the point where they cannot be applied, so also do they sometimes restrict the field of phenomena that should be considered in an economic model. Thus our second task is to formulate new relationships wherever experience shows them to be needed.

An example of this is the distribution of skills which accompanies any technique of production. Labour is a factor of production, and everybody recognises that there are many different kinds of labour, which require different kinds of education and training. Nevertheless, in production functions labour is usually treated as homogeneous. This is acceptable as a first approximation provided one it confident that in some unspecified way the right kinds of skill will be produced and flow to that part of the system where they are needed. But will they? Granted that demand *tends* to create its own supply, may we not find, in a period of rapid technical change, a serious lag between the skills needed for the new techniques and the skills in fact provided by the system of education and training? The answer seems to be « yes », in which case education and training become a necessary part of an economic model, not something which can be left on one side as a social process irrelevant to economics.

Another important point to keep in mind about the relationships in an economic model is whether they are to be used to answer questions or to ask them. For example, if the model-builder knew enough about the alternative techniques of production that will become available in the near future in different industries, he could say what changes in labour productivity could and should be brought about in order to reach certain objectives. In practice, however, the model-builder is most unlikely, at first, to know as much as this. But he can work out on reasonable assumptions a set of changes in labour productivity in different industries which would enable a given vector of output to be produced with a given quantity of labour. Then, instead of answering the question 'what increase in labour productivity is needed in each industry' he could ask the question 'could the increase in labour productivity I have worked out be achieved and, if not, what are the obstacles to achieving it'. By bringing such questions out into the open and showing what is needed if certain results are to be obtained, a great deal can be learnt and the people responsible for action can be provided with useful information. It may be that an industry simply cannot do what a model initially suggests; but it may equally happen that it could do as much or more if it believed in other results of the model, for example the estimated future demand for its products. In this case the model-builder is simply providing agenda for industry discussions. This may often be more useful than trying to anticipate the outcome of these discussions, since the discussions themselves may provide information to the model-builder more reliable than he could possibly have reached on his own, and may thus enable him to produce statements about the possible future of the economy which are realistic as well as consistent.

The final point I want to make about relationships concerns time-horizons. Any discussion of how best to operate the economy so as to achieve certain ends involves comparisons over time and leads to a variational problem. In theory there is no reason to stop at any particular point in the future and so we are led to a consideration of an infinite time-horizon. Valuable insights can be gained by this method, as in RAMSEY's theory of saving [34], but the weakness of the method from a practical point of view is that it calls for knowledge that we cannot possibly possess. This suggests that we should reformulate the problem with a finite, indeed fairly short time-horizon, a practice that is in fact adopted in all centrally planned economies.

c) *Estimation.* Having decided on the variables that are to enter into the model and on the forms of the relationships by which they are connected, the next thing to do is to estimate the parameters in these relationships. This may be a matter of simple arithmetic, as when an input-output coefficient is estimated by dividing the input of product j into product k by the output of product k. So simple a method, however, is only resorted to when there is an extreme shortage of information; more generally statistical methods, and in particular regression analysis, are involved. In any case the estimation procedure that must inevitably be followed at the outset can be described as the econometric analysis of past observations.

The methods of estimation available to econometricians have improved very considerably over the last generation. In particular it has come to be realised from HAAVELMO's original paper on the subject [16] that the fact that an economy is a system in which different influences may operate simultaneously has a bearing on the appropriate method of estimating economic relationships. This is called the problem of identification: in relating the price of a commodity to the quantity of it sold, how can we identify the parameter as a demand parameter, a supply parameter or some mixture of the two; alternatively, how can we arrange our estimation procedure so that the estimate we obtain is, let us say, a demand parameter? This problem has been the subject of intensive study at the Cowles Commission [11] [12] and more recently has been giveu an elegant form by presentation by HURWICZ [18] and, in terms of statistical estimation, by DURBIN [13].

Important as these developments are, they certainly do not solve all the problems that the model-builder has to face. One of the main problems arises from the fact that relationships are constantly changing. I have already given consumers' preferences as an example of this. Another example is offered by the techniques of production: a past input-output table will not be a good description of present intermediate technology; *a fortiori* it will not be a good description of the intermediate technology of the future. How then are we to proceed? The best thing to do is undoubtedly to consult outside experts about the way in which inputs have been changing, and to get from them an estimate of future input structures in different industries. But such information can only be expected from a limited number of highly articulate industries. In other cases one can sometimes find time-series of input-output coefficients and project these into the future. Often, however, this approach is closed too, and there is nothing to be done but to project input-output coefficients by a general method of extrapolation. A means of doing this is given in [36] and, in greater detail, in [9].

Another practical problem arises from the fact that, in formulating relationships, we usually begin by considering only the more general influences which we believe to be at work. For example, in formulating demand relationships we usually begin by allowing for the effects of income, prices and changing tastes on the demand for different commodities. We know of course that other, more specific, influences are at work: an abnormally cold winter or hot summer, the temporary rationing of some commodity, a particular advertising campaign. Sucb specific influences are numerous and difficult to take into account, and their effect on the broad pattern of consumption is not very noticeable; but in accounting for the detailed pattern they may well be important. In such cases it may be useful to follow a hierarchical principle. Thus we might start by estimating how, in given circumstances, total consumption would be divided among its main constituents, namely food, clothing, household expenses etc. We might then try to estimate how the expenditure on each of these gronps would be divided among the group's components and what specific influences, if any, might affect each component. And so on. As we got into greater and greater detail, however, we should expect any manageable system of demand relationships to break down. At this point we should have reached a position where our demand model was no longer specific enough and where, accordingly, we needed outside help, as in the case of the input-output relationships discussed above.

What I have just said brings out the fact that with the information at our disposal there is a limit to the amount of detail we can handle. It is sometimes argued that if only we could increase the size of our models we should get correspondingly better results. I do not think this is true. If, for example, we were to multiply the number of industries we distinguish, and were to represent each industry in the detail necessary to operate it, we should have to make the relationships of our model altogether more sophisticated. In practice we could not do this. I suggest therefore that the proper way to introduce great detail into a model of the economy is not to expand that model beyond a certain point, but to set up separate submodels for different industries, related to the general model but established and operated by the industries themselves with all the expert knowledge that this would make possible. The final outcome would result from an iteration between the general model and the industry models, somewhat on the following lines. The general model would indicate the output levels required and the distribution of these outputs over uses. The industry models would then convert these demands into specific products and decide how these products could best be produced. The general model would then check the total primary-input requirements of the productive system and indicate whether any primary inputs were in short supply and, if so, to what extent they should be economised on. It would also change its initial cost structures and indicate revised output requirements. Such a treatment would allow for a manageable expansion of the initial model and at the same time would ensure that the information available was put to the best use.

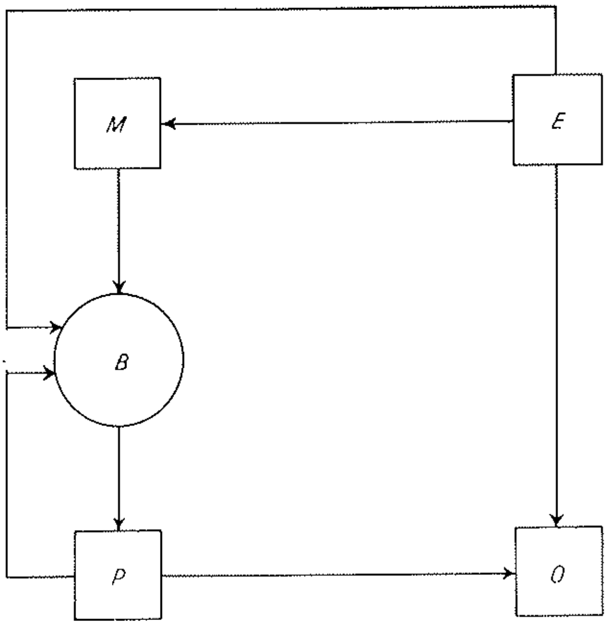
By the various methods I have suggested we may hope to increase the amount of information that the model can absorb. But cases may still arise where we are unable to formulate some of the relationships we need: some processes of real life cannot be represented in the model. All we can do is to observe certain influences and certain effects; we cannot relate the two. We must then turn to the 'black box' technique as adapted to economic and industrial problems by BEER [3].

Reduced to its simplest terms this idea can be formalised as follows. In the real world there is some system, the economy in our case, which produces a certain outcome: in a circuit diagram we could represent this process by an arrow leading from a box marked E to a box marked O. By studying this system we make a model of it and use this model to formulate a plan: this conceptual activity could be represented by an arrow leading from a box marked M to a box marked P. The two sides of life are connected: the model is based on the real world, and so an arrow loads from E to M; and the plan influences the outcome, and so an arrow leads from P to O.

Now, the model consists of certain relationships and these control the plan. If some of these relationships change in the real world but not in the model, then the plan determined by the model will always be wrong and the situation will get out of control. We could avoid this happening if between the model and the plan we could insert a device, the 'black box', which, being controlled by the economy and by the plan, would correct the instructions given by the model, as illustrated in diagram 2 below.

DIAGRAM 2

A Corrective Device for Economic Models



A familiar, if somewhat formal, example can be given to illustrate this scheme. Suppose that the purpose of the model is to tell us how much to produce of a certain commodity and, under free market conditions, what price to charge. Suppose, further, that market conditions are not always free but that at certain times the government imposes price control. If we set up the model in terms of a demand relationship and a supply relationship, we may assume for purposes of argument that:

the quantity demanded, y_1, is a homogeneous linear stochastic function of the price, y_3, and of an exogenous demand variable, x_1; the quantity supplied, y_2, is a homogeneous linear stochastic function of the price, y_3, and of an exogenous supply variable, x_2; under free market conditions the quantity supplied is equal to the quantity demanded; and under price control the actual price is equal to the price fixed by the government, x_3. In its reduced form this system can be written as

$$\text{(II. 1)} \qquad \begin{bmatrix} 1 & 0 & -a_{13} \\ 0 & 1 & -a_{23} \\ \lambda & -\lambda & 1-\lambda \end{bmatrix} \begin{bmatrix} y_1 \\ y_2 \\ y_3 \end{bmatrix} = \begin{bmatrix} b_{11} & 0 & 0 \\ 0 & b_{22} & 0 \\ 0 & 0 & 1-\lambda \end{bmatrix} \begin{bmatrix} x_1 \\ x_2 \\ x_3 \end{bmatrix} + \begin{bmatrix} e_1 \\ e_2 \\ 0 \end{bmatrix}$$

In (II.1), the a's and b's are demand and supply parameters, the e's are disturbances and λ is a number which takes the value 1 under free market conditions and 0 under conditions of price control.

In terms of the diagram, the model consists of the first two equations in (II.1) and the purpose of the black box is to ensure the appropriate value of λ at any time. If the model was established under free market conditions, it will operate initially with $\lambda = 1$. But if market conditions change to a state of price control, the black box must find this out and switch over to $\lambda = 0$. As indicated in the diagram, it does this by continuously comparing the price calculated by the model with the price actually charged in the real world. If they differ, it changes the value of λ. For under free market conditions the actual price and the model price will be the same, but if price control is introduced, the two prices will, in general, diverge unless $\lambda = 1$ is changed to $\lambda = 0$. With this change, the actual price and the model price will be the same as long as price control persists. If, however, price control is abolished, the two prices will, in general, again diverge unless $\lambda = 0$ is changed back to $\lambda = 1$.

Thus the purpose of the black box is to ensure that the model stays in touch with reality. As I have said, the example is a formal one, but there is no necessity in practice to restrict the device marked B to a single, instantaneous decision.

d) Computability. With a large model, computability presents a problem at every stage: in the preliminary processing of data, in estimating the parameters and in reaching solutions for the system of equations. I have already emphasised the importance of making a start and later modifying and extending the initial model; if to this is added the need to reach solutions for a wide range of initial assumptions, it is clear that some special steps will have to be taken if the computing problems are to be kept under control. As explained in greater detail in [7] [44], a number of straightforward techniques are useful for this purpose.

First, the extensive use of matrix algebra, in addition to its notational convenience, has the advantage that in the numerical manipulation of matrices a fundamentally simple set of operations is repeated a fixed number of times. This number depends on the order of the matrices and can be regarded as a parameter of the programme. Accordingly, if the number of categories in some classification has to be increased, it is only necessary to change a parameter and continue with the old programme.

Second, great convenience lies in the computer's facility for handling iterative or relaxation processes. By requiring a sequence of operations to be repeated until some numerical condition is satisfied, the same master programme can be retained when linear relationships, which would themselves admit of direct analytical solution, are replaced by more complex relationships which would not. To do this, a general method of solution must be adopted at the outset.

Third, the programme can be subdivided into independent parts each of which can be contained, data and programme, in the high-speed memory of the computer. This allows each stage to be developed and tested independently so that, as research progresses, it can be replaced by more complicated versions without necessarily upsetting the remaining stages.

Fourth, the programme can be used to compile 'ready reckoners' which make it possible to trace the effect of changes in the initial conditions without the need for further runs. For example, the matrix multiplier $(I\text{-}A)^{-1}$ is a ready reckoner whose elements show the additional output of j which will follow directly or indirectly from a unit increase in the final demand for k. Analogous matrices can be compiled to show the effect of unit changes in the elements of final demand on complementary or competitive imports, on investment requirements, and so on.

Finally, once an initial model has been set up, it is necessary to establish an order of priorities in improving its various parts. For this purpose, sensitivity analysis, that is the systematic exploration of the model to find out what is sensitive to what, would probably be very useful.

2. Policies

Policies express what we want from a system after we have reconciled competing objectives in the light of a model; they provide therefore a set of consistent aims. As I have said, the formulation of objectives is primarily the responsibility of the politician, just as the construction of models is primarily the responsibility of the scientist. But the politician, like the scientist, cannot perform his task unaided and simply hand down a set of objectives to the model-builder without discussion. Three reasons for this have been suggested by Hitch in [17]. One is that there is seldom a clear-cut national objective which can be 'given' in all its details by a central authority; an obvious example of this is the composition of consumers' expenditure. Another reason is that objectives are multiple and conflicting: many of them can be satisfied in several ways which have substantial, differential effects on others. Finally, it is impossible to state the objectives that make up a policy without knowing a great deal about feasibility and cost. In each case the necessary knowledge can only come from the kind of analysis that a model is designed to provide.

An important question at this stage is the range of problems that the politician would like to see integrated. For example, if the model is a national model with no regional dimension, it can say nothing about locative problems. If there is to be a regional aspect to economic policy, then the model must be capable of providing solutions for the different regions. A regional dimension to an economic model is useful because it makes people think why they want particular things to take place in particular localities and enables them to compare the costs of the alternatives.

Thus we see that the politician needs the help of the model-builder in formulating a policy, just as the model-builder needs the help of many other people in building his model. We also see that the objectives from which the policy will be shaped get into a model in various ways.

An objective may be built into the model. This is the case where it is agreed that private consumers should be left to decide how they spend their money. In effect the policy maker says to the model-builder: I shall tell you how much money to assume available for private spending; your job is to find out how this spending will be allocated given the shadow prices that emerge from your model, and what indirect demands it will place on the system. In the course of drawing up the policy, alternative calculations are sure to be needed, but the maximisation of the average consumer's utility forms an integral part of the model.

Or, an objective may form a constraint on acceptable solutions of the model. For example, having fixed the terminal conditions at the end of the transitional period, we may agree to choose a path through that period by maximising consumption subject to the terminal conditions and, of course, to the operating conditions of the period itself. For any given set of conditions we can use the model to work out a path. If the path is unacceptable we must reconsider the conditions that determined it.

Or again, the model-builder may require some numerical data from the policy maker. For example, he will need to know how much is to be spent on public health, education, roads and other items of government consumption or investment in social capital. He will also need to know the input structure of these expenditures. Since all demands on resources compete with each other, it will be possible to settle priorities, including the level of total private consumption, only when the implications of these proposed demands have been worked out.

It will be noticed at this point that there is a need for a number of government sub-models just like the industry sub-models advocated earlier. The operation of a health service or a defence system is a complicated matter which cannot be built into a general model. What is required is a separate sub-model for each of these activities. Given an amount of money to spend and a set of prices, each sub-model would be used to decide how this money should be spent. From this a cost structure would emerge for use in the general model. The general model might show that some revision in the sums to be spent would be necessary. As in the case of industry, there would have to be iteration between the model and the sub-models. Social cost-benefit analysis should prove of great value in this field of research [14] [15].

To sum up, the main features of policy-making for a system are, in my opinion, as follows. First, the different objectives of policy should, as far as possible, be considered together.

The fact that in my examples I did not mention the maintenance of full employment, say, or the relief of poverty is of no significance; I was not trying to list all the aims, either present or future, of economic policy. In practice we shall certainly have to begin with something narrower than we wish. Thus even if we succeed in getting policies for education and training or of regional development into the general picture, it will be some time before we can do the same for urban renewal, to say nothing of mental health or crime. Nevertheless I believe that our aims should ultimately cover the whole socio-economic system.

Second, each objective must be expressed in sufficient detail to enable alternative methods of meeting it to be considered, and the demands which any of these methods places on the system to be worked out. In this way we can begin to compare objectives, and as a result make perhaps a better use of our resources.

Third, the existence of conflicts of interest should be recognised and as far as possible faced. For example, two towns may compete with one another to attract, say, a new power station or motorway. The choice between them can be greatly improved by an analysis of its consequences.

Fourth, policy makers should see that they have adequate information on which to base their decisions and should try to assess costs and benefits wherever possible. For this purpose, existing market prices are extremely useful; but they are insufficient, because many of the things we value are not priced on any market. Examples of this are uncongested roads and quiet surroundings: only recently has a partial attempt been made to put a price on road space by means of parking meters; and the cost of noise to health and productivity has so far received more attention from physiologists and psychologists than from politicians or economists.

Two conclusions emerge from this summing up: there must be close cooperation between the policy maker and the model-builder; and there must be a conscious effort to supplement the market mechanism by calculating costs and prices for activities which lie outside the market economy or which have important aspects which the market does not value.

3. Plans

A plan tells us how to set about achieving our policies given the operating characteristics of the system. It can be identified with administration or control. These words can in turn be identified either with coercion, exemplified by the policeman, or with a means of self-regulation, exemplified by the Watts governor or the thermostat. The opposite of plan is no-plan, or anarchy.

In theory *laissez faire* is not an example of no-plan; it is a perfectly coherent plan for operating an economic system. Its strength lies in the fact that it is self-regulating. This is achieved by placing decisions in a large number of centres each of which is intent on maximising its advantage. An examination of biological and ecological systems suggests that they owe their robustness to similar forms of control: the predator-prey relationship cannot be understood by identifying the predator with a policeman. The fundamental objective to *laissez faire* as a form of planning is that it works with limited values and limited information: the values of the market place and the information provided by current prices and by the prices on a small number of forward markets.

The reaction to *laissez faire* has taken two forms: central planning and government intervention in specific aspects of economic life.

The first reaction has the merit that it places the determination of policies squarely where this belongs, in the class of political decisions. Its shortcomings which derive largely from its political origins, lie in an exaggerated notion of the possibility of concentrating all information at some central point and in believing that all decisions can sensibly be taken at this point. The difficulties that follow this approach are increasingly recognised in centrally planned economies and the natural dialectical process may be expected to produce a better balance between the centre and the periphery.

The second reaction has the merit of recognising the fact that different decisions belong to different centres. Its shortcomings, which derive largely from the partial abandonment of one coherent political philosophy without the acceptance of a new one, lie in an exaggerated notion of the usefulness of modifying some part of a system while ignoring the others: specific acts are justified in terms of the necessity to do something in the area concerned and of the immediate effects intended. The bad consequences of this kind of sporadic planning are becoming every day more obvious, but it is not yet realised that the more one tries to plan a system without studying it as a whole, the less one is likely to succeed.

Thus it is a mistake to associate planning with collectivism and antiplanning with private ownership. We should make better progress if we thought in terms of good planning and bad planning, that is, of functional and unfunctional design. In other words, having agreed on what the system is supposed to do, we should make sure that the operating controls are designed so as to get it done. If we look at planning from this point of view, we are likely to discover that the key problems of economic organisation are the following.

First, the administrative machinery, public and private, which has grown up historically will often exhibit cases where a given function is duplicated and cases where a given function is simply not performed or where the arrangements for performing it are unsatisfactory. An example of this in Britain is the large number of agencies, both public and private, concerned in one way or another with the problems of redundancy and retraining: obviously this part of the administrative machinery must be considerably modified and coordinated if the labour force is to accept mobility and equip itself with modern skills.

Second, even where the machinery exists, we may question the criteria on which it operates, on which, that is, administrative decisions are taken. It used to be generally believed that optimal or near optimal decision-rules were inherent in the *modus operandi* of private enterprise. Experience has shown, however, that this is not always so, even if we agree to consider only private costs and benefits in determining efficiency. Still less is it so if we consider social costs and benefits which the market does not value. Many writers, such as LERNER [22], have tried to formulate effective and mutually consistent rules for economic decision-making. These ideas should be followed up and as far as possible put into practice.

Third, the application of any set of administrative criteria presupposes an adequate flow of information to enable the controls to work properly. We must therefore examine the flow of information in the system and ensure that the necessary information is available in the right place at the right time. We must also keep in mind that if a system is to control itself by virtue of the information that flows in it, this information must not be distorted by interference; if it is, the system will work badly, and if the interference is insistent enough, the system will break down altogether. A good deal of the information available in any economy comes from the movement of relative prices and costs; without such information, derived either from the working of the market or, in the form of shadow-prices, from the working of a model, the decentralisation of decisions is virtually impossible. So we should make the most of price signals as a source of information and should beware of fixing or changing them arbitrarily in the interest of some specific policy, or they will defeat their own purpose. The stop-and-go measures adopted by Britain in recent years for the sake of balancing the balance of payments provide a good illustration of these dangers. The objection to these measures is not that they are unduly authoritarian or unduly *laissez faire,* but simply that they are inappropriate to a system. They do indeed enable the country to muddle through, but at a considerable cost in bad business planning at home and loss of prestige abroad.

Fourth, if we want to do away with sporadic interference, we must examine the robustness of the system in reacting to unforeseen events and try to build stability into it wherever we can. An example of such an administrative arrangement is MEADE's proposal [25] to vary social security contributions automatically with the level of unemployment so as to offset short-term fluctuations in purchasing power. These devices, as PHILLIPS has emphasised recently [28] [29] [30], must be very knowledgeably designed, since a good control mechanism should keep a variable at a chosen level and not allow it to fluctuate.

These problems are encountered in any system and therefore in any economy, whatever its political complexion. A wider recognition of this fact may be expected to bring the operating characteristics of eastern and western economies much closer together over the next generation.

III

A DUAL MODEL OF ECONOMIC GROWTH

1. A NEW DEVELOPMENT

I shall now exemplify the ideas on model-building expressed in the first section of the preceding chapter in terms of the model of the British economy on which I and my colleagues are working. Four progress reports on this work have already appeared in this series [7] [8] [9] [10]. In the first of these we began by setting up a single model for a future state of steady growth and, in our conclusions, hinted at the need for an extension of the model which would help us to plot out a path from the present to the future state. This extension is now beginning to take shape.

In this short chapter, I shall outline the two parts of the model and explain how they interact. In chapter IV, I shall review in greater detail the structure of the first part, our original model, and discuss a number of modifications we are bringing to it. Finally, in chapter V, I shall describe the structure of the second part, on which we have recently started work.

In my exposition I shall try to bring out: the organic character of the model, that is, how its structure and relationships tend to change as knowledge is accumulated; the receptiveness of the model to observations from different sources, including estimates from industrial experts and others, which can improve its realism; and the degree of consistency which we try to impose on the model [41].

2. Steady states and transient states

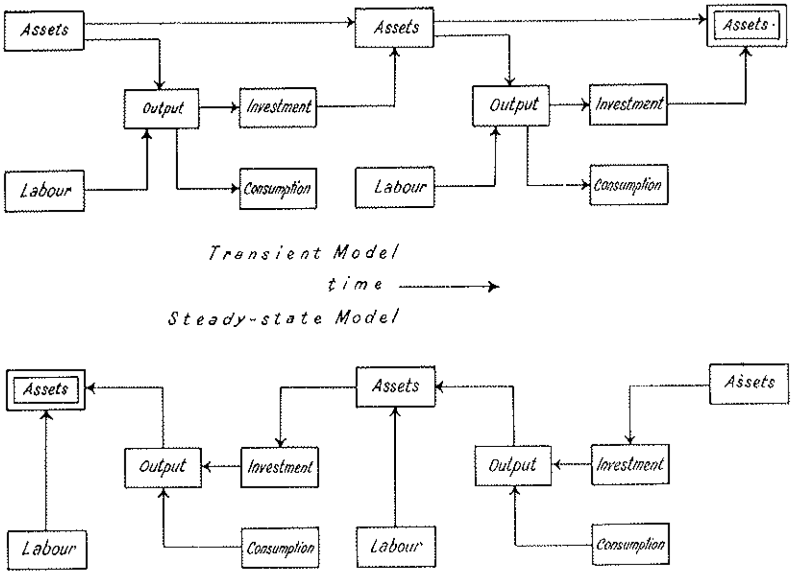
In planning for the future, we should recognise, first, that what we do now affects to some extent what we can do in the future and, second, that what we shall want to do in the future, and what facilities we shall have to do it with, becomes less and less clear as we try to imagine times which are more and more remote. We cannot work forward into the future without setting a target and we cannot derive this target from the remote future because we have no information about it. Accordingly, we have set up our model of growth in two parts, and our solution comes from iterating between them. One part is concerned with the rates at which the outputs of different products might grow after a transitional period ending, say, in 1970; this is the long-run or steady-state model. The other part is concerned with the problem of adapting the economy during the transitional period to meet the initial conditions of the steady state of growth; this is the short-run or transient model.

The structure of these two models and the relationship between them can best be seen by concentrating on essentials and leaving all detail for subsequent treatment. The two structures are shown in diagram 3 below.

Each structure involves a building block for each period, containing five components: assets, labour, output, investment and consumption. We can describe the relationships between these five components in two ways. On the one hand we may say that given amounts of assets and of labour enable us to produce a certain amount of final output which will be absorbed either by investment or by consumption: this is the order that appears in the transient model. On the other hand we may say that investment and consumption add up to final output and that, given the labour available, the production of this output will require certain quantities of assets: this is the order

DIAGRAM 3

A dual model of economic growth



that appears in the steady-state model. In each case the building block is the same; what differs is the nature and direction of the arrows which show the relationships between the components.

In each model the building block is repeated for as many years, or time-periods, as are necessary. In the transient model these time periods stretch from a base year, in our case 1960, to the end of the transitional period, in our case 1969, and culminate in a single component, the stock of assets, as a terminal stock. This stock is shown in the right-hand top corner of the upper half of the diagram, enclosed in a double square. In the steady-state model, the time-periods stretch from the first post-transitional year, in our case 1970, into the indefinite future. In terms of the highly simplified account I am now giving, the purpose of the steady-state model is to determine the minimum initial stock of assets required in the post-transitional period. This initial stock is, of course, the same as the terminal stock of assets of the transitional period and is shown in the left-hand top corner of the lower half of the diagram, also enclosed in a double square to indicate that it is the link between the two models.

Our first step is to use the steady-state model to determine the stock of assets that must exist at the beginning of 1970. This is done on the assumptions that consumption is to take a certain value in 1970 and to grow steadily at a given rate from 1970 onwards. For this rate to be possible, there must take place in 1970 a certain amount of investment which can be thought of as determined by the stock of assets and work in progress needed at the beginning of 1971, which in turn is determined by the assets and work in progress needed at the beginning of 1972, and so on. In fact the whole future of the structure of assets is involved although, if we do not try to push the rate of growth of output up to the technological ceiling of the system, we may expect subsequent requirements to have a rapidly diminishing influence on the assets needed at the beginning of 1970.

Once we have determined these initial assets which, given the labour force and the technology expected to be available in 1970, will depend partly on the level of consumption in 1970 and partly on the rates at which its components are to grow, we can pass to the transient model. We have a known stock of assets in the base year and a required stock at the end of the transitional period. Given the labour force and technology expected to be available throughout the transitional period, we now try to maximise consumption over this period subject to the base-year stock of, and terminal requirements for, assets; and subject also to some restriction on consumption, for example that it should not fall below a certain level or that it should not fall below the level of the preceding year.

The attempt to solve this problem will show whether or not it is solvable. If it is not, we have either put an insufficient restriction on consumption during the transitional period or we have set our sights too high for the post-transitional period. In any case we can make a modification and try again until we obtain a solution.

Once we have a solution we can examine the whole time-path of consumption. Before the base period consumption moved in a known way; we have just calculated its path through the transitional period; and we have arranged that it shall be able to grow at such and such a rate after the transitional period. Taken as a whole this path may be unacceptable because, let us say, it is too uneven: it may show a certain rate of growth up to the transitional period, followed by a slower one which increases enormously at the end of the transitional period to meet the initial conditions of the steady state, followed by the long-run growth rate. An acceptable path should, one might suppose — though this is a political question — be reasonably smooth. One way of smoothing it would be to reduce the level of consumption assumed for the beginning of the steady state; in other words, to make the take-off into steady growth start from a lower level. The purpose of iteration between the two models is to reach an acceptable path.

In the following two chapters I shall show how the basic simplifications adopted in the above account can be removed both in theory and in practice. There is, however, one important point that must be mentioned here since it affects the relationship between the two models. Briefly, it is this. I have so far assumed fixed relationships between the assets available to an industry and its capacity to produce, and between the assets installed in an industry and the investment necessary to instal them. Consequently, the investment of the transitional period could in principle take place at any convenient time without affecting the composition of the stock of assets at the beginning of the steady-state period. This has enabled me to speak of the assets needed at the beginning of 1970 independently of the distribution of investment through the 1960's, and thus to make a sharp distinction between the two models. In fact we know that the relationships I have assumed to be fixed do change with time, and therefore that we cannot calculate exactly the assets needed to provide a given capacity at the beginning of the steady-state period until we know the timing of investment through the transitional period. Here again, as in the case of consumption, the problem can only be solved by iterating between the two models.

3. Why a dual model?

Questions are sometimes raised about the method I have just outlined. Why, it is asked, do we need two models? Could we not work out a path which would carry consumption into some preassigned rate of growth as soon as possible? My answer is that at the present stage of the work it seems easier to divide the problem into two. With a single model it would be impossible to say in advance when the growth rate of the path calculated by the model would approximate to the growth rate preassigned for the steady state. In other words, it would be impossible to say how long the transitional period would last: the model would be a transitional model without any time limit. Consequently, it would be necessary to take a view on preferences, technology, and other variable factors, for an indefinite time-span in the future.

Also, the desire for a single model is sometimes accompanied by a belief that it must always be possible to move smoothly into a faster rate of growth without ever falling below the initial growth rate of the system. We may hope that this is true, but it does not seem sensible to found an analysis on the assumption that it is. In any case, the dual model is intended to explore several possible paths to the steady state and to indicate the good and bad points of each alternative.

However, as illustrated by this series of papers, our approach to model-building is essentially organic. In time, the dual aspect of the model may come to seem less important and the two models may become more closely integrated and even be merged into one. This is not a matter of principle but of practice.

IV

THE STEADY-STATE MODEL

1. Introduction

The structure of this model as it stood eighteen months ago was explained in detail in [7]. A further addition relating to labour skills and to the system of education and training on which they depend, together with some preliminary results for 1970, were set out in [5]. Thanks to the generosity of the Bank of England and other institutions in the City of London in response to our appeal for research funds, we are now starting work on the financing side of the model. This means that we shall study the flow of capital funds between different sectors of the economy, the saving behaviour which adds to these funds and the preferences of different sectors for holding particular portfolios of assets and claims.

In this chapter I shall outline the model following the order of section I of chapter II above. I shall not go over every point described in [3] [7] but shall try to show how our ideas have developed, why we have abandoned certain details that appeared in our original presentation and what our intentions are for the future.

2. The variables

The basic variables of the model are brought together in a social accounting matrix, SAM for short, which at present contains 253 accounts grouped into fifteen classes. The complete matrix for our base year, 1960, is given in detail in [8]; a summary version for 1962, showing the totals within each class, is given in table I below.

For obvious statistical convenience we have keyed in our main totals to the official estimates of national income and expenditure [48]. At certain points, however, we have departed from the treatment followed in these estimates. The most important difference lies in the fact that we define consumers' durables not as consumption goods but as fixed assets. This means that in our treatment these goods are bought on capital account and their consumption is measured by depreciation. Nevertheless, our estimates of total private consumption plus net investment in consumers' durables are equal to the official estimates of consumers' expenditure.

As can be seen from table 1, the accounts in SAM are simply a logical development of the four national accounts [42], and can easily be reduced back to them by appropriate consolidation. The use of fifteen classes of accounts instead of four is largely dictated by the need to reconcile different classifications. This can be illustrated by considering the four classes which appear as the first four rows and columns in table 1 and which, taken together, constitute the national account for production.

Class I relates to commodities, that is to say products or groups of products which are characteristic of British industries. The entries in column 1 show the sources of these commodities: £44,272 million come from British production and £2,458 million, to which must be added £134 million of customs duties, come from abroad in the form of competitive imports. The entries in row 1 show the uses to which these commodities are put: £20,943 million go to industries as intermediate product; £13,249 million go to private consumers; £1,761 million go to public consumers; and so on until, as can be seen from the entry in column 15, £5,128 million go to the rest of the world

TABLE I

A PROVISIONAL ACCOUNTING MATRIX FOR BRITAIN, 1962

(£ million)

Type of account	Description of class	Production accounts				Income and outlay accounts		Capital transactions accounts								All accounts	Total incomings
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
Production accounts	1. Commodities	0	20943	13249	1761	0	0	81	1168	1969	661	1416	81	407	0	5128	46864
	2. Industries	44272	0	0	0	0	0	0	0	0	0	0	0	0	0	0	44272
	3. Consumers' goods & services	0	0	0	0	0	18149	0	0	0	0	0	0	0	0	214	18363
	4. Government purposes	0	0	0	0	0	4856	0	0	0	0	0	0	0	0	0	4856
Income and outlay accounts	5. Indirect taxes and subsidies	134	638	2160	50	0	0	0	32	45	102	118	0	0	0	0	3279
	6. Institutional sectors	0	18855	852	2735	3279	0	0	0	0	0	0	0	0	0	325	26046
Capital transactions accounts	7. Commodities	0	0	0	0	0	0	0	0	0	0	0	0	0	86	0	86
	8. Industries, replacements	0	1200	0	0	0	0	0	0	0	0	0	0	0	0	0	1200
	9. Industries, extensions	0	586	0	0	0	0	0	0	0	0	0	0	0	1428	0	2014
	10. Cons. goods, replacements	0	0	763	0	0	0	0	0	0	0	0	0	0	0	0	763
	11. Cons. goods, extensions	0	0	658	0	0	0	0	0	0	0	0	0	0	876	0	1534
	12. Govt. purposes, replacements	0	0	0	81	0	0	0	0	0	0	0	0	0	0	0	81
	13. Govt. purposes, extensions	0	0	0	74	0	0	0	0	0	0	0	0	0	333	0	407
	14. Institutional sectors	0	—136	0	0	0	2933	0	0	0	0	0	0	0	0	0	2797
All accounts	15. Rest of the world	2458	2186	681	155	0	108	5	0	0	0	0	0	0	74	0	5667
	Total outgoings	46864	44272	18363	4856	3279	26046	86	1200	2014	763	1534	81	407	2797	5667	

as British exports. In our complete matrix, class I contains at present thirty-one commodities.

Class 2 relates to the productive activity of the thirty-one industries which produce these commodities. It can be seen from row 2 that the whole output of these industries, £44,272 million, flows into class 1. The cost of producing this output is shown in column 2, thus: nearly half the total cost, £20,943 million, relates to the intermediate inputs, namely raw materials, semi-finished products and fuels, absorbed in production; £638 million represent indirect taxes (less subsidies), which we charge direct to industries; £18,855 million represent factor incomes, namely wages, profits etc., paid out by the industries; £1,786 million represent depreciation, of which £1,200 million correspond to the value of assets estimated to have been scrapped during the year and the balance, £586 million, is available to finance extensions, that is additions to the stock of assets; - £136 million is not a real entry, but corresponds to the residual error which appears in the official accounts; finally, £2,186 million represent complementary imports, that is to say imports of products which are either not produced in Britain, like crude oil and raw cotton, or produced there in relatively small quantities, like raw wool.

We have adopted this distinction between commodities and industries because there is not a one-to-one correspondence between the two concepts. Most of our basic data make use of the distinction and so it is convenient to follow it in setting out these data, although for input-output analysis we get rid of it and make use of a table which shows the commodities needed to produce commodities, as is explained in detail in [9].

Class 3 relates to consumers' goods and services, or private consumption. These goods and services are classified in SAM under forty headings, corresponding broadly to a shopping list. In this form they lend themselves better to demand analysis, but they still have to be related to products in the industrial classification: again, there is no one-to-one correspondence.

For example, in the consumer classification the category 'clothing' includes the principal products of five industries: textiles, clothing, other manufacturing, transport and distribution. For this reason we carry out our analysis of consumers' demand in two parts: first, an analysis of this demand in terms of consumers' categories; and second, a conversion from these categories into commodity categories and hence into demands on industries.

Class 4 relates to government purposes. Like private consumers, government departments habitually classify their expenditure in a manner different from that used for industrial products. But this time it is not a shopping list that is used but a classification by purposes: health, education, defence and so on. In order to form any view of future demands for these purposes we must work with this classification, but in order to relate these demands to demands on industries we must convert them into a commodity classification.

It is for these reasons that we have four classes of production accounts. As a consequence we have an elaborate accounting system which many people may regard as tedious and technical. So, in principle, it is. But in principle does not mean in practice, and a realistic representation of the actual world inevitably requires such technicalities.

I shall not continue with a list of classes and the reasons for adopting them; all such details can be found in [8]. Instead, I shall mention one part of the existing structure that we hope to change and a number of directions in which we hope to develop.

The change will come not in the structure itself but in the numerical value of the entries. As I have mentioned, we divide capital expenditures between replacements and extensions. In SAM, replacements are calculated by reference to past capital expenditures and to the life-spans of different kinds of asset, which are assumed to be fixed. We know perfectly well, however, that while there may be an average life-span for any asset, the actual life will depend on economic circumstances. A more realistic approach, therefore, would be to assume that assets are scrapped when they cease to earn a return. We should like to substitute this assumption for the one we are making at present, and our current work on production functions points to a way in which this might be done.

The first development I shall mention relates to the distribution of incomes. At present, households are a single sector in SAM, and so have a single income and expenditure account. As a consequence, our model can say nothing about the distribution of incomes by size; it can neither say what changes, if any, in the present distribution might be expected in any particular circumstances nor can it say what consequences would follow if a particular change were to come about. In order to study these problems we should have to subdivide the income and expenditure account of households into separate accounts for different income groups. This could certainly be done; it is entirely a question of time and money. The first problem would involve dividing the *incoming* side of the account by income group and classifying the incomings by industry of origin and type of income; this would be comparatively difficult to do. The second problem would involve dividing the *outgoing* side of the account by income group and detailing the expenditure patterns of the different groups; this would not present much difficulty. As a second step, however, it would be necessary to work out how the average expenditure pattern would change if the distribution of income changed; this could be done by an extension of our present demand functions.

A second development on which, as I have said, we have just started to work relates to problems of financing. In terms of variables, this means in the first place a large extension of our institutional capital accounts, class 14, so as to introduce a flow-of-funds statement into the system. For this purpose we intend to increase considerably the number of sectors in this part of the accounting system so as to distinguish many different kinds of financial intermediary, and to add a new class of accounts relating to different kinds of financial claim. In the second place it means a serious attempt to construct sector balance sheets, an aspect of the social accounts which, in Britain, has only recently been pioneered in the work of MORGAN [26] and in the as yet unpublished study of the national capital on which REVELL has been working at the Department of Applied Economics.

A third development which seems to me desirable is to add a regional dimension to the model. As our model stands at present it is a purely national model and therefore has nothing to say on the regional aspect of any economic activity. But regional problems are obviously important. Again, it is a matter of time and money.

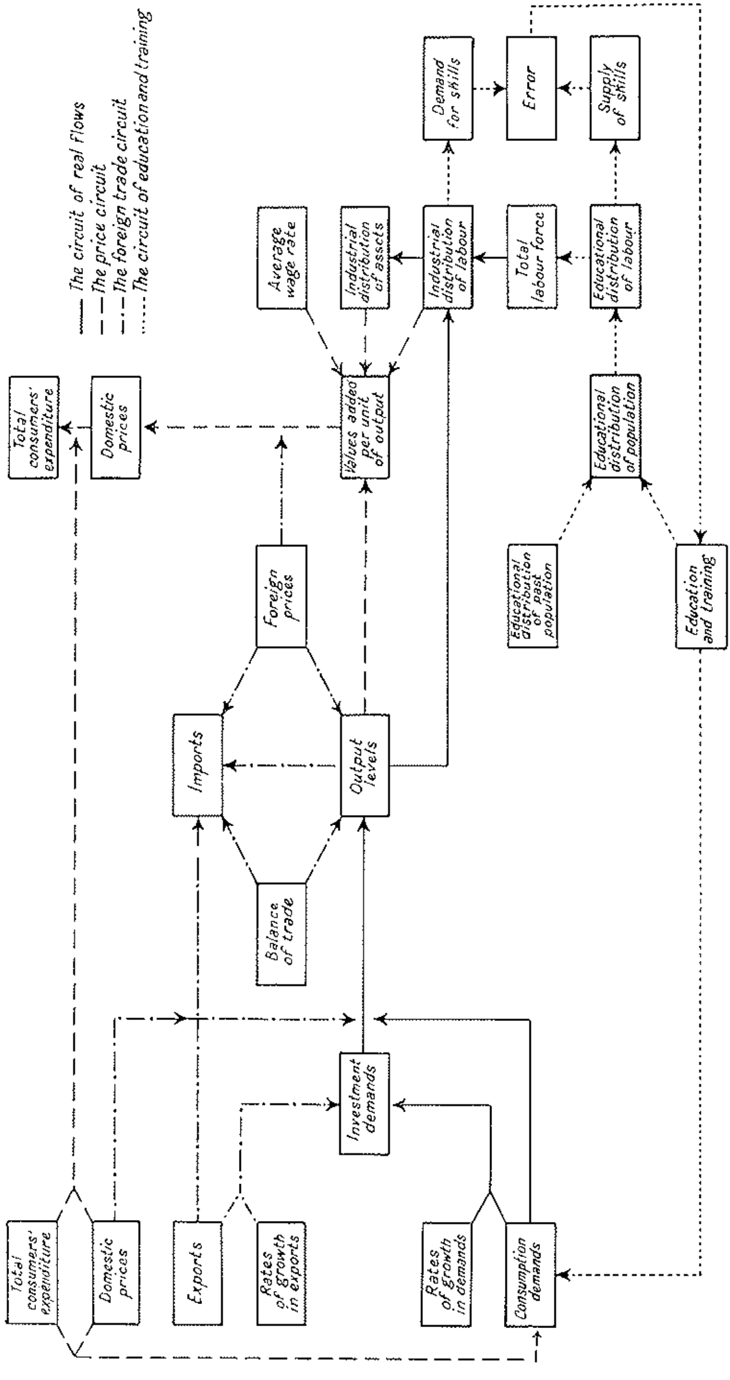
3. THE RELATIONSHIPS

The relationships of the steady-state model, as they stand at present, can be summarised conveniently in a flow diagram which I have already made use of in [5] and which is reproduced here as diagram 4. This consists of four interacting circuits which enable the model to be built up gradually and the complexities of the real world to be introduced bit by bit. In my description I shall mention two further circuits which do not appear in the diagram.

a) *The circuit of real flows.* If we begin at the beginning of this circuit, we find two boxes labelled *consumption demands* and *rates of growth in demands*. Consumption demands relate to both private and public consumption and include, so as not to complicate the diagram unduly, investment in consumer's durables, housing and social capital such as schools, hospitals and roads. For the moment let us assume that we have succeeded in calculating the level of these demands in 1970, have

DIAGRAM 4

The Steady-state Model



converted them into demands for industrial products and for each product have calculated the growth rate which would accompany a given growth rate in consumption as a whole. The first relationship we have to consider connects this information with the *investment demands* of industry. We can ignore replacement demands since, until we improve our production functions, these depend, as I have said, on past investment and on the fixed life-spans assumed for different assets. We need therefore a relationship connecting consumption demands and their rates of growth with industrial extensions.

To obtain this relationship, we first write the basic flow equation for products in the form

$$q = \mathrm{A}q + v + e \qquad \text{(IV. 1)}$$

where q, v and e denote respectively vectors of output, industrial investment and consumption, and where A denotes a current input-output coefficient matrix. Equation (IV. 1) states that output is divided between intermediate demands, $\mathrm{A}q$, and final demands, $(v + e)$; and that final demands are divided between investment demands v, and consumption demands, e.

Second, we write the relationship between investment demands and the growth of output from one year to the next in the form

$$v = \mathrm{K}\Delta q \qquad \text{(IV. 2)}$$

where Δq denotes the excess of next year's output over this year's output and K denotes a capital input-output coefficient matrix.

Finally, we consider the case in which the components of consumption are to grow exponentially. This can be expressed in the form

$$\mathrm{E}e = (I + \hat{r})e \qquad \text{(IV. 3)}$$

where E denotes an operator which advances by one year the variable to which it is applied, so that Ee denotes next year's consumption vector; I denotes the unit matrix; and $\hat{r}$ denotes a diagonal matrix of the growth rates of the components of consumption.

It has been shown in [7] [43] that on these assumptions

(IV. 4) $$v = \sum_{\theta=1}^{\infty} [K(I - A)^{-1}]^{\theta} \hat{r}^{\theta} e$$

This is the relationship we need. It has been shown in [24] that the infinite sum in (IV. 4) will converge provided that the largest element of r does not exceed the smallest latent root of $K(I - A)^{-1}$. This problem is also discussed in [17].

If the components of consumption are assumed to grow linearly rather than exponentially, then (IV. 3) is replaced by

(IV. 5) $$E^{\theta} e = (I + \theta \hat{r})e$$

and (IV. 4) is replaced by

(IV. 6) $$v = K(I - A)^{-1} \hat{r} e$$

which is simply the first term of (IV. 4).

It has been shown in [7] that (IV. 4) is capable of two generalization. First, if technology, as summarised in A and K, is changing in a known way, the expression corresponding to (IV. 4) can be derived. Second, if allowance is made for different investment lags, then (IV. 2) must be rewritten. To do this we need information about the work that must be done on investment goods in the successive years of their construction. If we have this information, then, again, we can rewrite (IV. 4) in an appropriate way. The information is important if the growth of the components of the output vector deviates from linearity. The problem has therefore a particular bearing on the transient model. So far however, we have not done any work on the decomposition of K.

Having thus determined investment demands, the next thing to do is to determine *output levels.* This follows immediately from (IV. 4), since

(IV. 7)
$$\begin{aligned} q &= (\mathrm{I} - \mathrm{A})^{-1} (v + e) \\ &= (\mathrm{I} - \mathrm{A})^{-1} \sum_{\theta=0}^{\infty} \left[\mathrm{K} (\mathrm{I} - \mathrm{A})^{-1}\right]^{\theta} \hat{r}^{\theta} e \end{aligned}$$

The elements of q are gross outputs. Their units might be physical, such as tons of steel or kilowatt-hours of electricity, but in practice, because of the coarseness of our product classification, they are values.

Our problem now is to pass from gross outputs to net outputs, or values added, since these are the output measures which influence most directly the *industrial distribution of labour* and the *industrial distribution of assets.* To handle this problem we shall assume an initial vector, p say, of product prices. How this vector is determined and modified in successive rounds of calculations we shall see when we come to the price circuit. For the moment let us take it as given.

If we use f to denote a vector of primary inputs per unit of output, then, corresponding to (IV. 1), we can write

(IV. 8)
$$p = A'p + f$$

or

(IV. 9)
$$f = (\mathrm{I} - \mathrm{A}')p$$

where A′ is the transpose of A. If we use y to denote the vector of net outputs, then

$$(IV.\ 10) \qquad y = \hat{q}f = \hat{q}\ (I - A')p$$

on premultiplying (IV. 9) by q. The elements of y are now to be related to the labour and capital inputs they require.

In our original exposition [7] (where, incidentally, we did not distinguish between q and y), we proposed to relate outputs to primary inputs by a modified form of the COBB-DOUGLAS function. This modification, proposed by PITCHFORD in [31] and by ARROW and others in [1], is designed to generalise the COBB-DOUGLAS function so that the elasticity of substitution between labour and capital, though still a constant, need no longer be numerically equal to one. This type of function can be written in the form

$$(IV.\ 11) \qquad y_s = a_s\left[(1 - b_s)\, l_s^{-c_s} + b_s k_s^{-c_s}\right]^{-c_s^{-1}}$$

where the suffix s denotes the s'th element of each vector. Thus y_s denotes the net output of industry s, and l_s and k_s denote respectively the inputs of labour and capital into industry s. The three parameters a_s, b_s and c_s can be given an economic connotation: a_s is associated with the efficiency with which labour and capital are used in industry s; b_s is associated with the shares of labour and capital in the net output of industry s; and c_s is associated with the substitution of labour and capital in industry s. The elasticity of substitution between labour and capital in industry s is equal to $(1 + c_s)^{-1}$, and so, as $c_s \to 0$ (IV.11), approaches the simple form of the COBB-DOUGLAS function.

In order to use (IV. 11) or a similar relationship to determine the industrial distribution of the labour force and the associated stocks of assets needed to produce a given vector of net outputs, we must know how large the *total labour force* will be and on what criterion it should be distributed. For the size of the labour force in 1970 we already have official estimates. For the criterion on which it should be distributed we have the familiar condition that the marginal physical products of labour and capital should bear a common ratio to one another in every industry; this is equivalent to saying that we must choose a distribution such that it could not be improved by any redistribution.

On further reflection, however, we have decided not to use relationships of the form of (IV. 11). Two reasons are perhaps sufficient to explain this decision. First, technical progress gets into (IV. 11) by allowing a_s to increase with time. This is not satisfactory because it implies that output will increase over time for given inputs of labour and capital independently of the amount of investment that is being carried out. But if no investment is being carried out the quality of the capital stock cannot improve, and it is hard to see, therefore, how any substantial amount of technical progress could take place. Second, if $c_s < 1$ it is possible to substitute capital for labour indefinitely and thus to produce any amount of output with a given labour force simply by giving it more and more capital to work with. But we know that this is not true. At any given time new plant will embody about as much capital as can profitably be used and it seems doubtful whether much more capital would be used even if capital were a free good. The reason is that, typically, techniques do not exist for using much more capital. Accordingly, there is a limit to the substitution of capital for labour and we could not, even if we wanted to, increase output indefinitely with a given labour force.

Our latest ideas on this subject have been set out in [33] and lead to a production function for industry s at time t which can be written, on the assumption that plants have a fixed life-span, in the form

$$(\text{IV. } 12) \qquad y_{st} = \frac{a^*_s}{b^*_s} l_{st} + \frac{1}{b^*_s} \int_{t-\theta}^{t} v^*_{s\tau} (1 + a^*_s r^*_{s\tau}) \, d\tau$$

In this equation, y_{st} and l_{st} denote respectively the net output of, and labour employed by, industry s at time t; $v^*_{s\tau}$ is the investment undertaken by industry s at time τ expressed in wage units, and $r^*_{s\tau}$ is the initial rate of return on plant which comes into operation in industry s in year τ; and a^*_s and b^*_s are parameters. The integration spans a period of Θ years equal to the fixed life spans of the plants.

We have already seen, when discussing SAM, that the assumption of fixed life-spans is not satisfactory because actual life-spans depend on economic conditions. We can allow for this by integrating over an interval which is different from year to year and which for each year is chosen so that a plant is assumed to be scrapped only when the real wage becomes such that the value added by the plant would be less than the wage bill, even if it were operating at full capacity. This can be expressed by writing (IV. 12) as the functional

$$(\text{IV. } 13) \qquad y_{st} = \frac{a^*_s}{b^*_s} l_{st} + \frac{1}{b^*_s} \int_{R_s(t)} v^*_{s\tau} (1 + a^*_s r^*_{s\tau}) \, d\tau$$

where $R_s(t)$ is the set of values of τ which satisfy the no-scrapping requirement at time t in industry s.

We are only now assembling the data necessary to estimate a^*_s, b^*_s and $R_s(t)$. So in the preliminary estimates for 1970 given in [5] we had to adopt a simpler approach based on [32].

Let us now drop the subscript s. Let Δy denote the increase in capacity net output in a particular industry in a particular year; let x denote the capacity added in the year as a result of new plant coming into operation; and let s denote the capacity subtracted through the scrapping of old plant. Then

(IV. 14) $$\Delta y = x - s$$

Correspondingly, let Δl denote the increase in employment; let n denote the labour added to man the new plants; and let r denote the reduction in employment due to plant retirements. Then

(IV. 15) $$\Delta l = n - r$$

Finally, let p denote the net output price, which is equal to the cost of labour and capital per unit of output, and let w denote the wage rate. Then if we multiply (IV. 14) by p and (IV. 15), by w and subtract, we obtain

(IV. 16) $$\begin{aligned} p\Delta y - w\Delta l &= (px - wn) - (ps - wr) \\ &= (px - wn) \end{aligned}$$

if plant is scrapped when it ceases to earn a return, that is when $ps = wr$.

Now let us define the initial rate of return, r^*, as the gross rate of return to capital embodied in new plant in the first year of its operation. Then, denoting gross investment in new plant by v^*,

(IV. 17) $$\begin{aligned} r^* &= (px - wn)/v^* \\ &= (p\Delta y - w\Delta l)/v^* \end{aligned}$$

from (IV. 16), on the assumption that plant is scrapped when it ceases to earn a return. Equation (IV. 17) can be rewritten either as

(IV. 18) $$p\Delta y = w\Delta l + r^* \; v^*$$

which shows the value of the change in capacity net output, $p\Delta y$, divided into a part due to labour, $w\Delta l$, and a part due to capital, $r^* v^*$; or as

$$\Delta l = (p\Delta y - r^* v^*)/w \qquad \text{(IV. 19)}$$

which shows the change in employment, Δl, as equal to the value of the change in capacity net output, $p\Delta y$, minus the part due to capital, $r^* v^*$, all divided by the wage rate, w. As described in [5], estimates can be made of the terms on the right-hand side of (IV. 19) over a future period, and this enables us to calculate Δl for the different industry groups.

On this basis, the initial stock of assets required in 1970 is determined by capital-output ratios. The allocation of labour to the different industries (1) is such that the labour force expected to be available is fully used and (2) implies that the initial rates of return (or pay-off periods) are the same as, or related to, those observed in an earlier period. The required changes in labour productivity in the different industries emerge from these calculations and average out to the productivity implied by the total increase in output and the total labour available.

The results obtained by this method are based on less information than would be supplied by productiou functions. As a consequence they are provisional and are certainly not demonstrably achievable. They do, however, provide a ground for discussion with individual industries until we have developed our production functions.

b) *The price circuit*. At the moment, prices enter explicity into the model only as determinants of the composition of private consumption, government consumption being treated in a simpler way indicated in section 4 below. Many of the effects of prices appear implicity, however, in the projection of input-output coefficients, which will also be discussed in section 4. There is, of course, nothing final about this arrangement; it is simply a reflection of practical difficulties.

In terms of diagram 4, we see *total consumers' expenditure* and *domestic prices* coming together to determine *consumption demands*. The level of consumers' expenditure is fixed by assumption and left unchanged through each run of the calculations, but relative prices can, initially, only be guessed at. However, as the model simulates the productive process, it builds up a cost structure in each branch of activity and this makes it possible to revise the initial estimates of prices, as follows. Given an *average wage rate* as a unit of account and the corresponding rates of profit implied by the efficient distribution of labour and assets, we work out the future costs of labour and capital per unit of output in each industry and thus obtain *values added per unit of output*. To these we add the cost of intermediate inputs and of indirect taxes and thus obtain the cost, or price, of a unit of output in each industry. If these new *domestic prices* are different from those we had assumed at the start, we must alter the figure for *total consumers' expenditure* to correspond, and repeat the cycle of calculations until our estimates of prices cease to change.

If in the model future consumption is to be sensitive to future prices, two things are needed: 1) a set of price-sensitive demand functions; and 2) a set of future prices. Let us now see how each of these requirements is met. In the following treatment I shall restrict myself to private consumption and I shall find it convenient to set out the analysis on a per head basis; to apply the results to the whole community all that is needed is to multiply them by the population.

As explained in [7], our model of consumers' behaviour is a variant of the linear expenditure system which allows explicitly for changes in tastes and habits. The version we have used so far can be summarised in the following three equations:

(IV. 20)
$$\hat{p}e = b\mu + (I - bi')c\hat{p}$$
$$= \hat{p}c + b(\mu - p'c)$$

(IV. 21)
$$b_0 = b^* + \Theta \ b^{**}$$

and

(IV. 22)
$$c_0 = c^* + \Theta \ c^{**}$$

In (IV, 20), p denotes a vector of commodity prices and $\hat{p}$ denotes a diagonal matrix formed from this vector; e denotes a vector of quantities of the different commodities demanded per head of the population; $\mu = p'e$ denotes total expenditure per head; b and c denote vectors of parameters restricted only by the fact that $i'b = 1$; and, as usual, i and I denote respectively the unit vector and the unit matrix. In (IV. 21) and (IV. 22), Θ denotes a particular year; and the starred b's and c's denote vectors of parameters restricted only by the fact that $i'b^* = 1$ and $i'b^{**} = 0$.

The second row of (IV. 20) makes possible a simple interpretation of the elements of b and c. The elements of c represent the components of the average consumer's basic standard of living and are bought whatever values are taken by μ and p. When these purchases have been paid for, the amount of money left over is $\mu - p'c$, and this is allocated to the different commodities in proportion to the elements of b.

An obvious criticism of (IV. 20) taken in isolation is that the elements of b and c are unlikely to remain constant over time. The simplest means of meeting this criticism is set out in (IV. 21) and (IV. 22), where the elements of b and c are all made linear functions of time.

The results of fitting the model consisting of (IV. 20), (IV. 21) and (IV. 22) to annual data for Britain relating to eight main commodity groups from 1900 to 1960 are given in [45]. On the whole the fit is good, and in two cases where comparisons were easy to make, namely food and clothing, we found close agreement between the total expenditure elasticities derived from the model and the corresponding estimates derived independently from family budgets. A still better fit was obtained with quadratic trends in b and c [39]. But such simple trends are certainly not ideal for projection purposes and we are now working on more complicated varieties.

The model can be fitted simultaneously to time series and budgets [39] and can be generalised to cover adaptive behaviour, that is gradual responses to changes in circumstances [7] [35].

The model is decomposable and so can be applied hierarchically. This means that we can start with an analysis of main groups, then analyse separately the sub-groups of these main groups, then the sub-groups of the sub-groups ,and so on [7]. At each stage we can check on the performance of the model. This is necessary because we may expect that its performance will get worse as we go into greater and greater detail unless we are able to take the special features of individual markets into account. As with other parts of the main model, we are working at present on improving it, and are trying to obtain outside comments on the projections it yields.

This brings me to the second requirement: estimates of future prices. We start with an extrapolation of current price-trends and adjust the base-year expenditure to allow for this change in prices. This adjustment is based on the constant-utility price-index implied by the model [19], which can be written in the form μ^*_1/μ_0 where

(IV. 23) $$\mu^*_1 = p'_1 c_1 + (\mu_0 - p'_0 c_1)\gamma_{b1}$$

Here the suffixes 0 and 1 denote respectively the base year and the projection year; and γ_{b1} denotes a geometric index of price ratios (year 1 in relation to year 0) with the elements of b appropriate to the projection year as weights. The value of μ_1 is then a multiple of μ^*_1, the multiplier depending on the increase in real consumption assumed between the base year and the projection year.

Once we have reached the end of the circuit of real flows, we obtain, as in (IV. 8), a vector, f, of primary inputs, or value added, per unit of output. From this we can recalculate the price vector from the relationship

$$p = (\mathrm{I} - \mathrm{A}')^{-1} f \qquad \text{(IV. 24)}$$

We must now recalculate μ^*_1 and continue until the price vector in the projection year ceases to change. Only a single cycle of calculations is shown in diagram 4.

c) *The foreign trade circuit.* The introduction of foreign trade complicates two of the relationships given so far and adds a new one.

First, in calculating *investment demands* we must allow for *exports* and their expected *rates of growth*. If we denote the export vector by x and the rates of growth of its components by s, then, in place of (IV. 4), we have

$$v = \sum_{\theta=1}^{\infty} [\mathrm{K}(\mathrm{I} - \mathrm{A})^{-1}]^{\theta} (\hat{r}^{\theta} e + \hat{s}^{\theta} x) \qquad \text{(IV. 25)}$$

Second, we must allow for the fact that part of the goods needed by the economy will come from *imports*, and so the demands on domestic production will be affected. We divide imports, the elements of a vector m, into two categories: competitive imports, m_1, namely goods like steel, cars and clothing, which are produced in large quantities in Britain as well as being imported; and complementary imports, m_2, namely goods like crude oil, raw cotton and wool, which are either not produced in Britain at all or are produced there only in relatively small quantities. We assume: that m_1 depends on the amount of money available from export sales and income received from abroad, after allowing for the necessary expenditure on complementary imports and for the sums required for gifts or lending abroad; and that the elements of m_2 are proportional to those of q, the vector of *output levels*. If we denote by β the *balance of trade*, that is the excess of the value of exports, $p'x$, over the value of all imports, $p^*{}_1{}'m_1 + p^*{}_2{}'m_2$, where the p^*'s denote vectors of *foreign prices*, then, following the argument of [7],

$$m = m_1 + m_2$$

$$\text{(IV. 26)} \qquad = [a_1 + \hat{p}^*{}_1{}^{-1} \; a_2 \; (p'x - a'_3 \; \hat{p}^*{}_2 \; q - \beta)] + \hat{a}_3 q$$

where the vector a_1 and a_2 contain the intercepts and slopes in the linear equations assumed to connect the elements of $\hat{p}^*{}_1 \, m_1$ with their total, $p^*{}_1{}'m_1$; and the elements of the vector a_3 are the factors of proportionality relating the elements of m_2 to the corresponding elements of q. The only element of (IV. 26) which is so far unknown is q. It can be worked out from a revision of (IV. 1) which, with the complication of foreign trade, can be expressed as

$$q = \mathrm{A}q + v + e + x - m_1$$

$$\text{(IV. 27)} \qquad = (\mathrm{I} - \mathrm{A} - \hat{p}_1^* a_2 \, a'_3 \, \hat{p}_2^*)^{-1} \, [v + e + (\mathrm{I} - \hat{p}_1^{*-1} \, a_2 p') x + $$

$$+ \hat{p}_1^{*-1} \, a_2 \beta - a_1]$$

Third, we must allow for the fact that *domestic prices* are now affected because some of the inputs into British production come from abroad. Let each element of a vector h denote the share of domestic production in the total supply of a competitive product; then in place of (IV. 24) we have

$$
\text{(IV. 28)} \quad \begin{aligned} p &= A'[\hat{h}p + (I - \hat{h})\,p_1^*] + \hat{a}_3\, p_2^* + f \\ &= (I - A'\hat{h})^{-1}\,[A'(I - \hat{h})\,p_1^* + \hat{a}_3\, p_2^* + f] \end{aligned}
$$

The account just given of the treatment of foreign trade is only one of three variants proposed in [7]. All three, however, are highly simplified because they represent an attempt to do without foreign trading functions, on which foreign trade may be supposed to depend. The establishment of such trading functions is quite beyond our present scope; in time, however, a co-operative venture may make it possible to extend the model in this way. Looking forward to that time, the following sketch may indicate the kind of information needed.

Let us begin by rewriting (IV. 27) to indicate a separate flow equation for complementary imports, and let us now define these imports as goods which cannot be produced domestically. Then we can write

$$
\text{(IV. 29)} \quad \begin{bmatrix} q \\ \cdots \\ m_2 \end{bmatrix} = \begin{bmatrix} A & \vdots & O \\ \cdots & \cdots & \cdots \\ A^* & \vdots & O \end{bmatrix} \begin{bmatrix} q \\ \cdots \\ m_2 \end{bmatrix} + \begin{bmatrix} v + e + x - m_1 \\ \cdots\cdots \\ m_2^* \end{bmatrix} =
$$

$$
= \begin{bmatrix} (I - A)^{-1} & \vdots & O \\ \cdots\cdots & \cdots & \cdots \\ A^*(I - A)^{-1} & \vdots & I \end{bmatrix} \begin{bmatrix} v + e + x - m_1 \\ \cdots\cdots \\ m_2^* \end{bmatrix}
$$

Most of the symbols in (IV. 29) have already been defined. The new ones are: m^*_2, the elements of which are the complementary imports flowing directly into final demand; and A^*, the elements a^*_{jk} of which represent the input of complementary import j into a unit of output of k.

Suppose that we know v, e and m^*_2 but not x or m_1. From (IV. 29) we could calculate a provisional value of the vector $\{q \vdots m_2\}$. Given the price of each product in each trading region, we could try to allocate the demand for each element of $\{q \vdots m_2\}$ over the sources of supply by means of a price-sensitive variant of the linear expenditure system [38]. This means that for the jth element of q we should use

$$\text{(IV. 30)} \qquad q_j = (c_j + C_{jj} p_j) + \hat{p}_j^{-1} b_j [\mu_j - p_j' (c_j + C_{jj} p_j)]$$

where q_j is a vector whose elements are the amounts of commodity j which come from domestic production or from one of the possible foreign sources of supply. Initially μ_j is unknown and must be adjusted until $i'q_j$ is equal to the jth element of q. The matrix C is a symmetric matrix of parameters and is of order equal to the number of sources of supply. A method of estimating the elements of this matrix is suggested in [38].

If we applied (IV. 30) to each commodity in each region we should generate a complete set of imports and exports. These would then have to be added and subtracted to give $\{v + e + x - m_1 \vdots m^*_2\}$ and the whole exercise would have to be carried out again with this vector in place of the provisional $\{v + e \vdots m^*_2\}$. This process would then be continued until it converged.

At this point we can recombine the estimates to give a three-dimensional regional trading matrix: region by region by commodity.

From all this information we can construct a region by region trading matrix, T say. The element t_{rs} say, of T shows the total exports of region r to region s, while the element t_{sr} shows the total exports of region s to region r. For simplicity, I assume that all transactions in T are recorded in terms of a single currency, the conversions to this currency being made by means of a supposedly consistent set of exchange rates.

The balances of trade of the different regions are given by the excesses of their total sales over their total purchases. If these balances are the elements of a vector t^*, say, then

$$t^* = (T - T')i \tag{IV. 31}$$

It is to be expected that $t^* \neq \{0, 0, \ldots, 0\}$. On the assumption that trade balances are determined by buying and selling, let us consider how to adjust the rates of exchange between the currencies so that with the new rates of exchange $t^* = \{0, 0, \ldots, 0\}$.

Consider first one region, r say, and one commodity, j say. From the demand equations we can set up a matrix of order equal to the number of regions, D_{rj} say, the elements of which are the derivatives of the expenditure in region r on commodity j obtained from region s with respect to the price of j in any one of the regions. For commodity j we can do this for each region. Now let us form $D_j \hat{p}_j$ where

$$D_j \hat{p}_j = \sum_{r=1}^{n} D_{rj} \hat{p}_j \tag{IV. 32}$$

where n denotes the number of regions. Having done this for one commodity, we can do it for every other. So let us define $D\hat{p}$ as

$$\begin{aligned} D\hat{p} &= \sum_{j=1}^{m} D_j \hat{p}_j \\ &= \sum_{j=1}^{m} \sum_{r=1}^{n} D_{rj} \hat{p}_j \end{aligned} \tag{IV. 33}$$

where m denotes the number of commodities.

Now let the initial exchange rates be the elements of a vector x_0 and the adjusted exchange rates be the elements of a vector x_1. Then

$$(IV. 34) \qquad (D\hat{p})\,(\hat{x}_0^{-1}\,x_1 - i) = (T' - T)i$$

that is

$$(IV. 35) \qquad x_1 = \hat{x}_0\,[I + (D\hat{p})^{-1}\,(T' - T)]i$$

The elements of $D\hat{p}$ tell us how the total expenditure of region r on the products of region s changes with a uniform change in the prices of each of the regions. The elements of $\hat{x}_0^{-1}\,x_1 - i$ represent the changes in the exchange rates expressed as a proportion of their initial levels. And the elements of $(T' - T)i$ represent the corrections to the initial balances of trade required to give $t^* = \{0, 0, \ldots, 0\}$. Consequently the set of exchange rates which will lead to a redistribution of purchases such that all the trade balances balance is given by (IV. 35). Evidently there is no difficulty if we wish to put $t^* = t^{**}$ in place of $t^* = \{0, 0, \ldots, 0\}$.

For each trading region the initial prices would come out of a model analogous to our main model. Having balanced all the balances of trade by changing the exchange rates, we should alter in each region the relative prices of goods obtained from different sources of supply. When we returned to the demand equations we should obtain a new set of demands. These would balance the balances of trade but, in general, would alter production levels in the different regions and so would alter costs and prices. And so on.

Perhaps enough has been said to show why, in the first instance, we adopted a short cut to the problem of foreign trade.

d) *The circuit of education and training.* The purpose of this circuit is to try to match the future *demand for skills,* as determined by future output levels and techniques in different industries, with the future *supply of skills,* as determined by the demographic characteristics of the population and the system of *education and training.* The idea is a simple one: to get away from the assumption of homogeneity in the labour force which is usually made in empirical work on production functions, and to accord to labour skills an importance equal to that usually accorded to changes in capital equipment. If the productivity of labour is to rise, new techniques must be adopted, but this will not come about easily unless more people are trained in the appropriate skills. Nowadays technological change is particularly rapid, and a failure to realise this may make it impossible to take full advantage of the improvements that science is offering.

The work we have done so far on this subject is contained in [5] [6]. It relates entirely to the demand for skills and suggests that, in Britain at any rate, the supply of the higher and, more particularly, the medium skills is not keeping pace with demand. This indicates the need to increase the number of technicians and craftsmen turned out by the system of education and training. The craftsmen, in particular, must be trained in the newer crafts; there are many crafts that are dying and do not need replacement.

But given that we know how the demand for skills is changing, how are we to change the supply to meet this demand? Our intention here is to set up an activity model of the system of education and training, including the very important element of retraining. By this means we hope to be able to work out the activity levels needed in different branches of the educational system to provide in the future an *educational distribution of the population* which, after allowing for 'wastage', that is for skills not used in the productive process, will ensure an appropriate *educational distribution of labour* and thus remove the discrepancy, or *error*, between supply and demand.

These calculations will produce an educational programme relating to the transitional period and beyond, to replace in our model the rather crude educational projections with which we are making do at present. In its turn, this educational programme will produce a certain pattern of educational expenditure in 1970 and thus affect our estimates of *consumption demands*, since these include, as I have said before, all expenditure on education, both private and public, current and capital.

e) *The financial circuit.* This circuit is not shown on the diagram because our work on it is not advanced enough. Our existing model, being set in an accounting framework, automatically generates enough income, and therefore saving, to finance investment at home and abroad. This does not mean, however, that the community will necessarily want to save the amount implied nor that its various sectors will want to hold the portfolios of new assets and claims that will emerge as a consequence of what is happening on the real side of the economy. For this reason, the relationships which we intend to study first will be concerned with saving behaviour and preferred portfolio patterns in different sectors of the economy. By such means we may hope to discover whether a given programme of growth is likely to lead to financing difficulties and, if so, what steps would be needed to overcome them.

The only work we have published so far on financial relationships is concerned with personal saving [46].

f) *The circuit of research and development.* This circuit is also not shown on the diagram and, indeed, forms no part of our immediate programme of work. It is, however, important in connection with the increases in productivity which emerged from the first circuit. If these increases cannot be met as things stand, perhaps research and development could bring us nearer to meeting them. Although it is difficult to find a relationship between productivity and research and development, it is fairly clear that one exists. Further study should show where the main opportunities lie.

4. The Methods of Estimation

Like the relationships of the model, the methods of estimation go through various stages of development. This is true both of the initial values and of the parameters. The typical procedure is to start either with existing estimates or with the econometric analysis of past observations. The parameters in both the consumption functions and the input-output relationships vary systematically with time, and so the next step is to calculate their values in 1970. These results are then scrutinised with the help of casual empirical knowledge to see how far they seem sensible, and some adjustments are made. Finally they are discussed, whenever possible, with outside experts, and further adjustments are made. So far we have not got much beyond the early stages of this process; in particular, our main attack on the last stage is only just beginning.

Let me now illustrate this sequence by outlining the methods of estimation we have used in the order in which they occur in the calculations.

a) *Exogenous final demand.*

This category can be divided into five components.

1) Private consumers' expenditure. Here we began by applying the model consisting of (IV. 20), (IV. 21) and (IV. 22), and a similar model which makes use of quadratic trends, to eight major groups of expenditure. The parameters were estimated from annual data covering the period 1900-1960 with the exception of the years 1914-1919 and 1940-1947. This was done by means of an iterative, two-stage, least-squares procedure, as follows.

We begin by guessing values of b^* and b^{**} in (IV. 21), which I shall here denote by b^*_0 and b^{**}_0. We then form a vector, y_0 say, of type $n \times 1$, where n denotes the number of groups of expenditure; thus

(IV. 36) $$y_0 \equiv \hat{p}_0 e_0 - (b_0^* + \theta b_0^{**}) \mu_0$$

We also form a matrix, Y_0 say, of order n, where

(IV. 37) $$Y_0 \equiv [I - (b_0^* + \theta b_0^{**}) i'] \hat{p}_0$$

Apart from a random element, y_0 and Y_0 are connected by the relationship

(IV. 38) $$y_0 = [Y_0 \vdots \theta Y_0] \begin{bmatrix} c^* \\ \dots \\ c^{**} \end{bmatrix}$$

If, denoting successive time-periods by 1, 2, ..., t, we now define

(IV. 39) $$y \equiv \{ y_1, y_2, \dots, y_t \}$$

and

(IV. 40) $$Y \equiv \{ Y_1, Y_2, \dots, Y_t \}$$

we can write, apart from a random element,

(IV. 41) $$y = Xg$$

where $X \equiv [Y \vdots \Theta Y]$ and $g \equiv \{c^* \vdots c^{**}\}$. The least-squares estimator, g_1, of g is

(IV. 42) $$g_1 = (X'X)^{-1} X'y$$

Given g_1, we can form a vector, w_0 say, of type $n \times 1$, where

(IV. 43) $$w_0 \equiv \hat{p}_0 [e_0 - (c_1^* + \theta c_1^{**})]$$

and a matrix, W say, of order n, where

(IV. 44) $$W_0 \equiv [\mu_0 - p_0' (c_1^* + \theta c_1^{**})] I$$

Apart from a random element, w_0 and W_0 are connected by the relationship

(IV. 45) $$w_0 = [W_0 \vdots \theta W_0] \begin{bmatrix} b^* \\ \cdots \\ b^{**} \end{bmatrix}$$

If we now define

(IV. 46) $$w \equiv \{w_1, w_2, \ldots, w_t\}$$

and

(IV. 47) $$W \equiv \{W_1, W_2, \ldots, W_t\}$$

we can write, apart from a random element,

(IV. 48) $$w = Zh$$

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where $Z \equiv [W \vdots \Theta W]$ and $h \equiv \{b^* \vdots b^{**}\}$. The least-squares estimator, h_1, of h is

$$h_1 = (Z', Z)^{-1} Z'w \tag{IV.49}$$

Given h_1, we can return to (IV. 38), replace b^*_0 and b^{**}_0 by b^*_1 and b^{**}_1 and calculate the next approximation to g, namely $g_2 \equiv \{c^*_2 \vdots c^{**}_2\}$. If we continue in this way until the estimates cease to change, we shall have reached a solution.

We can see from (IV. 44) than W_0 is a scalar matrix, and so in estimating h the system of equations breaks down into a set of single equations. From (IV. 37) we can see that Y_0 is not a scalar matrix, and so in estimating g we are, in effect, obtaining average values derived from all the equations. From (IV, 20) we can see that $b\mu$ appears as a separate term on the right-hand side, and so, since $p'e \equiv \mu$, it follows from the adding-up theorem that the constraint $i'b = 1$ is automatically satisfied by the more complicated form in (IV. 21).

Further details of this procedure and of the results and projections obtained by it for the eight expenditure groups are given in [39] [45]. We are at present working on combining these estimates with those obtained from family budgets and on analysing the components of each main group by the same procedure.

Until this work is completed we have to use more rough and ready methods. What we do is to estimate the levels of expenditure on the components of each main group by reference to their changing relative importance within the group; for example, within the food group the proportion spent on bread and cereals tends to fall with time, whereas the proportion spent on meat, fruit and vegetables increases at a rate well above the group's average. We also try to allow subjectively for the tempo of substitutions, such as an acceleration of the substitution of electricity and oil for coal as domestic fuels.

Once we have estimated the future values of these components, which correspond to the forty categories into which we divide consumers' expenditure, we then have the further task of converting them into demands for the principal products of our thirty-one industries, demands for complementary imports such as tea, cigars and wine bottled abroad, demands for direct labour such as domestic servants, and payments of certain indirect taxes. Here we have based our calculations largely on our classification converter for 1960.

Eventually, in view of the increasing importance of consumers' durables, for which the process of adaptation is relatively slow, we hope to use the adaptive version of the demand model described in [7] [35].

2) Public consumption. Here we have made use of the trends suggested in the report of the National Economic Development Council [50]. Eventually we hope to get a new view on some of the components of public consumption through the addition of new circuits to the model, such as the circuit relating to education and training.

3) Public expenditure on social capital. These estimates are rough and subjective. For example, investment in educational buildings is assumed to rise in proportion to current expenditure on education; the road-building programme is assumed to treble between 1960 and 1970. Gross investment in dwellings is similarly estimated at the present stage.

4) Exports. Here we have again based ourselves on the work of the National Economic Development Council [50] but have scaled down their annual growth rates for 1961-1966 in making our estimates for 1960-1970.

5) Industrial replacements. These are based on a study of investment statistics and of the life-spans of different types

of asset in different industries [10]. We have not yet reached the stage of replacing this physical determination of scrapping by an economic one.

b) *Endogenous final demand.*

This category can be divided into three components.

1) Industrial extensions. These are based on a calculation of the type of (IV. 25) and thus involve projected input-output matrices, both current and capital, as well as levels and growth rates of exogenous final demand. The capital coefficients were estimated with the help of the material relating to the years 1948-1960 brought together in [10], amended in many cases in the light of outside information.

2) Investments in stocks. These are related to changes in output levels on the basis of experience in the 1950's.

3) Imports. These are based on a calculation of the type of (IV. 26). The first step is to set a figure on the balance of trade to be aimed at in the future. If this is subtracted from export proceeds, the total value of imports is obtained.

Complementary intermediate imports are estimated by fixed coefficients applied to the outputs of the various using industries, in exactly the same way as any other form of intermediate input. These coefficients are based on postwar trends and at present are very rough, because the classification of imports has only been carried out for the three years 1948, 1954 and 1960.

Individual groups of competitive imports, as explained in section 3(*c*) above, are treated as linear functions of the total amount of money available for competitive imports as a whole. These coefficients are roughly assessed on the basis of recent experience.

Considerable improvements could be made in all these estimates simply by a more thorough analysis of existing foreign trade statistics. We hope to undertake this work before long.

c) *Intermediate demand.*

The last element needed in calculating total output is intermediate demand. With the help of a projected current input-output matrix we can then bring all the estimates together as in (IV. 27).

The methods we have used to bring up to date and project the input-output coefficients are described in detail in [9]. Apart from complications arising from changes in classification, the distinction between products and industries and similar practical problems, our procedure can be outlined as follows.

First, we bring the coefficient matrix estimated directly for 1954 [47] up to 1960 by adjusting the rows and columns of the corresponding transaction table to agree with marginal totals of intermediate outputs and inputs. These adjustments are based on the assumption that changes in coefficients are due to three factors: 1) price changes; 2) substitution effects which influence all the elements in a given row; and 3) fabrication effects which influence all the elements in a given column. On the further assumption that the second factor operates *uniformly* along the rows of the matrix and that the third factor operates *uniformly* along its columns, the problem and its solution can be formulated as follows.

Let A_0 denote a known, initial matrix of input-output coefficients, and let A denote the unknown matrix for period 1 which we wish to estimate. Let p denote a price vector whose elements are ratios of prices in period 1 to prices in period 0, and let r and s denote vectors of unknown constants. Then, on the assumptions made,

$$\text{(IV. 50)} \qquad A = \hat{r}\hat{p}A_0\hat{p}^{-1}\hat{s} = \hat{r}A^*\hat{s}$$

say, where $A^* \equiv \hat{p} A_0 \hat{p}^{-1}$. If we have, for period 1, an intermediate output vector, u say, an intermediate input vector, v say, and a vector of total outputs, q say, then

(IV. 51) $$Aq = u$$

and

(IV. 52) $$\hat{q} A' i = v$$

We can make an initial estimate, u_0 say, of u by premultiplying q by A^*. Thus

(IV. 53) $$A^* q = u_0$$

In general $u_0 \neq u$, but we can force an equality by an appropriate multiplication of the rows of A^*. Thus

(IV. 54) $$(\hat{u} \hat{u}_0^{-1} A^*) q = u$$

If we regard the term in brackets as an estimate of A, it can be seen that it satisfies the row conditions but not the column conditions. These can be satisfied by a substitution for A from (IV. 54) into (IV. 52), followed by an appropriate multiplication of the columns of A^*. Thus

(IV. 55) $$\hat{q} A^{*\prime} \hat{u} \hat{u}_0^{-1} i = v_0$$

and

(IV. 56) $$\hat{q} (\hat{v} \hat{v}_0^{-1} A^{*\prime} \hat{u} \hat{u}_0^{-1}) i = v$$

If we regard the term in brackets as a revised estimate of A, we can see that it now satisfies the column conditions, but not, in general, the row conditions. We can, however, repeat the cycle of operations until we obtain convergence with both the

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row and column conditions satisfied. Thus, after $n+1$ iterations, we shall obtain

$$(\hat{u}^{n+1}\hat{u}_0^{-1}\ldots\hat{u}_n^{-1}\,A^*\hat{v}^{n+1}\hat{v}_0^{-1}\ldots\hat{v}_n^{-1})q = u_{n+1} \tag{IV. 57}$$

For a sufficiently large n, the term in brackets in (IV. 57) can be taken as an estimate of A. This we call the RAS method.

The Belgian tests described in [9] show that the RAS method works well, provided that it is possible to estimate the controlling totals u and v, accurately and that certain coefficients, whose determination is different from that expressed by the theory, can be detected and estimated directly. For example, there has been a general tendency for coal input-coefficients to fall as a result of the competition of electricity and oil; but this tendency is at work only where coal is used as a fuel and not where it is a raw material, as in coke ovens. Since the theory is incapable of handling such exceptional cases and since coke ovens use a lot of coal, it is important to estimate the input of coal into coke ovens independently, remove this amount of coal from the transaction table and the controlling totals, and add it back after the remaining items in the matrix have been calculated.

An up to date matrix obtained in this way can only be approximate, and the next thing to do is to discuss the entries with experts in the different industries. In many cases significant improvements can be made in this way, but there will always remain a number of industries for which little or no up to date information can be obtained and which will still have to be handled in a theoretical way.

For purposes of projection, the theory can only offer the simple method of extrapolating the coefficients along exponential trends. Thus if A_1 denotes the estimated coefficient matrix for year 1, and if A_{12} denotes the coefficient matrix for a future year, 2, expressed at the prices of year 1, then

$$A_{12} = \hat{r}^{\theta}\, A_1 \hat{s}^{\theta} \tag{IV. 58}$$

If the intervals between 0 and 1 and between 1 and 2 are the same, then $\Theta = 1$.

The effect of this method of projection will be to change each coefficient in the way that it has been changing in the past. This can be only approximately correct, and so it is of particular importance to get as much direct information from outside as possible. For example, in Britain between 1954 and 1960 the oil component of the input of fuel into electricity generation rose from a very small figure to nearly 20%. A continuation of this trend would make oil the dominant fuel input by 1970. But we know that this particular trend will not continue as in the past because of the kind of fuel used in generators that have been built very recently and are planned for the immediate future. The direct information can come either from current statistics of input-output coefficients which are available in a limited number of cases, for example coal used in coke ovens, or from outside knowledge, as in the case of oil used in the generation of electricity.

We have used such direct information wherever possible and then applied (IV. 58) to the remaining elements of the input-output matrix. We have then discussed the results of this exercise cell by cell and changed the projections subjectively if this seemed desirable. The results of such a survey are illuminating. For example, we were surprised to find that in our projections the coefficient for machine parts and repairs into agriculture rose, indicating a greater use of machinery, while the coefficient for petroleum products fell. We were told that this was not really surprising because of the substitution of the less processed and cheaper diesel oil for petrol and that our results were a reflection of this particular type of fuel economy. We therefore did not change our provisional result in this case, although we did in many others.

d) *Output levels*. If we bring together the foregoing calculations, we can estimate output levels in 1970. In the preliminary calculations described in [5] we found that the increases of output required of our thirty-one industries during the decade of the 1960's varied from zero to over 100%. Apart from the allowance, already mentioned, for a trend projection in the prices of the main groups of private consumption, the estimates were all made at 1960 prices. In other words, only one cycle of calculations was made. We did not calculate shadow prices for 1970 and allow these to modify the composition of consumption or the technical coefficients of production. We plan to do this as soon as our production functions are ready for use.

e) *The industrial distribution of labour.* These calculations were based on (IV. 19). We first estimated the labour force available to our thirty-one industries in 1970, starting from official projections of the total labour force [49]. We allowed for 1.5% of unemployment and for government demands in line with our estimates of government expenditure on different purposes. The result was a 5% increase in labour available over the decade compared with a 50% increase in output.

In applying (IV. 19) we took the increases in output at 1960 prices, already calculated, to represent $p\Delta y$. We based our estimates of the initial rates of return, r^*, on the experience of the period 1948-1960, subject to a minimum rate of return of 5%. We estimated investment in fixed assets over the decade, v^*, as five times the sum of the 1960 level and the 1970 level, the latter being obtained by adding together our estimates of industrial replacements and extensions. We estimated the industrial real-wage rates over the decade, w, by increasing the 1960 wage rates in the different industries by one half of the increase in labour productivity required over the decade in industry as a whole. On this basis, the total labour demanded was 4% in excess of the estimated supply.

This suggests either that more capital would be needed in 1970, or that investment through the 1960's would have to average more than the arithmetic mean of the initial and terminal years, or that initial rates of return in the 1960's would have to be a little higher than in the 1950's. We did not attempt to resolve these questions in [5] but simply increased by a constant proportion the initial rate of return in each industry, thus changing the allocations of labour until, in total, they were equal to the supply.

From these calculations we obtained preliminary estimates of the changes required in the distribution and productivity of labour during the 1960's. We found that virtually the whole of the increase in labour would be absorbed by the distributive and service trades, leaving a stationary labour force to be shared by the rest of industry. The calculated increases in productivity varied widely from industry to industry. On the whole they were higher than in the 1950's but often not sensationally so; in some cases they were lower.

We propose to discuss these estimates with outside experts as opportunity arises, but we shall not go out of our way to do this until our work on production functions is complete.

f) *Changes in the spectrum of skills.* The work we have done so far on this subject is described in [5] [6]. We have divided the labour force into three main functions, managerial, clerical and technical, and have subdivided the last of these into five categories, qualified manpower, technicians, craftsmen, operatives and unskilled. From the census of population of 1951 we can estimate the number of men and women in each of these seven classes for each of our thirty-one industries and for government services. For 1961 the census results are not yet available and so we have made provisional estimates based on statistics of unemployment and vacancies. In making these estimates, we have tried to allow for the economic fluctuations, technological changes and sociological pressures which affect the labour market.

The picture which emerges from unemployment statistics we take to represent a past state of technology, and the picture which emerges from the statistics of vacancies we take to represent a future state. Our estimates for 1961 are obtained by giving each picture equal weight. We found that some skills, namely managers, qualified manpower, technicians and operatives, appear to have moved in line with the demands of technology over the 1950's. We then worked out the weights that would best reproduce the 1951 position for these four skills and found that they were 0.9 for unemployment and 0.1 for vacancies. We then reversed these weights and applied them to all skills to obtain a first approximation to the spectrum of skills for 1970. Obviously this is a very crude method of estimation, and we have only used it as a first device to feel our way into the subject.

We now have for 1970 a provisional vector showing the distribution of labour by skills and, from the calculations described under *e*) above, a provisional vector showing the distribution of labour by industries. If we consider an employment matrix with skills in the rows and industries in the columns, we see that these two vectors provide its marginal totals for 1970. We can now try to fill in this matrix for 1970 by the RAS method, using the corresponding matrix for 1961 as a starting point and the 1970 vectors as controlling totals.

We have carried out this exercise and found that convergence is almost immediate. Since the marginal totals were estimated independently, this result suggests that it may be possible to derive a simple relationships between changes in productivity and the skill distribution of the labour force in each industry. But at present this is a surmise whose verification must await further analysis.

5. The computing sequence

The model has been programmed for EDSAC 2 at the University Mathematical Laboratory and, more recently, for Ferranti's Atlas. The programme is written in stages, each of which can be modified as the need arises without disturbing the others. The present computing sequence, which does not include either the educational or the financial circuit, consists of seven stages. These are summarised in table 2 on p. 75, taken from [44].

So far, only the first five stages, 0 through 4, have been fully programmed. We are now engaged in programming stage 5, which had previously been calculated separately, and this will enable us to introduce shadow prices into our results. Stage 6, the final compilation, has still to be carried out by hand.

Some idea of the size of the programme as it now stands can be gained from the following figures. The numerical inputs, that is the parameters and conditions, needed for a computer-run number between five and six thousand. A run involves about thirty million multiplications: on a desk calculator this is equivalent to some sixty man-years of work; on Atlas it takes twenty-two seconds.

TABLE 2

THE COMPUTING SEQUENCE

Stage	Operations performed
0	Prepare estimates of private and public consumption, expenditure on social capital and foreign demands for exports in 1970, and their trends through that year.
1	Convert the elements in stage 0 into demands for the products of the thirty-one industries, complementary imports, etc., and the rates of change in these demands.
2	Project past input-output matrix and combine with outside information to give 1970 matrix.
3	Combine stages 1 and 2 to give estimates of the outputs of industrial products based on 1) demands from stage 1, 2) capital expenditure to provide for growth in demand and 3) indirect requirements for intermediate products, allowance being made for import requirements and the need to achieve a trade balance.
4	Convert the outputs of stage 3 into production levels in the thirty-one industries.
5	Allow for changes in the productivity of labour and capital, and estimate requirements for them in each of the industries.
6	Combine the foregoing results with certain other information and print out a provisional social accounting matrix for 1970.

V

THE TRANSIENT MODEL

I. Introduction

This model is still at the preparatory stage. The variables are the same as in the steady-state model. The relationships, already described in [40], are outlined below. The estimation problems are similar to those in the steady-state model. The computations are carried out by dynamic programming. We are assembling the material for a trial run with a four industry model and have written a programme in which the calculations proceed year by year. In this way the full model is kept within the capacity of the computer.

Since we have not yet made any calculations with this model, I shall describe here only the relationships we propose to use, limiting myself to the case of a closed economy with constant parameters. In [40] it is shown that these simplifications can be dispensed with.

2. The relationships

The relationships of the model are set out below. Unless otherwise stated, the notation follows that developed in the preceding chapter.

First, in the base year at the outset of the transitional period, the vector, s, of the economy's stock of capital goods is equal to a given value $\bar{s}$ say. That is,

$$s = \bar{s} \qquad \text{(V. 1)}$$

The elements of $\bar{s}$ and s relate to the total stock of capital goods, grouped according to the producing industry, not according to the using industry.

Second, at the end of the transitional period, which runs from $\Theta = 1$ to $\Theta = \tau - 1$, the terminal stock vector, $E_{\tau} s$, must have a certain minimum value, $\bar{\bar{s}}$ say, to make possible the production levels in the first year of the state of steady growth as determined by the long-run model. That is,

(V. 2) $$E^{\tau} s \geqslant \bar{\bar{s}}$$

Third, at all times there must be adequate capacity to make possible the level of production decided on. That is,

(V. 3) $$\begin{aligned} E^{\theta} s &\geq KE^{\theta} q \\ &\geq K (I - A)^{-1} E^{\theta} (e + v) \\ &\geq FE^{\theta} (e + Es - s) \end{aligned}$$

where $F \equiv K (I - A)^{-1}$. Equation (V. 3) can be written in the form

(V. 4) $$FE^{\theta} e \leq (I + F) E^{\theta} s - FE^{\theta+1} s$$

Fourth, at all times the labour force must be large enough to make possible the level of production decided on. If λ denotes the total labour force and f^* denotes the vector of labour requirements per unit of output in the different industries, then

(V. 5) $$E^{\theta} \lambda \geq E^{\theta} f^{*\prime} q$$

Fifth, if the demand functions of (IV. 20) are premultiplied by $F\hat{p}^{-1}$, it follows that

(V. 6) $$FE^{\theta} e = Fg + FhE^{\theta} \mu$$

where $g \equiv (I - hp')c$ and $h \equiv \hat{p}^{-1}b$. By combining (V. 4) and (V. 6), we see that

$$\text{(V. 7)} \qquad FhE^{\theta}\mu \leq (I+F)E^{\theta}s - FE^{\theta+1}s - Fg$$

Sixth, we should probably wish to ensure a minimum level of consumption in each year of the transitional period. This can be expressed as

$$\text{(V. 8)} \qquad E^{\theta}\mu \geq \mu^*$$

where μ^* denotes a preassigned minimum level. Alternatively, we might prefer to ensure that consumption did not fall during the transitional period, in which case μ^* in (V.8) would have to be replaced by $E^{\theta-1}\mu$.

Finally (V. 1), (V. 2), (V. 5), (V. 7) and (V. 8) form constraints which limit any short-run policy; the outstanding question is what policy to pursue. Since the terminal conditions ensure that the long-run policy can be realised at the end of the transitional period, the obvious course is to maximise the utility of consumption during the transitional period subject to the above constraints. If we denote the utility of consumption in year Θ by $E^{\theta}v$, then

$$\text{(V. 9)} \qquad E^{\theta}\upsilon = \Phi\left[\prod_{\xi}(E^{\theta}e_{\xi} - c_{\xi})^{b_{\xi}}\right]$$

where Φ denotes an arbitrary monotonic function and ξ denotes the typical commodity. Thus we should have to maximise

$$\text{(V. 10)} \qquad \sum_{\theta=0}^{\tau-1} E^{\theta}\upsilon = \sum_{\theta=0}^{\tau-1} \Phi\left[\prod_{\xi}(E^{\theta}e_{\xi} - c_{\xi})^{b_{\xi}}\right]$$

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In order to make use of this expression we should have to assign a form to Φ. The simple thing to do is to put $\Phi = \log$; in this case we maximise a weighted sum of the logarithms of the excesses of the consumption of the various commodities in the different years over the quantities that enter into the basic standard of living. This form is only possible if at all times each element in round brackets in (V. 10) is positive. As even simpler practical alternative would be to replace υ by μ, that is to maximise not utility but consumption itself.

As I have stated them, these relationships apply to an economy which is not only closed but stationary, that is has a fixed technology and fixed preferences. The way to remove these limitations is described in [40].

The maximisation of (V. 10) subject to (V. 1), (V. 2), (V. 5), (V. 7) and (V. 8) is, for practical purposes, a problem in dynamic programming. If the terminal stock requirements are set too high there will be no solution: we cannot meet these requirements and have a consumption in excess of μ^* throughout the transitional period. If we insist on μ^* as a minimum, then we must reduce our terminal stock requirements. If we insist on the rate of growth originally intended for the steady state, we must reduce the level of consumption in the first year of the steady state. By experimenting with different initial consumption levels for the steady state, and perhaps also with different forms of the maximand, we may hope to obtain a complete path for consumption that is acceptable as an object of policy.

VI

Conclusions

I shall bring this paper to an end by summarising the philosophy of economic model-building which we are trying to follow in our work.

First, it is useful at the outset to picture a model against the general background of knowledge, objectives and controls which would have to be taken into account if the model were used for practical purposes.

Second, the main use of a model is to help us in exploring possible worlds; in examining, that is to say, not only how a particular economic system works but how it might work, and in relating the performance of the component parts to the needs of the system as a whole.

Third, a model must have coherence and realism. Coherence can be achieved by giving the model a suitable structure; realism is quite a different matter. To achieve realism we must use relationships which recognise the changing character of preferences and technology. Our initial efforts to do this may not give very accurate results, partly because of the difficulty of formulating relationships which are both realistic and manageable and partly because the factual information relating to the past is incomplete. But this does not mean that we should stay content with first approximations.

Fourth, in choosing the relationships that are needed, such as consumption functions, production functions and so on, accepted economic theory provides a valuable guide. In formulating these relationships, however, economic theory is much less useful because most of its elaborated parts are based on a narrow, static view of the world. Within this narrow view, great generality is usually sought and frequently achieved. In practice it is often necessary to adopt a formulation at the same time less general than theorists would like within the range of phenomena they consider, and less restricted in its range. For example, the consumption functions we are using cannot handle complementary or inferior groups of commodities but they can handle systematic changes in preferences and also adaptive behaviour.

Fifth, is assembling observations on which to base our projections, we can achieve a great deal by the careful processing of existing data, but however thoroughly we do this our knowledge remains incomplete. We must therefore try to gain the cooperation of outside experts in practical walks of life, who may be less well placed than we are to attempt a synoptic view of the whole economic system, but whose specific knowledge is always greater than ours. The realism of our projections can only increase as we succeed in getting more reliable information into the model.

Sixth, to be a useful tool for policy-making a model must enable us to make not just one but many alternative projections based on different assumptions. When it has reached this stage the model becomes in its turn a source of information in the light of which a policy can be drawn up. If this policy is carried out, the model can then be used to make predictions.

Seventh, no policy can be carried out without a control system which keeps the plan in touch with events. This control system consists of a mixture of centralised and decentralised administrative machinery, including all private arrangements for the management of businesses, cooperatives, labour unions and so on. The model can be used to review the control system and show how administrative methods might be modified so as to improve the economy's inherent tendency to stability. I have not tried to formulate this range of problems because as yet we have done very little work on them.

Eighth, it is impossible to plan unless one knows what one is planning for. With this in mind, we have divided our model into two parts, a long-run model and a short-run model, and a solution comes from iterating between the two. The purpose of the long-run model is to help in choosing a direction and a rate of steady expansion to be achieved from 1970 onwards; and the purpose of the short-run model is to choose a path to this goal. If the model were used for policy-making, it would of course be necessary to take a new view of long-run objectives at regular intervals.

Ninth, an economic model should cover all aspects of economic activity. As it stands at present, our model is restricted mainly to the real side of the national economy, but we are now beginning to extend it into the financial sphere and hope eventnally to develop it in other directions as well.

Finally, we have started with a purely economic model, conceived on traditional lines, because there we felt on reasonably firm ground. But we believe that the main motive forces of economic growth are to be found in human abilities and attitudes: organising capacity, acceptance of education and training, response to innovation, labour mobility, and so on. However, we could hardly have begun with these indefinite and on the whole badly documented areas of interest; and in any case it would have been useless to do so until we could embody them in a coherent picture of the socio-economic system. So, naturally enough, we decided to build out from the familiar and to use our working experience as the starting point for our work.

Pythagoras' remark, ἀρχή δε τοι ἥμισυ παντος, 'the beginning is half the whole', applies to social and economic model-building as it does to all complicated human endeavours. The only practical course is to build a prototype and then improve on it in the light of experience and needs. It is fairly safe to say that the modern aeroplane would never have come into being if no aeroplanes had been flown until tbey were as good as they are today.

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DISCUSSION

MAHALANOBIS

The introduction to what I may perhaps call the general philosophy of development of Professor STONE, I feel, has been an extremely appropriate, and a most significant contribution to the Study Week. At a later stage I should like to make some observations on the logical and philosophical aspects of Professor STONE's paper rather than on the details of the model.

FRISCH

If Prof. MAHALANOBIS feels that he is at this moment prepared to put questions or discuss the philosophical aspects, I think by all means that he should be allowed to do so now.

MAHALANOBIS

My first point is that any domain of interest in which observations have an operational meaning, that is, have a meaning in the sense of science, may be considered to be a part of (what I shall call) the world of reality. This world of reality must be distinguished from the world of mathematics, of logic, of purely formal relations. My next point is that in this world of reality we are always concerned with a bounded part of it, that is, limited in both space and time. So, we have to start with some bounded part of the world of reality which I may call « R » (in a diagram on the blackboard). This bounded part of reality consists of certain elements or elementary units which also, in my view, are finite in number. It is possible to make observation(*s*), one or more, on each or some of these elementary units. In this way we get a system of observations which I may call « O » (shown in the blackboard) which has to be distinguished from « R », the reality, but is based iu some sense on this reality. With any given set of observations « O », it is possible to make a model, which I may call « M ». Now, there are two gaps, which are important: the system of observations « O » may not be valid, that is, may not be adequately representative of « R » the world of reality. Secondly, a model « M » (or models) may not be relevant or adequate. At this stage, objectives or aims must be taken into consideration to examine whether any particular model « M » is relevant or adequate. For the Study Week the objective or aim is economic development; I am including the question of fluctuations within development.

Model making, in the sense of Professor STONE, would belong to the world of reality, and not to the world of abstraction — if I have understood him right, and must be, therefore, limited iu time.

Now, I shall pass on to a second point. In this Study Week we are interested in models in the world of reality. But models of what types? Comiug from the uuder-developed areas, I suggest, ultimately (not necessarily during this Study Week) the aim of economic development must be that of the world as a whole.

There is the system of the world as a whole, bounded, of course, in space and also in time, that is, time up to what is of interest to us, 1970 or 1980 or 2000 but perhaps not 5,000 A.D. If I take the system of the world as a whole, we have three broad areas: briefly: WEST and EAST (in the political sense) and DEVELOPING countries. I think we have to think of the world system as acting and interacting between these three areas, rather than as a single unit. I shall use the symbols W, E and D (on the blackboard) without trying to define where is the exact geographical boundary of the

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West, or of the East, or of the Developing countries; these areas are defined in fact in terms of political tensions and interrelations which also are shifting over time. This band (points out something on blackboard) is the relation between East and West, and there is the Developing countries; so, the world system would consist of W, E and D and their interrelations. To some extent, Professor LEONTIEF has taken this into account in his paper in calculating what should go from either W or E to D.

We thus have a kind of world system of W, E, and D. Then we have to consider also the national system of each particular country. In model making, there will continually be the need to think of the different national systems as constituent elements of the world system. However, there would be need of delimiting the sphere of interest according to the purpose in view. We may have to consider a particular national system or a particular sector or groups of sectors within a national system. A nation or a country is, however, not the only basis of delimiting the sphere of interest. There will be many other ways of delimiting the system, for example, in external trade we have bilateral or multilateral systems. It seems to me that it would be a good step forward to have an agreed terminology in this whole business of model making. The two concepts « macro » and « micro » require to be arranged in some kind of a logical system of hierarchy involving dimensions in space, in terms of economic sectors, and also of time. It would be then possible to view the micro-models at different levels fitting into a macro-model at a higher level, and then these macro-models fitting into wider macro-models at still higher levels until the global world system is reached.

Going back to my first point, if I have understood Professor STONE correctly, there would be need of taking into consideration experience or results of experiments to assess the validity of the model « M » in respect of the system of observations « O », and finally in relation to the world of reality « R ». When we speak of experience it would, I think, be of great help to keep in mind that a model developed in one country, on the basis of experience of that country, it may be verified or experimented upon, or modified in some other country. It is possible to make models which are realistic in the sense of Professor STONE but which cannot be verified in the U.S.A., because there is not much of central planning there, but which are capable of being verified in the U.S.S.R., or which may find great opportunities for experimentation in a centrally planned country like U.S.S.R.

To sum up, I suggest that it would be useful to go into the question of terminology at one stage, to identify the hierarchical levels of multi-dimensional analysis. Professor FRISCH may be able to think of suitable words. An appropriate terminology would be of help in fitting « macro » or « micro » models at different levels into a global frame of the world as a whole, and to delimit different domains of interest (also of a multi-dimensional character, and covering, as necessary, both social and political aspects) to suit the purpose in view.

WOLD

It is most appropriate that Prof. MAHALANOBIS has given us some broad indications towards a general frame of reference for our Study Week. Prof. STONE's excellent paper gave a very good start in this direction, and Prof. MAHALANOBIS has now emphasized the regional view of the globe. I would like to take up another basic aspect, also with a view to clarify fundamental ideas, namely the general philosophy of model building. The need for a broad consensus about views and terminology is here so much the more pressing, as the literature of professional philosophy leaves much to be desired in this respect. The situation is somewhat paradoxical, for in almost any treatise of philosophy of science we find a laudable introductory statement to the effect that the area of the treatise is the procedures and methods in current use in the many branches of science, the purpose of philosophy of science being to study and assess the general principles of scientific inference; on the other hand it is clear that although the approach of model building by and large is all pervading

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in the entire domain of natural and human sciences, the treatises of philosophy of science have very little if anything to say about the general principles of model building.

It is not my intention to give here a general review of the principles of model building. My main point is to emphasize a fundamental distinction between three aspiration levels in scientific model building. The first is *fact finding*, the second is *understanding*, the third is *prediction*. Correspondingly, we may talk about

(1) descriptive models;

(2) explanatory models;

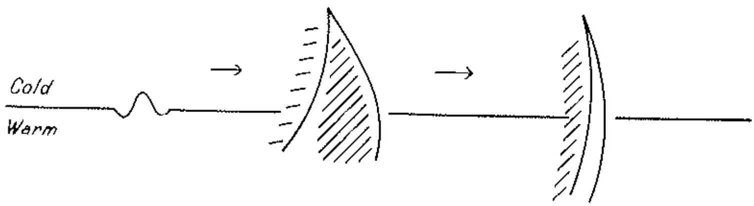
(3) forecasting models.

Fact-finding answers questions of the type: « What has happened? » The answers are given by observed actual facts, and the observations are organized in a more or less systematic fashion into a descriptive model. Explanatory models answer questions of the type « Why has it happened? » Speaking broadly, the answers involve an element of causal inference, and the model makes a joint construct of theoretical analysis and empirical observation. Forecasting models answer questions of the type « What will happen? » Explanatory models are based on past experience, and when such a model is used for prediction it constitutes a forecasting model.

The three types of models thus represent stages of scientific evolution towards higher aspiration levels. If we examine the various branches of scientific endeavour from this point of view, we find that there are great differences. Some sciences stride along happily at the level of fact-finding and description. Others have succeeded to assess a stable causal pattern in the observed facts, thereby ascending to the level of reliable explanatory models. Still more advanced are the sciences where the explanatory models are reliable enough to provide valid forecasts of future events.

Economics and econometrics, I believe, have reached this third stage rather recently. The situation is far from uniform. Certain areas of economics are well covered by reliable explanatory and forecasting models, whereas other areas are more or less white spots on the map of successful model building. This point of view goes to the core of our proceedings: The very theme of our Study Week makes a challenge to econometrics to give evidence how far our science has reached towards the goal of valid explanatory and forecasting models in two areas of paramount importance: economic growth and business cycles.

I should like to develop this point a bit more. Let us take very briefly one example: meteorology. Fact-finding includes of course everything about the weather. The theory of meteorology has developed gradually, but it is rather recently that explanatory models were built that could be exploited for reliable forecasts. The breakthrough was the thermodynamic theory of cyclones, founded by the Bergen school around 1918, with J. Bjerknes and T. Bergeron as leading names.



With reference to the graph, the theory says that cyclones tend to develop on the border between cold polar air and warm equatorial air; on the northern hemisphere the cyclones move eastwards, go toward culmination in some few days, and then dissolve in an occlusion of the warm front and the cold front. On the basis of the thermodynamic theory the forecasting techniques have gradually improved and become more reliable. Here is a clearcut case where we have seen an important development in model building in the short span of some twenty years, during which all three levels of model building have come into the picture. Two points of general scope will be noted. One is the feed-back on fact-finding of an explanatory model: the construction of the model makes use of empirical observations, and once the model has been constructed it directs the fact-finding to new types of observations that are of relevance for the improvement of the model. The second point is that the problems of short-range and long-range forecasting often are distinctly different, and require quite different types of explanatory models. This is so in meteorology: The thermodynamic theory provides fairly realistic forecasts over the next 24 or 48 hours; the cyclones usually die out in a matter of days, so if we want weather prediction over weeks instead of days we must find another theoretical basis for the forecasting model.

At the other extreme, I should like to refer to the science of history, and then place economics and econometrics as intermediate between meteorology and history. History, of course, is in a sense very near to economics, and reference is here made to the brilliant review in Prof. STONE's paper of the interdependence between the economic developments on the one hand, and political objectives and policy making on the other. Here we are on the border between economics and history, and if we adopt Prof. MAHALANOBIS' global point of view this is perhaps more history and politics than economics. Anyway, it is interesting to examine this area with regard to the three types (1)-(3) of models. The prevalent view among professional historians is that forecasting lies outside the realm of their science. In a way, of course, this is true, or rather a truism; history looks back into the past to explore « wie es eigentlich gewesen sei, » to quote a famous dictum. The point I wish to make is, however, that history does not show an entirely blank record when it comes to forecasting. For one thing, the study of history is one of the lines of university education that by long tradition qualify for a career in the diplomatic corps and other strata of civil service where judgements and counsels about future developments are important elements of the professional activity. Of special interest in this respect is the great work of ARNOLD TOYNBEE. It is a qnestion what is most admirable, his encyclopedian approach toward historical fact-finding, or his paving of new ways in historical analysis by the use of general explanatory categories, typical examples being the notions of *enormity* and *mimesis*. But the main inspiration of ARNOLD TOYNBEE is, I believe, the area of forecasting: he is deeply worried about the present trends of the world, and he tries to tell what will happen if we do not understand things better and take action accordingly. Well, to repeat, my point is not to defend or criticize historical views; what I want to emphasize is that the distinction between explanation and forecasting is of crucial relevance in history as in any other science.

How about economics? Here the distinction between fact-finding and forecasting is well recognized, and in some areas of economics the explanatory models are reliable enough to be successfully exploited for purposes of forecasting. Areas where the techniques of forecasting have been successful include demand analysis, production analysis, cost analysis, and on the whole the key areas of microeconomics. In macroeconomics, on the other hand, we are still in the beginnings in the transition from explanatory models to forecasting models, and, to return to the starting point, the theme of our Study Week is a challenge to assess the present status about valid model building in the areas of economic growth and business cycle analysis.

From the general point of view of scientific method, model building is a pluralistic endeavour. Science can be regarded as a collection of models: meteorological models, economic models, historical models, etc. And in each science there is a plurality of models: short range models, long range models; we have purely theoretical models, integrated theoretical-empirical models, all sorts of models. Science is not a unified system which embraces everything; it is a myriad of different approaches, partly consistent with each other, partly inconsistent, and each approach takes the form of a model. The pluralism is often to distinct advantage. A case in point is the Eastern and Western approaches towards economic growth. It is a question whether it would at all be meaningful to construct an integrated model to cover both the Eastern and Western types of long range economic models; the differences would be too deepgoing. It would be more instructive to have one clearcut model of each type, and compare the models to explore where and what the relevant differences are.

My own report to the Study Week, when it comes up, will deal with three specific aspects of model building. I had prepared some notes about the general principles of model building as an introduction to the oral presentation of my report. Then when I heard Prof. MAHALANOBIS' plea for a general consensus about basic terminology and notions, I felt it was in line with his plea to cut out these notes and present them right now, as a contribution towards unifying our basic notions about model building.

LEONTIEF

I would like to comment on the relationship between theoretical construction and factual observation within the framework of Professor STONE's presentation. The relationship is essentially an iterative one.

The theoretical model in its first experimental version is formulated so as to be capable of being implemented with the available, or at least, obtainable factual information. At the same time it should serve as a guide in determining the most promising direction of further empirical inquiry.

The bulk of official statistics is still being gathered to serve various administrative needs or to supply up-to-date information to general users. However, an ever larger volume of data is being collected with the specific purpose of being fitted into explicitly formulated analytical models.

In the past the theorist was too often inclined to leave the responsibility for the factual implimentation of his conceptual schemes to the statistician. Now the model builders have come to realize that they have to take active part in the preparation and planning — if not the actual performance — of the data gathering task. As a result of this they find themselves forced to replace the essentially symbolic concept of traditional economic theory by concrete operational concepts referring to directly observable and measurable facts with which a business man, an engineer or a practical economic planner has to reckon in the couse of his every day activities.

In some instances we find ourselves seeking collaboration with other disciplines. In describing and analysing processes of production, an economic model builder must be prepared to speak the language of a production manager and of an engineer; in dealing with the structure and behavior of households, he must be prepared to speak the language and use the concepts of a demographer and a social-psychologist.

The time when the economist or the econometrician could limit his efforts to construction of mathematical models — or devising more sophisticated methods of statistical inference — is passing fast. He has to take increasingly active part, not only in the actual use of these models and application of these methods, but in the organization and the direction of the fact gathering activities without which all theory and methodology will continue to be no more than intellectual exercise.

MAHALANOBIS

There is one point in Professor WOLD's observation about which I am not clear. When he said « forecasting » did he include « targets »? It is possible to take suitable action to attain certain targets which are considered desirable; this is the system, for example, in central planning in U.S.S.R. It is not forecasting based essentially on historical experience or time-series analysis but setting up certain targets which it is desired to achieve over a certain period of time. Would Professor WOLD include setting up of such targets within forecasting?

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WOLD

Prof. MAHALANOBIS' question is important, and my answer is in line with Prof. STONE's views on the integration between forecasting and objectives. The notion of targets is closely related to the notion of objectives. Reference is also made to the distinction between instruments and targets emphasized in the works of Prof. TINBERGEN.

In my brief comments on descriptive, explanatory and forecasting models I was limiting myself to the purely scientific aspects of model building. In the analysis of policy problems we come to a fourth type of model, *policy models*. A frequent type of policy model specifies different alternatives of policy in terms of objectives and targets to be achieved on the one hand, and instruments to be used on the other. Speaking generally, a nonscientific element enters in the policy model when it comes to the actual choice between the different alternatives of policy.

MAHALANOBIS

Another supplementary question: in meteorology, some experiments have been made to find out whether rain can be influenced by artificial means. That would bring in — I take it — Professor STONE's point of experimentation. Would such experimentation be included in forecasting? I am trying to get Professor WOLD's views clear in my mind.

WOLD

This question points to the very important distinction between two ways of gathering knowledge: by controlled experiments and by nonexperimental observations. These two ways of getting access to knowledge cut through all levels of scientific model building — descriptive, explanatory and forecasting models. My brief outline was not intended to be complete, and I choose the three areas meteorology, history and economics so as to be mainly of a nonexperimental kind. It is very true that experiments also belong in the picture, but that was just to simplify.

To comment more in detail on the question about meteorology, I would say that while most of the fundamental law of physics can be demonstrated by controlled experiments, the empirical evidence on the thermodynamic theory of cyclones is essentially nonexperimental — you do not experiment with cyclones on a world global scale, and the same holds true of many other meteorological applications of physics. At the same time there are elements of genuine controlled experiments in meteorology — rainmaking devices are a case in point. The experiments of rainmaking provide material for model building at the descriptive and explanatory as well as the forecasting level, showing that experimental and nonexperimental evidence combine and merge at all levels of model building, including the level of forecasting.

FRISCH

I want to congratulate both Prof. STONE and Prof. MAHALANOBIS for their presentations. I am very much in agreement about what they say about the philosophy of models and as a matter of fact they have relieved me of a good portion of my own task when I am going to speak in a few days from now. Of course I also agree completely to the three points which Prof. WOLD put before us — the three levels of aspiration so to speak, but — and this is something which I took down while he was talking — there is a fourth point which must absolutely be added (and here of course I am talking in the same vein as Prof. MAHALANOBIS) namely the analysis of the decision on *action*. What are we going to do? That of course depends on what we would *like* to see happen sometime in the future and as a matter of fact this will really be a very essential point in my presentation. This is the essence of the distinction between a forecasting model and a decision model. I understand that Pro-

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fessor WOLD agrees with me on this. We really have three types of models: the explanatory models, the forecasting models and the decision models. My primary concern is with decision models.

HAAVELMO

I have a brief comment on Prof. STONE's paper at the point where he mentioned the role of estimation. If I understood him right he suggested that estimation of certain parameters in a model might be made, so to speak, in advance, by the scientists, but the question is whether this can be done in a way that is independent of the *use* of the model. I think the answer is NO because the method of estimation will depend essentially on what kind of statements you want to make. Ordinarily you will not have point estimates, you will have confidence intervals, and depending on the « gambling attitude » of the policy makers, the kind of confidence intervals which you will want for your estimates will depend on the purpose of the model. As an illustration, consider two Ministers of Finance, one being afraid of losing his job if there is unemployment, the other being afraid of losing it because of inflation: the estimates, in the way of confidence intervals for the multiplier, which you would give to these two gentlemen, could be quite different.

KOOPMANS

I have two different comments, one to Prof. STONE's paper: I think the model on which he and his associates are working is highly interesting because of its scope and the detail of disaggregation and because it gives an opportunity to test the return on disaggregation; one can experiment with the model, re-introduce aggregation and in that way determine how much of the information that is obtained by the extreme disaggregation is lost owing to going over to higher degrees of aggregation; I would like to ask whether Mr. STONE has made plans to exploit this possibility and urge him to do so, because it might be that the return on disaggregation after a certain point ceases to be worthwhile.

My second comment has to do with the proposal towards agreeing on a terminology in which to discuss model construction. I have misgivings about that proposal. Scientific terminology generally comes about by a process very much like consumer's choice in the market place; some authors use new terms that are picked up; the same authors may have used other terms that were not taken up. There is a social process of accumulation of terminology that is an extremely valuable screening process, not only of terms, but also of the ideas associated with these terms. If we make a concerted effort to set up a terminology, we may find ourselves unwittingly and nnwillingly setting up ideas rather than terms, because some terms are most suitable as vehicles for certain ideas. I would therefore put in a plea for allowing the natural selection of terminology to take its course also in this area.

MAHALANOBIS

I should apologise for having failed to convey what I meant by terminology. Sometimes we speak of « macro » and « micro » models. My point was that it would be useful to have, for example, an agreed terminology of using the two concepts « macro » and « micro » in relation to the particular domain of interest of which we are making a model. Now if we simply wait for general agreement, such agreement may never be reached. I may give a specific instance; in social accounts, in national income, it became necessary continually to have a standard terminology; for example, what should be meant by « net national income ». I suggested that model making has now reached a stage where an agreed terminology, in a purely scientific sense, would be extremely useful for purposes of description. I think it is possible up to a point to have a terminology without injecting any views in it. ... I may give a specific example. In my own country, we have distinguished between « projection » and « target ». « Projection » is what we get from historical experience,

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that is, what is likely to happen as long as the same historical regime continues. On the other hand, we can use the word « target » in the sense of purposive selection of something which is desired to be achieved. The two words « target » and « projection », before a clear distinction was made, led to much confusion in India. This is the type of terminology I had in mind; I do not know whether Professor KOOPMANS will have any objection to that.

KOOPMANS

I think, as Prof. MAHALANOBIS notes, that « macro » and « micro » economics, is a fine example of the natural selection of terms that I have referred to. This terminology, originally coined, I believe, by Prof. FRISCH in the '30s, caught on and is now part of the language. I think the example of « East » and « West » as a terminology in model construction of the world economy is an example of the thing I am afraid of; the content of « East » is changing before our eyes; the content of « West » will as well be changing sooner or later. If we set up standard terms for parts of the world which we wish to distinguish, terms which in some way get a stamp of approval from a terminology creating committee, we may actually inhibit thought and analysis.

PASINETTI

I should like to make simply a short remark. There is a distinction which I thought emerged quite clearly both from Prof. FRISCH's paper and from Prof. STONE's paper, but which has been left into the shadow in the discussion so far. The distinction is between those relations which in an economic system are so fundamental as to be independent of the institutional set-up that society has chosen to adopt and those relations which are specific to a particular institutional set-up. For example Prof. LEONTIEF's input-output inter-industry system is independent of institutions; it is a kind of analysis which can be carried out for a socialist country as well as for a capitalist country. On the other hand, for example, the processes through which prices are actually reached are specific to particular institutional set-ups: they are different according to whether we consider a socialist economy, a capitalist economy, or any mixed type of economy. It seems to me that this distinction is preliminary to, and should be put behind the classification which have been put on the blackboard by Prof. WOLD completed by Prof. FRISCH.

ALLAIS

I would simply like to make a few remarks on the points which have been raised during the discussion.

In the first place, the thinkers of earlier times do not appear to have been as preoccupied with method as in our day. I think that this difference results from the unequal development of our science. Three centuries ago, at a time when mathematics and physics were still only stuttering, DESCARTES felt it necessary to study method. If today we economists speak of method, it is simply because our science has not yet reached a sufficiently high degree of attainment.

A second remark: I am struck by the fact that several speakers have paid great attention to the question of aims in the construction of models. Personally, I feel that models ought to be neutral, and constructed independently of objectives. I would willingly associate myself with Professor FRISCH's suggestion that explanatory, forecasting, and decisional models should be distinguished. It is not possible to bring science back to a single type of model. Personally I consider the most significant type of model to be the explanatory one, and I believe that to subordinate the construction of explanatory models to the pursuit of certain objectives is potentially very dangerous.

A third point is that Professor KOOPMANS has just said that there is a social process of selection, which he described as being very useful, and fruitfull both in ideas and in terminology. On the contrary, it is my view that this is a process which must be regarded with the greatest suspicion. Past experience with theories in physics shows that what was fashionable at one time or in a certain period was subsequently completely invalidated. We must therefore be very critical about the choices made by current opinion, and personally I would abstain from taking up any position on the question of whether the choices which are presently those of the majority are or are not useful or fruitful. I only think that it is necessary simply to be very careful.

A fourth point is that there are three essential stages in the construction of a model. The first, the working out of the basic assuptions follows a process of successive approximations similar to that mentioned by Professor STONE. The second stage is purely logical, a deductive stage in which essentially mathematical techniques are applied. Finally, the third and probably the most important stage is the confrontation of the theory and the facts. Further, I believe that these are Professor STONE's ideas, but it seems to me that his conception is more neutral, and it appeals to me personally more than some of the points of view which have been expressed. I do not think that we ought to have a priori or normative ideas about what we are going to do. It is first necessary to understand well and to describe properly, and the basic aim of those who construct models ought to be above all to give as complete information as possible, and I would add, as neutral as possible. It is also desirable that there exist decisional models in parallel with this, but these are completely separate fields. There is the description of facts, their explanation, and action, which implies normative choices, but each of these stages must be kept separated carefully. Thank you.

SCHNEIDER

What does it mean: « Models must be neutral »?

ALLAIS

This is the same difference as that existing in pure and in applied science. The model is neutral if it constructed by a scientist who has a non-emotional attitude to it, has no ideological aims, and who does not include views on what ought to be at the base of his construction. The model is neutral if it aims to describe and explain the facts. It is no longer neutral once it is intended to act on the facts, and this is the reason for my complete acceptance of Prof. FRISCH's terminology, distinguishing between explanatory, forecasting and decisional models. Decision must not be confused with explanation, and neither decision nor explanation should be confused with forecasting; these are different things. During the discussion I was struck by the fact that several speakers appeared to have been mainly guided by decisional considerations when constructing their models.

LEONTIEF

Our discussion seems to reveal the existence of two different, not to say, opposite approaches to the choice and formulation of economic models. Some favor a fully integrated approach associated usually with the concept of a decision model. The so-called objective function, the description of all structural and behavioral relationships and even the methods of statistical estimation of relevant parameters are viewed in this case as interdependent parts of a single tightly integrated system. A change in any one of them requires, accordingly, modification of all the others. In another alternative approach, the description of basic structural relationships, the estimation of relevant parameters and the choice of relevant objective functions are approached as three interrelated, but separable problems. A change or a modification in the solution of one of them does not necessarily require in this case a corresponding modification of the solution of the two others.

Although the first approach appears to be more elegant, the second might prove to be more useful since it permits replacement or modification of its individual elements without requiring each time a complete reconstruction of the entire system.

FRISCH

Time will not permit now to go into a detailed discussion of what is an explanatory model, what is a forecasting model and what is a decision model, but there is one point which I must mention regarding a decision model, to avoid misunderstanding. When we start out consciously to build a decision model we begin by an attitude which in a sense is *neutral*. At this stage we are not at all deciding anything about what the outcome ought to be. We are to begin with perfectly neutral in this respect. We as scientific analysts are perfectly neutral even in another respect, namely regarding what the preference function ought to be. This is a matter to be decided by the politician not by the scientific analyst. True enough the scientific expert will have to help the politician with respect to the form, i.e. the language in which the function is expressed, but certainly the scientist has not to decide the substance matter expressed hy the preference function, therefore, in these very fundamental aspects we are still neutral when we decide to work consciously on the construction of decision models. There is no political attitude involved.

DORFMAN

Some time ago, in discussing this methodological problem, Professor LEONTIEF called to mind a very fruitful standpoint which I always associate with the name of KARL POPPER, though he probably did not originate it. That is: scientific advance is an iterative process. The models we build, the terms we use to express them, the objectives for which we build the models, the measurements which they dictate and on the basis of which we verify them — all these are in constant interplay and as we learn from each step we revise all the others.

I therefore subscribe to Professor KOOPMANS' thesis that we should not try to freeze the meanings of the terms used in econometrics at this stage. I think it will be our experience here this week that as we discuss econometrics we shall learn, and as we learn the words we use will come to have different and somewhat sharper meanings than they have now. I fear that we can bog ourselves down by attempting to clarify points of terminology. Science is groping, and as we grope we shall wish to change the meanings of the technical words we use.

MAHALANOBIS

I should like briefly to make two points. First I shall remove the apprehensions of Dr. KOOPMANS; I agree that to try to define what is East of West would be absolutely futile; but we may say a « macro-national » or « micro-national » or use such neutral terms. I agree also with the points made by Professor LEONTIEF. It seems to me that what is a decision model or what is a forecasting model also involve the question of terminology.

However, terminology is in one sense a procedural poiut. I do feel that we should have some discussions regarding the objectives of model making. Whether the question of terminology is pursued or not I have no strong views; but even the present discussion indicates that some clarification of terminology would he useful. It is purely my own personal ignorance. I should like to understand clearly what is meant by such term as « objective », « neutrality », « forecasting model » or « decision model » and such things. What are the different types of models in relation to different spheres of interest? I am not suggesting that we should be interested in only one type.

I agree generally with the observations made by Professor FRISCH that even if we have decision in view by politicians or others, the role of the scientist is to keep a « neutral » mind in advising how those « objectives » may be attained. I do not see that « neutrality » is in any way destroyed by keeping certain « objectives » in mind. Ou that point I am in complete agreement.

As far as I have understood Professor DORFMAN, I am in broad agreement with him, but I think that our discussion today shows the need for some clarification of ideas which can only be done through words and therefore through the use of agreed meanings of words.

ALLAIS

I must stress that there is a very good reason for my not completely agreeing with Prof. FRISCH. There is a very great difference between three types of model: explanatory models, forecasting models and decisional models. This difference is the following. As far as the explanatory model is concerned there is a judge, you can verify your explanation when you observe the facts. The same is true when you have a forecasting model. Your forecast may be wrong, but you can see if it is right or wrong. But when you develop a decisional model, what is the criterion of truth? I cannot see that there is one. You may think « I am neutral ». You may think this is always true, but you can be wrong; and if I think « you are not neutral » and if you think « I am neutral », and if we are in disagreement, who is to decide? You see here a very great difference between the first two types of model and the third. For the first two models there is a judge: nature. Nature can answer « You are right » or « You are wrong », but with a decisional model nobody, nothing can answer.

MAHALANOBIS

Neutrality is a word which we should not press too far because even in gathering facts it is necessary to have a conceptual framework; in one sense, you can collect only such facts as you are looking for. Also, all observed facts would be affected by errors of observation arising from personal bias. One has to go even farther; ultimately, according to the HEISENBERG principle of uncertainty it is not even possible to make an observation without disturbing the system itself. We should not press the question of neutrality in an extreme way, but take a broad view that for certain purposes certain models would be more appropriate than others.

The point stressed by Professor LEONTIEF is extremely important — the validity and also the precision and the effectiveness of a model to serve certain purposes which may be purely explanatory or may be decision making or may be forecasting or may be of other types. I think this raises questions of substance which we should discuss; and we should, among ourselves at least, provisionally agree on what kind of words we should use.

WOLD

I am not sure whether there is really any disagreement between Prof. ALLAIS and Prof. FRISCH, but their debate does confirm Prof. MAHALANOBIS' view that it is a good thing to clarify our terminology — not necessarily to establish it for the indefinite future, but at least for the purpose of our discussion during the Study Week. The question has been posed: What is a decision model, and how does it differ from a forecasting model? It is my undestanding that decision model and policy model are essentially the same notion. If so, the question can be answered along the lines of a famous argument by the distinguished Swedish economist GUNNAR MYRDAL. Political actions, includiug actions of economic policy, are based on value judgements, and it is typical that the judgements are radically different for members of different political parties. The analysis of economic policy and other decision systems takes the form of a policy model, where the value judgements underlying alternative political actions are included as specified hypotheses, hypotheses which in themselves are politically neutral. In this way the policy model becomes, in principle, an instrument for strictly scientific analysis of alternative lines of political action.

STONE

I should like to thank you all for the very interesting discussion on my paper and I am sure that you will forgive me if I do not attempt to reply individually but merely try to summarise the position. In 1927 the biologist J.B.S. HALDANE published a book called *Possible Worlds* in which he tried to examine how far certain things were possible from the standpoint of physical, chemical or biological laws. Could one imagine, for example, an elephant ten times larger than the elephants that are actually known? And, if not, what was it that prevented us from imagining that such animals could exist? My type of model building, and, I think, many other people's model building too, is concerned with doing exactly the same sort of thing for an economic system: we should like to obtain certain results; could we imagine that they could come about? Now one may ask the question: how is one to decide such things? And this seems to me to get to the heart of the difference between predictive models and models which aim at projections based on hypotheses about changes that might be introduced into the world. If we assume that the world will continue to work as it has in the past, and if we do not like some of the consequences of this, we may ask: could we change things for the better? The answer is not necessarily « yes » but, equally, it is not necessarily « no », because the way in which the world in fact works is not by definition the best possible way. It seems to me that in trying to decide questions of this kind one must follow the principle that upholders of *laissez-faire* claim that *laissez-faire* maintains, namely the maximising principle: that wherever you see economic action taking place it is always successfully directed to maximising something that one wants to have maximised. Now if this were actually the case there would be very little need to build models because we should already have a perfect system which could not be improved. It can only be in the belief that this is not actually the case that we have interested ourselves in the rather exacting and energetic pursuit of economic model building.

Another question that came up and which I think is worth discussing can be summarised briefly under the term « disaggregation ». It certainly is not my belief that the way to get better models and a more realistic representation of the economy is to start with a general model of the kind I described and then simply make it larger and larger and larger. The reason why this is not a good idea is, firstly, that it is impossible to get, in any group, sufficient information to build so large a model and, secondly, that it is quite unnecessary to do so. If you are interested in the operation of the chemical industry, even if you are only interested in a certain group of activities brought together in a single firm, you can set np a model, as large or larger than mine, to analyse the operations of that industry or firm. But it will be quite impossible for any single group of scientists to integrate into their own work models of this degree of detail. But why should they; if the industries concerned are willing to do the job themselves and are obviously very much better at doing it? And this goes not only for the industrial side of the model. The same can be said about many of the activities of government. It is too much to expect that a group of economists who have the problem of a general model on their hands will also be able to build models of, say, the health service, the educational system and the defence system. The rigbt way, I think, to get this sort of disaggregation is to have a series of sub-models (which, however, must be linked to the main one), to decentralize the building of these models and to put this work in the hands of people with the necessary specialized knowledge.

Finally, I shonld like to say something about another recurring theme in this morning's discussion: the question of iteration. It seems to me that this is the fundamental principle on which all learning and all model-building is based. One has to start somewhere. One knows perfectly well that one's prototype model will not be a very perfect tool, but the really important thing is that one should set it up, see how the parts of the system interact, and check how the relationships of the model work out in practice. We are bound to start with relatively simple ideas, we are bound to start with relatively inaccurate facts. We can try to find out how far the facts need to be more accurate than those we have at present and we can try to find out what relationships in the model are really important for the main purposes for which we want to use it. But if we build models, put them in journals and allow them to be forgotten, if we do not keep them up to date and if nobody builds on them, I do not think that we shall ever get satisfactory applied economics.

TOWARD A VERDICT ON MACROECONOMIC SIMULTANEOUS EQUATIONS

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INTRODUCTORY

It is now 25 years since JAN TINBERGEN launched his pioneering macroeconomic models for the NETHERLANDS (1937), US (1939) and UK (1951). His approach marks a bold raise of aspirations levels, in economic thinking as well as in the statistical techniques of fact-finding and analysis, and above all in the systematic coordination of theoretical and empirical approaches. If we think of the allembracing sectors of economic life and the huge complex of static or dynamic interrelations that the model builder sets out to master, the dimensions of his task bring to mind LAPLACE's fathom who could forecast in detail the future course of events in the entire universe by solving an enormous system of differential equations. It adds to the greatness of TINBERGEN's work that large masses of economic-statistical data had gradually accumulated, that the young discipline of econometrics was in the starting holes for vigorous advances, and that it did not tarry long until his approach, now known as causal chain systems, was followed by other keen innovations, in the first line input-output analysis (LEONTIEF, 1941) and interdependent systems (HAAVELMO, 1943). The literature of the area is large and rapidly expanding. The high goals of macroeconomic model building, that much is now clear, are an important incentive for the development of new techniques in quantitative economic analysis. Thus in the short period of 25 years it has become manifest that macroeconomic model building is a mighty challenge to econometric method. At the same time it has emerged as a significant factor in the long range scientific evolution.

It is appropriate to evoke the long range perspective if we call to account in the challenge, asking for the results thus far obtained in macroeconomic model building. Immediate benefits of an indirect nature have often and rightly been emphasized, including the build-up of knowledge about macroeconomic facts, the improvement of statistical data-collection, and the training of new cadres of econometricians in theoretical and applied work, all of which are tangible boons that cannot be overrated. As to direct results in the form of forecasts and other types of operational inference from the models, the horizon is eagerly watched for signals of progress. On this score the outlook is more undecided, and leadiug authorities have voiced scepticism and disappointment about the reported achievements. A symptomatic feature is the recent symposium in Econometrica (1) with the motto « Simultaneous equation systems: Any verdict yet? » But here patience is in order, for, to repeat, 25 years is a short time to master the tremendous tasks at issue.

The Study Week, a most felicitous initiative of the Pontifical Academy of Sciences, provides a forum for unprejudiced appraisal of ends and means, aspirations and actual achievements in macroeconomic model building. The timing of the Study Week could not have been better. There is plenty of progress in the many avenues of scientific research and development that run together in a fullfledged macroeconomic model. On

(1) *Econometrica,* October 1960, Refs. 6 and 7. Cf. also Refs. 8 and 9.

the empirical side, the assessment of quarterly data sets new standards of fact finding, standards that are highly influenced if not called forth by the subject-matter considerations embodied in the model building. For another thing, causal chains and interdependent systems have turned out to be genuine innovations in dynamic model building, not only in econometrics but in the wide realm of nonexperimental model construction. At the level of the stochastic foundations of model building the innovation has posed new types of problem; the debate of the 1950's on the rationale of simultaneous equation systems shows that it has taken a long time to sort out and come to grips with these key issues. As to the design and actual construction of macroeconomic models, several projects are in the picture, of different types, of different size, and at different stages of completion. It is an important task to subject the accumulated material to systematic scrutiny and comparative studies, with a view to assess the theoretical and practical value of the achievements, and, what is perhaps even more important in the present stage of development, to obtain guidance for further work.

The present report focusses on three specific aspects of macroeconomic models:

1) The three types of simultaneous equation systems known as vector regression (VR-), causal chain (CC-) and interdependent (ID-) systems are presented as three levels of generality in the mathematical design of dynamic models. The step from CC- to ID-systems is crucial from the point of view of the operative use of (*a*) the behavioural relations, and (*b*) the reduced form. Thus in CC-systems all relations (*a*)-(*b*) can be specified as *eo ipso* predictors, that is, as conditional expectations subject to random disturbance, whereas ID-systems allow such specification for relations (*b*) but in general not for relations (*a*). This parting of the ways is the stochastic aspect of the much-discussed feature that the behavioural relations of CC- but in general not of ID-systems are designed for a

causal interpretation in the sense of stimulus-response relationships.

2) The parting of the ways between CC- and ID-systems is examined from the point of view of the transition from disturbance-free to stochastic relationships in the formal design of simultaneous equation systems. While the dualism at issue does not exist in disturbance-free systems, it arises in the stochastic specification of the models because the rules for operating with disturbance-free relations sometimes but not always extend to *eo ipso* predictors. Thus in CC-systems the transformation to the reduced form is a matter of iterated substitutions such that *eo ipso* predictors are carried into *eo ipso* predictors, whereas in ID-systems this transformation in general is an operation that does not preserve *eo ipso* predictors. The dualism can be removed by a respecification of the behavioural relations of ID-systems, namely, by letting those explanatory variables that are current endogenous variables be replaced by their conditional expectations as given by the reduced form. In the resulting systems, calling BEID- (bi-expectational ID-) systems, both the behavioural relations and the reduced form make *eo ipso* predictors, and accordingly allow a corresponding causal interpretation.

3) In experimental situations the empirical testing of a model is essentially a matter of replications under controlled conditions. In nonexperimental situations, and in particular in macroeconomic model building, no routine techniques are available for testing the model with regard to its practical value. Here *predictive tests* are of key importance, that is, follow-up studies in which the forecasts obtained from the model are confronted with the actual course of the time series that are subject to analysis and forecasting. Thus far it is only for very few macroeconomic models that such follow-up studies have been reported, and a plea is made for the systematic use of predictive tests. Reference is made to the *Janus quotient*, a predictive test which is designed as a criterion whether the

time series under analysis have the same structure of interrelations in the observation range and the prediction range. As applied to CC- and ID-systems the Janus quotient can be adapted in various ways so as to take into account the distinction between genuine forecasts from the model and ancillary forecasts of the exogenous variables. A specific use of the Janus quotient is as a danger signal against *overfitting*, that is, the pitfall of estimation techniques where illusively small residuals are obtained because the parameters to be estimated are numerous relative to the available observations. This aspect of the Janus quotient comes to the fore when ID- and BEID-systems are estimated by the two-stage method of least squares. The predetermined variables then pile up with unknown coefficients in each relation of the reduced form; hence unless special precautions are taken the reduced form will be overfitted, with risk to run into the pitfall to conclude that the current endogenous variables coincide with their conditional expectations, and that the ID-system coincides with the corresponding BEID-system.

The topic of the report is an area of active interest to my research seminar. I am greatly indebted to Professor ENDERS A. ROBINSON, with whom I collaborate at present in conducting the seminar, and to Messrs. A. GADD, E. LYTTKENS, S. MARTINELLE and G. STOJKOVIC for allowing me to incorporate unpublished research results in the report, as indicated by specific references to their work.

1. THREE LEVELS OF GENERALIZATION IN DYNAMIC MODEL BUILDING (2)

We shall consider three types of approach which (*a*) have the form of simultaneous equation systems; (*b*) are designed to

(2) The reader is assumed to have some orientation in the literature of dynamic econometric models. For textbook treatments of CC- and ID-system, see Refs. 10 and 11. The present exposition leans heavily on Refs. 12 and 13.

include forecasting as a main purpose in their applied use; (*c*) make use of the device called *forecasting by the chain principle*. We say that a forecast that spans k periods $t+1$, $t+2$, ..., $t+k$ is obtained by the chain principle if the calculations proceed step by step so that when the forecast for the period $t+i$ has been obtained ($i=1, 2, \ldots, k-1$) it is used as an actual observation when calculating the forecast for the period $t+i+1$.

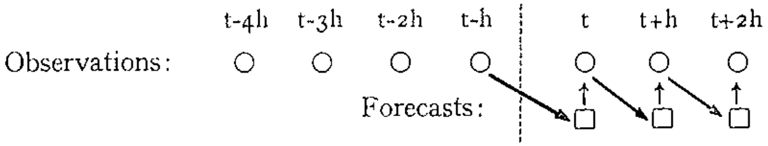

FIG. 1 — *Forecasting by the chain principle*

All through sections 1-2 we shall be concerned with purely theoretical aspects of the three types of model. For the specification of the models we shall make use of the notion of *eo ipso* predictor, that is: If a random variable y allows the representation

$$(1) \qquad y = f(x) + \upsilon$$

where $f(x)$ is the conditional expectation of y for given x,

$$(2) \qquad \mathrm{E}(y|x) = f(x)$$

then $f(x)$ is called an *eo ipso* predictor of y. The notion extends to vector variables y, x, υ. As to the empirical treatment of *eo ipso* predictors, we note that they can under very general conditions be consistently estimated by least squares regression (3).

(3) For a detailed proof, see Ref. 14.

It will suffice for our purpose to consider relationships and *eo ipso* predictors that are linear. The following terms and notations will be used throughout:

$$(3) \qquad y_t = (y_{1t}, \ y_{2t}, \ \ldots, \ y_{nt})$$

is the vector of current endogenous variables;

$$(4) \qquad x_t = (x_{1t}, \ x_{2t}, \ \ldots, \ x_{mt})$$

is the vector of exogenous variables; and

$$(5) \qquad z_t = (y_{t-1}, \ y_{t-2}, \ \ldots; \ x_t, \ x_{t-1}, \ \ldots)$$

is the vector of predetermined variables.

I.I. *Vector regression (VR-) systems.*

The general formula for linear vector regression is given by

$$(6) \qquad y_{it} = \mathrm{L}_i(y_{t-1}, \ y_{t-2}, \ \ldots; \ x_t, \ x_{t-1}, \ \ldots) + \upsilon_{it}$$

where $i = 1 \ldots, n$; the functions L_i are linear; and we assume that L_i is an *eo ipso* predictor of y_{it} for all i and t,

$$(7) \qquad \mathrm{E}(y_{it} | y_{t-1}, \ y_{t-2}, \ \ldots; x_t, \ x_{t-1}, \ \ldots) = \\ = \mathrm{L}_i(y_{t-1}, y_{t-2}, \ \ldots; \ x_t, \ x_{t-1}, \ \ldots)$$

In matrix representation we write the vector regression system (6)-(7) as follows,

$$(8) \qquad y_t = \mathrm{B} \ z_t + \upsilon_t$$

with

$$E(y_t|z_t) = B\ z_t \tag{9}$$

We note the following general features of vector regression systems:

1) The system contains one (and only one) explanatory relation for each of the current endogenous variables;
2) In each relation, all of the explanatory variables are predetermined;
3) Each relation is an *eo ipso* predictor subject to random disturbance.

1.2. *Causal chain (CC-) systems.*

The general formula for causal chain (also known as *recursive*) systems is in the linear case given by

(10)

$$y_{it} = L_i(y_{1t},\ y_{2t},\ \ldots,\ y_{i-1,t};\ y_{t-1},\ y_{t-2},\ \ldots;\ x_t,\ x_{t-1},\ \ldots) + \upsilon_{it}$$

where $i = 1, \ldots, n$; the functions L_i are linear; and L_i is assumed to be an *eo ipso* predictor of y_{it} for all i and t,

$$\begin{aligned}(11)\quad & E(y_{it}|y_{1t},\ y_{2t},\ \ldots,\ y_{i-1,t};\ y_{t-1},\ y_{t-2},\ \ldots;\ x_t,\ x_{t-1},\ \ldots) = \\ & L_i(y_{1t},\ y_{2t},\ \ldots,\ y_{i-1,t};\ y_{t-1},\ y_{t-2},\ \ldots;\ x_{t-1},\ \ldots)\end{aligned}$$

To give the system (10)-(11) in matrix form we write

$$y_t = A\ y_t + B\ z_t + \upsilon_t \tag{12}$$

with

$$E(y_t|y'_t,\ z_t) = A\ y_t + B\ z_t \tag{13}$$

where the prime in y'_t indicates that when the vector y_t serves as an expectational condition for the variable y_{it}, this variable is deleted in the vector, and where

$$(14) \qquad a_{ik}=0 \qquad (i=1, \ldots, n;\ k=i,\ i+1, \ldots, n).$$

To state (14) in words: the matrix A is *subdiagonal* in the sense that all elements in and above the main diagonal are zero.

System (10) is called the *primary* (or *structural*) *form* of the model ([4]). Thanks to the subdiagonal design of the matrix A the current endogenous variables y_{kt} can be eliminated from the right-hand members of the primary form (10) by a sequence of iterated substitutions. This operation leads to the *reduced form* of the model,

$$(15) \qquad y_t = \mathrm{R}\, z_t + \omega_t$$

where the matrix R is given by

$$(16a\text{-}b) \qquad \mathrm{R}=(\mathrm{I}-\mathrm{A})^{-1}\ \mathrm{B}; \qquad \omega_t=(\mathrm{I}-\mathrm{A})^{-1}\ \upsilon_t$$

and the following relation can be established,

$$(17) \qquad \mathrm{E}(y_t|z_t)=\mathrm{R}\ z_t$$

showing that the component elements of R z_t are *eo ipso* predictors for the corresponding variables y_{it}, or briefly stated, that the reduced form relations make *eo ipso* predictors for the current endogenous variables.

A key feature in the generalization from VR-systems (6)

([4]) With regard to its theoretical content, a model consists of a set of assumptions and a group of theorems deduced from the assumptions. The term primary form brings in relief that this form contains the basic assumptions; the term structural form accentuates that the specification of this same form determines the theoretical structure of the entire model, including the reduced form and other relationships deduced from the basic assumptions.

to CC-systems (10) is that in CC-systems the reduced form (15) differs from the primary form (10), whereas in VR-systems the primary form and the reduced form are one and the same system (6).

The following general features of CC-systems will be noted:

1) The primary form (10) contains one (and only one) explanatory relation for each of the current endogenous variables;

2) In the transformation that leads from the primary form (10) to the reduced form (15) the current endogenous variables can be eliminated from the right-hand members by a sequence of iterated substitutions.

The causal aspect of the substitutional design has given the model its name « causal chain system ». To paraphraze, the current endogenous variables form a causal chain of the same type as in the nursery fad: « The cat on the mouse, the mouse on the rope, the rope on the hanged man's neck. »

3) Each relation in the primary form and the reduced form is an *eo ipso* predictor subject to random disturbance.

In view of the potential use of the primary form and the reduced form for predictive and other inferential purposes, 3° is a key feature of CC-systems. A model which has property 3° will be called *bi-expectational*. The bi-expectational property of CC-systems is closely tied up with the substitutional design 2°, inasmuch as the substitutions at issue are an operative procedure that carries *eo ipso* predictors into *eo ipso* predictors.

1.3. *Interdependent (ID-) systems.*

A broad class of interdependent systems is covered by the following model:

(18) $$\tilde{y}_t = A\, y_t + B\, z_t + v_t$$

where the vector

(19) $$\tilde{y}_t = \tilde{I}\, y_t = [y_{i_1 t}, y_{i_2 t} \ldots,\ y_{i_n t}]$$

is formed by current endogenous variables in such manner that for any specified i the variable y_{it} may or may not occur in y_t, and may occur twice or more; and where

(20) $$a_{k,\ i_k} = 0 \qquad (k = 1,\ \ldots,\ n)$$

that is, matrix A is such that the relation for $y_{i_k t}$ does not involve this variable in the right-hand member.

System (18) is the *primary* (or *structural*) form of the model. Regarding the primary form (18) as a system of implicit relations for determining the current endogenous variables, and solving for these variables in terms of predetermined variables, we obtain the model in *reduced form*,

(21) $$y_t = \mathrm{R}\ z_t + \omega_t$$

where

(22a-b) $$\mathrm{R} = (\tilde{\mathrm{I}} - \mathrm{A})^{-1}\ \mathrm{B}; \qquad \omega_t = (\tilde{\mathrm{I}} - \mathrm{A})^{-1}\ \upsilon_t$$

and where we assume that the relations make *eo ipso* predictors for the current endogenous variables,

(23) $$\mathrm{E}(y_t | \mathrm{R}\ z_t) = \mathrm{R}\ z_t$$

We note the following general features of interdependent systems (18):

1) The primary form (18) contains as many relations as endogenous variables;

2) The matrix $(\tilde{\mathrm{I}} - \mathrm{A})$ is nonsingular.

3) Each relation in the reduced form is an *eo ipso* predictor subject to random disturbance.

1.4. *Incentives for the generalization from VR- to CC- and ID-systems.*

1. *Aggregation over time.* Vector regression (6) is of old standing in dynamic model building, and especially in the natural sciences a good many dynamic theories can be quoted that make use of this type of approach. An important feature is that if the data so permit, the time unit can be chosen very small; hence, in principle, VR-systems (6) cover also the approch of differential equation systems of any order. Model (6) thus lies near at hand in situations where the data are registered continuously (recording barometres, seismographs, etc.), or, more generally, are registered periodically with time intervals that are short relative to the changes in the variables between the recordings.

CC-systems (10) and ID-systems (18) are recent innovations, both emerging in econometrics in the decade 1935-1945. It is not by chance that this generalization was initiated in econometrics, an area where theoretical model building had been well developed since long ago, and where by long tradition a large part of the available time series data had the form of annual aggregates. Hence there was — and is — a twofold incentive for the generalization from (6) in the direction of (10) and (18). One was that annual data were known to involve lots of information in the form of interrelations between economic factors observed in one and the same year, a source of information that cannot be exploited in the VR- approach (6). And the very existence of large masses of annual data reinforced the incentive for the generalization at issue.

2. *The chain principle of explanation and forecasting.* More recently, another incentive for the generalization has come to

the fore, inasmuch as VR-, CC- and ID-systems make use of the chain principle in extracting inference from the model. If a forecast is to span 18 months, say, and weekly data are available, a model of type (6) with weekly data would require as much as 78 links, a design that involves the danger that the forecasting errors will aggravate by accumulation; it would then be attractive to build a model of type (10) or (18) on the basis of quarterly data, say, a model that exploits the interrelations between variables as observed during one and the same period; such an approch would require only 6 links in the forecasting procedure. A specific point in this connection is that in economic statistics the observational errors are in practice relatively more important if the aggregation period is short ([5]); hence there will be a downward tendency in the magnitude of regression coefficients, and the accuracy of the forecast will not be optimal if the aggregation period is too short. To put it otherwise, if the disaggregation goes too far it works against the law of large numbers, and thereby attenuates the inference from the model. This is just one aspect of the problem what period of aggregation is optimal in the design of the model, a highly important, many-faceted and difficult question that falls outside the scope of this brief review.

The chain principle is a unifying feature of VR- CC- and ID-systems. More specifically, the forecasts are generated by the chain principle as applied to the reduced form, and this has the same mathematical structure in all three models, as seen from (8)-(9), (15)-(17) and (21)-(23). At the same time the chain principle brings in relief that the models work at three different levels of generalization. Thus in VR-systems (8) the primary form coincides with the reduced form; in CC-systems the primary form (12) is transformed to the reduced form (15) by a sequence of iterated substitutions; in ID-systems

([5]) Ref. 15, Appendix B, discusses this point with reference to correlation coefficients, and the same argument applies to regression coefficients.

the reduced form (21) cannot be obtained from the primary form (18) by iterated substitutions. This last feature is closely related to the fact that ID-systems are not bi-expectational: the predictor specification (13) of the primary form (12) of CC-systems has no parallel in ID-systems (18). The lack of parallel to (13) in (18) is clearly a stochastic feature of the models, for it would not appear if the ID-systems (18) were deterministic in the sense of disturbance-free relations; in fact, the left-hand members of (23) would then be nothing else than the component variables y_{it}, and (23) would be precisely the same system of relations as (21).

In the much-discussed dualism between CC- versus ID-systems it has been a veritable stumbling block that ID-systems are not bi-expectational ([6]). As briefly noted above, this key feature results from the merging of two lines of generalization, namely from VR- to CC- and ID-systems on the one hand, and from deterministic to stochastic specification of the models on the other. We shall return to this matter in section 2 for a more detailed review.

3. *Accounting identities vs. equilibrium relations.* To summarize the argument, accounting identities make no incentive in the generalization from VR- or CC-systems to ID-systems, whereas the incorporation of equilibrium relations into the model is one of the main incentives in the generalization from CC- to ID-systems ([7]).

The argument will be illustrated by simple cases in point. A typical accounting identity is given by

$$Y_t = C_t + S_t \tag{24}$$

([6]) See, also for further references, Refs. 16 to 18 and Ref. 30.

([7]) The exposition makes systematic use of *eo ipso* predictors; otherwise the argument of this subsection is well known.

where Y_t is gross national product, C_t total consumption, and S_t total savings. As to equilibrium relations, a typical case is the assumption

$$(25) \qquad S_t = I_t$$

where S_t is saving and I_t investment.

Speaking generally, if the primary form of a VR- CC- or ID-system involves an accounting identity, an equivalent model can be constructed by interpreting the identity as a behavioural relation for one of the variables. For example, if the model involves the identity (24) and the two behavioural relations

$$(26a\text{-}b) \qquad Y_t = L_1(z_t) + \upsilon_t \qquad \text{with} \qquad E(Y_t|z_t) = L_1(z_t)$$

$$(27a\text{-}b) \qquad C_t = L_2(Y_t, z_t) + \upsilon'_t \qquad \text{»} \qquad E(C_t|Y_t, z_t) = L_2(C_t, z_t)$$

we obtain

$$(28a\text{-}b) \qquad S_t = Y_t - C_t = L_1(z_t) - L_2(Y_t, z_t) + \upsilon''_t \ \text{ with } \ \upsilon''_t = \upsilon_t - \upsilon$$

and under general conditions the relations (24) and (26)-(27) further imply

$$(29a\text{-}b) \qquad \begin{cases} E(S_t|Y_t, z_t) = Y_t - L_2(Y_t, z_t) \\ E(S_t|z_t) = L_1(z_t) - L_2(L_1(z_t), z_t) \end{cases}$$

Turning to equilibrium relations, for example (25), the situation is fundamentally different. The model involves one behavioural relation for each of the variables that are subject to the equilibrium assumption, and the equilibrium is regarded as the result of corresponding changes in an *equilibrating va-*

riable, in this case the interest rate, say q. In symbols, let the two behavioural relations be

$$(30a\text{-}b) \quad S_t = L_1(q_t, z_t) + \upsilon'_t \quad \text{with} \quad E(S_t|q_t, z_t) = L_1(q_t, z_t)$$

$$(31a\text{-}b) \quad I_t = L_2(q_t, z_t) + \upsilon'_t \quad \text{»} \quad E(I_t|q_t, z_t) = L_2(q_t, z_t)$$

where for simplicity we have assumed that q_t is the only current endogenous variable that influences S_t and I_t. Further let M_t denote the common total of savings and investment,

$$(32) \qquad M_t = S_t = I_t$$

Then under general conditions of regularity we may substitute (30a) and (31a) into (32) and solve for the equilibrating variable, say

$$(33) \qquad q_t = L_3(z_t) + \upsilon''_t$$

Thus we may regard (32) as an implicit and (33) as an explicit behavioural relation for the equilibrium variable q_t. Now with regard to the rationale of the generalization from CC- to ID-systems the following points will be noted.

The assumptions (30)-(31) make two behavioural relations for the endogenous variable M_t, and no explicit behavioural relation for the endogenous variable q_t, and this situation is incompatible with the general design (10) of CC-systems. This is so even if the *eo ipso* predictor specifications (30b) and (31b) are abandoned. In this connection it is important to note that if specifications (30b) and (31b) are adopted, relations (30)-(31) imply

$$(34) \qquad E(q_t|z_t) \neq L_3(z_t)$$

showing that relation (33) cannot be specified so as to make an *eo ipso* predictor.

In an ID-system (18), on the other hand, it is perfectly legitimate to incorporate relations (30a) and (31a), and the ensuing relation (32) may perfectly well be specified so as to make an *eo ipso* predictor,

$$\text{(35)} \qquad E(q_t|z_t) = L_3(z_t)$$

but in the specification of the ID-system we must in general abandon the assumptions (30b) and (31b). This last point is entirely in line with the lack of counterpart to (13) in (18).

The upshot of the argument is that accounting identities (24) can be incorporated into any VR-, CC- or ID-system, whereas an equilibrium (25) makes a parting of the ways between VR- and CC-systems on the one hand, and ID-systems on the other. The salient point is that (24) is an exact identity, whereas (25) is an approximation, inasmuch as the deviations from equilibrium are ignored. This comment also gives a clue to how the situation may be dealt with in CC-systems, namely by taking the difference between the two members (30a)-(31a) of the « equilibrium » into explicit account, and exploiting it, possibly with a suitable lagging, as an explanatory factor for the equilibrating variable q_t. A simple example is given by the following model for the balance between demand and supply in a market under free competition ([8]).

Demand relation:

$$\text{(36a)} \qquad d_t = 1.580 - 0.390\, p_t + 0.520\, f_t + u_t$$

([8]) Model (36) refers to the US market for pork 1939-1956. Refs. 19 and 20 give similar models for several agricultural products, based on US data, Ref. 21. The data having been revised, Ref. 22, I am indebted to Mr. STOJKOVIC for recalculating his pork model, Ref. 20, on the basis of the revised data for the purpose of the present report.

To list the exogenous variables, f_t is a price index for farm products; c_t is corn price; g_t is a dummy variable representing war effects 1942-46; w_t is the farm wage rate.. Quantities are per capita values; prices and wages are in real terms.

supply relation:

(36b) $$s_t = 1.825 + 0.055\, p_{t-1} - 0.097\, c_{t-1} + 0.123\, g_t + u_t'$$

price mechanism:

(36c) $$p_t = 0.493\, (d_{t-1} - s_t) + 0.229\, w_t + 0.414\, c_t + u_t''$$

with all three relations specified as *eo ipso* predictors, giving

$$E(d_t | p_t, f_t) = 1.580 - 0.390 p_t + 0.520\, f_t$$

and similarly for (36b) and (36c). Model (36) has the formal design of a CC-system, inasmuch as s_t is influenced only by predetermined variables, p_t by s_t and predetermined variables, and d_t by p_t and predetermined variables.

These simple illustratious bring in relief the distinction between the notion of ***instantaneous equilibrium*** and other modes of equilibria. In an ID-system that includes (30a), (31a) and (32) the instantaneous equilibrium enters among the basic assumptions of the model. In CC-systems, on the other hand, instantaneous equilibria have no place; instead, equilibrium tendencies may enter the picture by way of theoretical deductions from the model. Thus in model (36) demand and supply will under general conditions of stochastic regularity be in stationary balance, a state of never-ceasing random fluctuations around a limiting equilibrium level.

4. *The general scope of vector regression and causal chain systems.* Brief reference is made to two groups of theorems that establish the general scope of VR- and CC-systems. The theorems refer to a set of observed time series (3)-(4) that are conceived of as extending into the indefinite past $(t-1,\ t-2,\ \ldots)$, and they have the nature of representation theorems that yield predictive inference under the assumption that the structure of the interrelations between the time series

be the same in the future as in the past. Thus it is shown that the observed time series can be represented in the form of a VR-system, a representation that holds under very general conditions and to any prescribed accuracy in the stochastic specification ([8a]). A similar theorem holds for CC-systems. Both theorems exist in two versions, one where the observed set of time series is regarded as a (multidimensional) realization of a stationary process, and the expectational properties of the VR- and CC-systems are specified in terms of *cross section averages* of the various possible realizations. The other version refers to no other realization than the observed time series, and specifies the expectational properties of the system as *averages over time* based on the single realization. The theorems are closely related to the general representation theorem known as *predictive decomposition* of stationary stochastic processes; Refs. 15, 24 and 46. The predictive decomposition is parametric, and the parameters are uniquely determined. An important feature of the predictive decomposition and of CC-systems is that representations of this type yield predictions that are optimal in the sense of *minimum-delay* of information ([9]). Thus far we have referred to the given time series as stationary, but the representation theorems extend to the case of nonstationary processes; Ref. 25.

Mathematical generalization is not an unmixed blessing. When a theoretical model is generalized so as to cover wider areas, the basic assumptions are relaxed to some extent, and the relaxation brings on that the inference from the model is attenuated in some respect or other. The ensuing balance between generalization and attenuation of inference is a most important aspect of the three models under review. To summarize, any set of observed time series can be cast in the form

([8a]) For this and the following theorem, see (also for further references) Ref. 23.

([9]) Announced in Ref. 46, the full proof of the CC-representation is as yet unpublished.

of a VR-system And CC-systems, too, are of general scope in the same sense. Here the substitutional design referred to in 1.2 (2), is the salient point. Thanks to this design it is possible to carry through the specifications (13) of the primary form and (17) of the reduced form in terms of conditional expectations, although this bi-expectational specification of CC-systems might seem highly restrictive at first sight. At the same time the substitutional design marks the limit beyond which the generalization from VR-systems in the direction of CC- and ID-systems cannot be pushed without losing the bi-expectational property. Hence, as noted in 1.4 (2), ID-systems in general are not bi-expectational. Another important property that goes lost in the generalization from CC- to ID-systems is the optimality with regard to minimum-delay of information.

2. Ends and means in the transition from deterministic to stochastic approaches

Whe shall in this section consider a number of situations that are special instances of the universal mathematical rule that generalization of a theoretical model is always accompanied by attenuation of inference from the model. The theorems relevant to the argument of this section belong to the foundations of probability theory; thus (50) goes back to the beginnings of correlation and regression analysis around 1900, and the origin of (41) is still more remote.

2.1. *A review of basic notions.*

The transition from deterministic to stochastic specification is a radical generalization of a theoretical model, and the ensuing attenuation of inference goes down to the very foundations of model building. To emphasize the basic arguments we shall start from scratch and give some few simple illustrations of the notions of *univariate distribution* and *bivariate relationship*.

With reference to Figs. 2a and 3a, let x be the (unspecified) molecular weight of sugar. Physical chemistry tells us that the sugar molecules are crystals, all of which have the same weight, $\mu = 3.01 \times 10^{-22}$ grams, that is 180 times the atom weight of hydrogen. If the weight μ could be measured exactly, the situation would be as shown in Fig. 2a. In practice, the weighting is subject to observational error, and if the errors follow the normal distribution the measurements will be distributed as shown in Fig. 3a. The observed average $\bar{x}$ of this distribution provides a point estimate of the unknown molecular weight μ. Next let x be the molecular weight of a polymere, say a specific make of nylon. The nylon molecules are bands of different length; that is, x is not a specific number, but a variable subject to a specific distribution, say, as shown in Fig. 4a. Here μ denotes the mathematical expectation of the distribution,

$$\mu = \mathbf{E}[x] \qquad (37)$$

Distinguishing between the theoretical and the observed distribution, as illustrated in Fig. 5a, the observed mean $\bar{x}$ gives a point estimate of the theoretical mean μ. Conceptually, the dotted curve represents the distribution of a variable $x = x^* + \varepsilon$ which is composed of a variable x^* with the same distribution as in Fig. 4a, and an observation error ε which for fixed x^* has a distribution of the same type as in Fig. 3a. In the present illustration it so happens that the molecular distribution can only be observed indirectly, since the individual molecules are too small for direct observation. Conceptually, we may think of the observed distribution as referring to the individual molecular weights subject to observational error.

Comparing the situation in Figs. 2a and 3a with the more general situation in Figs. 4a and 5a we note two simple instances of attenuated inference:

1) In Fig. 2a the weight μ refers to each of the sugar molecules, and similarly for the estimate $\bar{x}$ in Fig. 3a. In Fig. 4a the weight μ holds only as an *average* for all of the nylon molecules subject to observation, and similarly for its estimate $\bar{x}$ in Fig. 5a.

2) Let M be the (average) second order momentum of the molecules. For the sugar molecules the theoretical model illustrated in Fig. 2a gives

$$M = \mu^2 \tag{38}$$

and in the model illustrated by Fig. 3a a point estimate of M is given by

$$M \sim \bar{x}^2 \tag{39}$$

For the nylon molecules the theoretical distribution gives, as illustrated in Fig. 4a,

$$M = \mu^2 + \sigma^2 \tag{40}$$

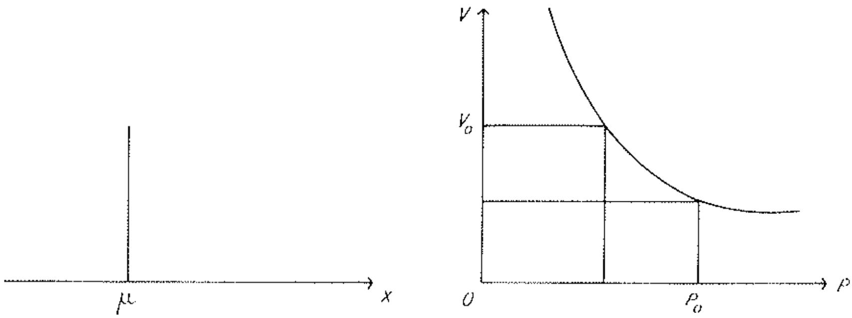

FIG. 2 — *Nonstochastic variables subject to exact observation.* a) *Molecular weight of sugar.* b) BOYLE'*s law* $PV = c$.

where σ is the standard deviation of the distribution. The simple point we wish to illustrate is that the inference (38) for

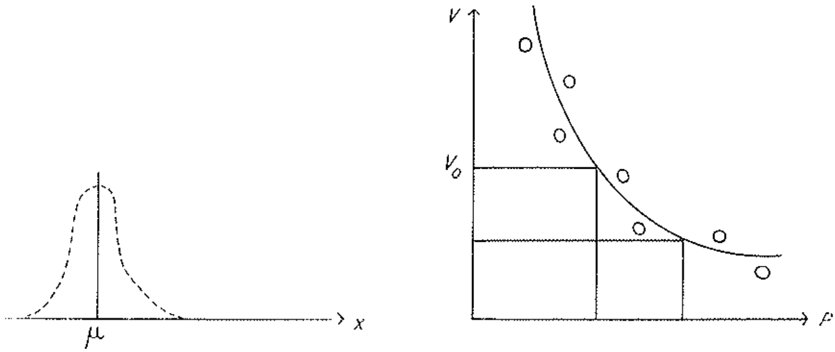

FIG. 3 — *Nonstochastic variables subject to observational error.* a)-b) *Same variables as in Fig. 2 a-b.*

the deterministic situation does not extend to the inference (40) for the stochastic situation, or more generally,

$$(41) \qquad f(\mathrm{E}[x]) \neq \mathrm{E}[f(x)]$$

that is, the expectation of a nonlinear function $f(x)$ will only in exceptional cases equal the function of the expectation.

Turning now to bivariate relationships, Figs. 2b and 3b refer to BOYLE's law,

$$(42) \qquad \mathrm{P\,V} = c$$

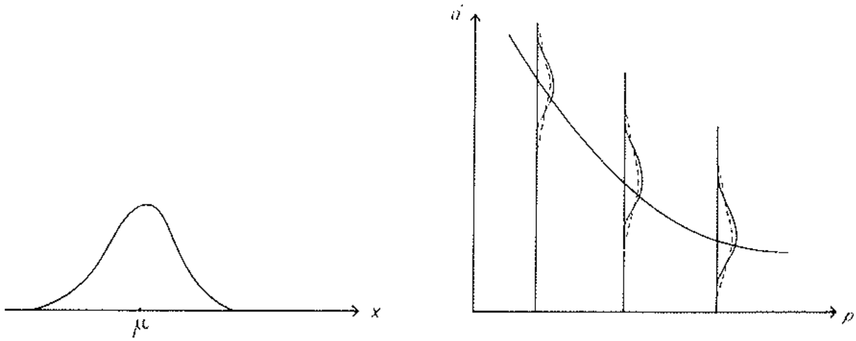

FIG. 4 — *Stochastic variables subject to exact observation.* a) *Molecular weight of nylon.* b) *Consumer demand y as function of market price x.*

or in words: For an ideal gas kept at constant temperature in a closed container with alterable volume, pressure P times volume V is constant. In the accuracy of ordinary scale readings, BOYLE's law holds as a deterministic relation. Typical inferences from BOYLE's law are that if the volume of the container is known, say V_o, the gas pressure is given by

$$P = c/V_o \tag{43}$$

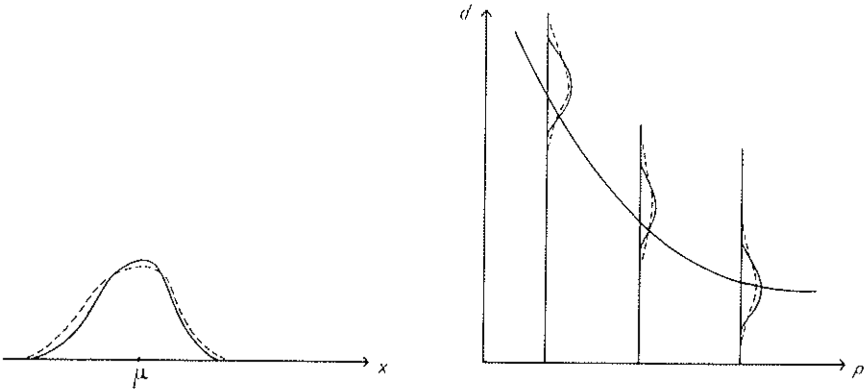

FIG. 5 — *Stochastic variables subject to observational error.* a)-b) *Same variables as in Fig. 4 a-b.*

and if the pressure is known, say P_o, the volume is

$$V = c/P_o \tag{44}$$

If an experiment is performed to demonstrate BOYLE's law there will be small deviations owing to observational errors in P and V, as shown in Fig. 3b. Taking the constant c to be unknown it can be estimated from the data, for example by the method of least squares as applied to the logarithmic relation

$$\log P + \log V = \log c \tag{45}$$

The resulting estimate can then be used for the inferences (43) and (44).

Next we come to Fig. 4b, which illustrates a demand relation specified by way of an *eo ipso* predictor, say

$$(46) \qquad d = c\, p^{-\alpha} + \upsilon$$

with

$$(47) \qquad \mathrm{E}(d|p) = c\, p^{-\alpha}$$

where α is the demand elasticity with respect to price. A typical inference from the model (46)-(47) is that if price is known, say p_o, the expected value of consumer demand is given by

$$(48) \qquad d = c\, p_o^{-\alpha}$$

Comparing with (43) and (44) we note that the present inference is attenuated in two respects:

1) Whereas the inference (43) is deterministic, exact, the inference (48) about consumer demand is designed to be true only as an expected or average value. This is so because demand is influenced by many other factors than price, influences that are summed up in the residual variable υ.

2) Whereas the deterministic relation (42) allows the twofold inference (43)-(44), the stochastic model (46)-(47) allows only the prediction (48) of d for known p. In fact, if we solve (48) for p_o and drop the subscript the ensuing relation

$$(49) \qquad p = c^{\frac{1}{\alpha}}\, d^{-\frac{1}{\alpha}}$$

is not an *eo ipso* predictor,

$$\mathrm{E}(p|d) \neq c^{\frac{1}{\alpha}}\, d^{-\frac{1}{\alpha}}$$

More generally, considering model (1) and letting $f^{-1}(\cdot)$ denote the inverse function of $f(\cdot)$, we have

$$(50) \qquad E(x|y) \neq f^{-1}(y)$$

The inference (49) from d to p would hold good if the model (46) were disturbance-free (υ having probability one of being equal to zero), or if our model were not (46) but instead

$$(51) \qquad p = c^{\frac{1}{\alpha}} d^{-\frac{1}{\alpha}} + \omega$$

with

$$(52) \qquad E[p|d] = c^{\frac{1}{\alpha}} d^{-\frac{1}{\alpha}}$$

In such case the ratio $1/\alpha$ would be an operationally meaningful quantity, namely, the *price flexibility* with respect to demand [10]. Again, of course, model (51)-(52) does not allow the reverse inference (47)-(48).

Coming finally to Fig. 5b, the measurements are here subject to observation error. Thus for fixed p, the dotted curve represents the distribution of $d = d^* + \varepsilon$, where d^* has the same distribution as in Fig. 4b, and ε is an observation error with the same type of distribution as in Fig. 3a.

2.2. *Operational aspects of deterministic and stochastic models.*

The simple illustrations in 2.1 have been selected with a view to elucidate three aspects of the transition from deterministic to stochastic approaches.

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(10) See Ref. 26, which is a basic reference for deterministic approaches in econometrics.

(1) *Deterministic and stochastic models do not obey the same operational rules.* Since stochastic models cover deterministic models as a special case, the general rules for operating with *eo ipso* predictors are valid also for deterministic models, but the converse is not always true. Operations that in a general way extend from deterministic relationships to *eo ipso* predictors include addition and substitution. Procedures that never extend to *eo ipso* predictors include squaring and inversion. The explicit solving of a system of implicit relationships extends to *eo ipso* predictors only in the special case when the solving can be performed by iterated substitutions ([11]). It is this last restriction that lies behind the fact, noted in 1.2 (3) and 1.4 (3), that the predictor specification (13) of the primary form of CC-systems has no counterpart in ID-systems.

Causal relations ([12]). If we compare the operative aspects of cause-effect relationships and *eo ipso* predictors we note a far-going isomorphism, and specifically so with regard to the basic operations of inversion and substitution. Thus if y is influenced by a causal factor x, this does not imply that x is influenced by y; isomorphically, if $f(x)$ is an *eo ipso* predictor of y this does not imply that $f^{-1}(y)$ is an *eo ipso* predictor of x. As regards substitution, if y is influenced by a causal factor x, and x is influenced by a causal factor z, we say — and in principle this is a piece of causal inference — that y is influenced by z via x. For linear *eo ipso* predictors we have the corresponding theorem that if the variables x, y, z are interrelated by

(53a-b) $$\mathrm{E}(y|x, z) = f(x, z) \quad \text{and} \quad E(x|z) = g(z)$$

then ([13])

([11]) Ref. 27; cf. also Refs. 28 and 29.

([12]) For a more detailed discussion of the causal aspects of model building, see Refs. 16-19 and 30.

([13]) See Ref. 12 for a detailed treatment of the linear case. The substitutional theorem is in (53)-(54) quoted for three one-dimensional variables x, y, z. It extends to the case when z is a vector variable, the key feature being that functions f and g involve the same vector z.

$$(54) \qquad E(y|z) = f(g(z), z)$$

Thanks to the isomorphism here briefly touched upon, *eo ipso* predictors are a most convenient tool for the analysis of cause-effect relationships. Since behavioural relations are cause-effect relationships, this last comment leads us to the second point, namely:

(2) *The rationale of making use of eo ipso predictors in the specification of behavioural relations.* The main argument is, of course, that unless a behavioural relation makes an *eo ipso* predictor it cannot provide forecasts that are unbiased in the sense of expected or average values. This point is brought in relief by (47) and (50), and the reader will have no difficulty to supply any number of similar illustrations.

(3) *Eo ipso predictors in multipurpose model building.* Speaking broadly, the transition from deterministic to stochastic models makes no trouble, in principle, if the model involves just one relation of potential use for forecasting; all that is needed is to design the relation so as to make an *eo ipso* predictor. It is quite another matter that the relation can be a bad forecasting device because of specification errors, but in this respect there is no difference between deterministic relations and *eo ipso* predictors. The trouble begins when the model involves two or more predictive relations, inasmuch as the corresponding *eo ipso* predictors may be incompatible. It is important to note that the ensuing questions of compatibility or noncompatibility belong to the pure probability theory; no empirical or substance-matter considerations enter into these matters. Such is the situation in (48)-(49), where the inference from p to d and from d to p cannot be obtained by way of two *eo ipso* predictors that form a pair of inverse functions. This nonexistence theorem in probability theory was one of the cornerstones when KARL PEARSON laid the foundations of correlation and regression analysis, but its implications for causal analysis by regression methods remained obscure for

a long time, as witnessed by the debate on « the choice of regression » in the 1920's and 1930's. Such is also the situation in multirelation systems with regard to predictive inference from the primary form and the reduced form. In VR-systems this dualism does not arise since the two forms coincide; in CC-systems both forms can be specified in terms of *eo ipso* predictors thanks to the substitutional design of the primary form; but the design of ID-systems is too general to allow this bi-expectational specification. The situation makes a genuine dilemma for the ID-approach, for if the primary form with its behavioural relations cannot be specified in terms of *eo ipso* predictors and thereby as cause-effect relations subject to random disturbance, the operational meaning of the entire model comes in doubt.

The dilemma of ID-systems is reflected in the debate on the rationale of « simultaneous equation systems » in the 1940's and 1950's. In hindsight, what has made the controversial and partly confused debate on « the choice of regression » and « simultaneous equation systems » so persistent is the old and strong tradition of deterministic model building in economics, combined with the fact that empirical treatment of the models and thereby the need for their stochastization came into the picture at a relatively late stage. In the research literature the need for stochastic models was fully recognized in the early 1930's, but in economic textbooks the deterministic models still dominate the scene. Specific reference is made to the deterministic cobweb model of 1930, in the simplest case given by [14]

$$(55a\text{-}c) \qquad \begin{cases} s_t = S(p_{t-1}) & \text{supply relation} \\ d_t = D(p_t) & \text{demand relation} \\ d_t = s_t = q_t & \text{instantaneous equilibrium} \end{cases}$$

[14] See M. EZEKIEL (1938) for an excellent review. Cf. also H. SCHULTZ (1938), pp. 77-80.

in which model we further note the ensuing relation for current price,

$$p_t = D^{-1}(q_t) = D^{-1}(S(p_{t-1})) \tag{56}$$

This famous model that has entered a great many textbooks is perhaps the single feature that has contributed most to the obscurity in the debate on « simultaneous equation systems. » We see that the model paves the way into two main pitfalls that hamper the stochastization of multipurpose deterministic models, one being the inversion of single relationships, the other being the explicit solving for the current endogenous variables in nonrecursive multirelation models. It should be clear from the above that this comment is not written in a critical vein, but rather to emphasize the innovating features of CC- and ID-systems. If an appraisal is in place, it is to pay homage to JAN TINBERGEN, one of the three initiators of the cobweb approach, whose superb intuition led him around these pitfalls later on when he constructed the first CC-systems.

A way out of the dilemma referred to is provided by a recent theorem that makes ID-systems bi-expectational by means of a respecification of the primary form. We proceed to a brief presentation of this new twist of the ID-approach.

2.3. *On bi-expectational interdependent (BEID-) systems* ([15]).

Given an ID-system (18)-(23), the corresponding BEID-system is obtained as follows: The primary form (18) is respecified by the definition

$$\tilde{y}_t = A\, y^*_t + B\, z_t + v^*_t \tag{57}$$

([15]) For equivalent results in less elaborate form see Ref. 13, Theorem 10. and Ref. 12, Remark 3.2.2b.

where A, B and z_t are the same as in (18), while ([15a])

$$y_t^* = E(y_t | R z_t) = R z_t \tag{58}$$

is the vector of conditional expectations of the current endogenous variables as given by the reduced form, which as before is assumed to be given by (21)-(22) with (23), to repeat

$$y_t = (\tilde{I} - A)^{-1} B + \omega_t \tag{59}$$

Then the following relation can be established,

$$E(\tilde{y}_t | y_t^*, z_t) = A y_t^* + B z_t \tag{60}$$

showing in conjunction with (58) that the BEID-system is bi-expectational in the sense of 1.2 (3).

As to the proof of (60), we note that the model becomes deterministic if we respecify the primary form (57) and the reduced form (59) by deleting all residuals and in the left-hand members substitute $\tilde{y}_t^*$ and y_t^* for $\tilde{y}_t$ and y_t. This follows as an immediate corollary from the substitution theorem (53)-(54), allowing z to be a vector variable.

In the debate on « simultaneous equation systems » it has been a key point what causal interpretation, if any, can be given to the parameters a_{ik} of the behavioural equations in the primary form (18) of ID-systems (again, see footnote [6]). The parameters a_{ik} being numerically the same as in the corresponding BEID-system, relations (57) and (60) give the answer that the parameters allow the same cause-effect interpretation as in CC-systems, except that whenever a current endogenous

([15a]) In the manuscript as presented at the Study Week, the matrix R was missing in $E(y_t | R z_t)$ in formulas (23) and (58). Cf. the paper (*b*) referred to in the subsequent discussion, page 56, footnote ([1]).

variable y_{kt} occurs as causal (explanatory) variable it must be replaced by its expected value y^*_{kt} as given by the reduced form. Or to paraphraze in terms of MARSHALL elasticities, if all variables y_{it}, z_{it} in (18) are logarithmic, and $y_{i_k,t}$ is current demand and y_{jt} current price, then a_{kj} is the elasticity of demand with respect not to *observed* price y_{jt} but to *expected* price y^*_{jt}.

It will be noted that the expected valne y^*_{it} of a current endogenous variable is in (58) introduced as a purely stochastic concept. It is an entirely different issue whether this expected valne can be given a subject-matter interpretation as an expectation in the psychological sense. Thus if y_{jt} is observed market price, and the consumers' anticipations of market price could be assessed, say y^{**}_{jt}, for example by interviews on a sampling basis, the definition (58) involves no implicit conjecture as to whether y^*_{jt} and y^{**}_{jt} will be approximately equal.

The parameters of a BEID-system are numerically the same as for the corresponding ID-system. Hence the problem of parameter estimation is precisely the same for BEID- as for ID-systems. Among the estimation techniques developed for ID-systems, specific reference is made to H. THEIL's two-stage method of least squares, Ref. 33, which conforms operationally to an extension to BEID-systems. Briefly stated, the procedure is to estimate the reduced form by least squares regression, substitute the resulting estimates for the left-hand members into the right-hand members of the primary form, and then estimate the primary form by least squares regression.

In the following illustration we shall consider three types of model, all with the same patterns of nonzero coefficients A and B in (18), but in general with different numerical values for the nonzero coefficients.

(1) *ID-systems,* or *RFUE-* (reduced form uni-expectational) *systems.* This is an arbitrary system of type (18).

(2) *PFUE-* (primary form uni-expectational) *systems.* This model is obtained from (18) by respecifying the nonzero

parameters so that all relations in the primary form make *eo ipso* predictors.

The notation RFUE-system serves to emphasize that the reduced form but in general not the primary form makes a set of *eo ipso* predictors. In PFUE-systems it is the other way around ([16]).

(3) *BEID-* (bi-expectational interdependent) *systems.* Here, to repeat, both the primary and the reduced form are specified in terms of *eo ipso* predictors.

Illustrations ([17]). Whereas an ID-system and the corresponding PFUE- and BEID-systems in general generate three different stochastic processes, the following three models have been designed so as to generate one and the same stochastic process. Hence if a realization has been generated from one of the models, the realization by itself cannot indicate from which one of the three models it has been generated. The process involves two endogenous variables p_t, q_t and no exogenous variable, and it is stationary and GAUSS-MARKOVIAN with the following nine parameters,

$$\text{(61)} \qquad E(p_t) = E(q_t) = 0; \qquad \sigma(p_t) = \sigma(q_t) = 1$$

$$\text{(62)} \qquad E(p_t q_t) = -\frac{4}{5}; \; E(p_{t-1} q_t) = \frac{3}{5}; \; E(p_{t-1} p_t) = -\rho;$$

$$E(q_{t-1} q_t) = -\frac{12}{25}; \; E(q_{t-1} p_t) = \frac{4}{5}\rho$$

([16]) PFUE-systems are what I have earlier, Refs. 12, 28 and 30, called *implicit* or *conditional* causal chain (CCC-) systems, covering as special cases *circular* and *bicausal* chain systems.

([17]) Models (65)-(67) and (68)-(70) are quoted from Ref. 30. I am indebted to Dr. LYTTKENS for pointing out an erratum in Ref. 30, p. 394, where the relation that corresponds to (69 *c*) is wrongly stated as $E(v_{1t}\; v_{2,t\pm k}) = 0$. The erratum does not affect the statement that the three models there considered define one and the same stochastic process, but it does destroy the MARKOV character of model (68)-(70). For example in (66b) we have

$$E(q_t | p_{t-1}) = E(q_t | p_{t-1},\; p_{t-2}, q_{t-2},\; p_{t-3}, \ldots)$$

but in general not so in (70b).

where ρ is a constant that can be fixed arbitrarily in the interval

$$(63)\qquad 0 \leq \rho \leq 0{,}96 \ .$$

The basic ID-system is a highly simplified demand-supply model where instantaneous equilibrium is assumed,

$$(64)\qquad \text{demand } d_t = \text{supply } s_t = q_t$$

To bring out the characteristic differences as clearly as possible the primary forms of the models have been constructed so as to differ only in one parameter, namely the coefficient of price p_t in the demand relation.

An ID- (or RFUE-) system. The primary form:

$$(65a\text{-}b)\quad \begin{cases} d_t = q_t = -\dfrac{3}{5\rho} p_t \quad + \upsilon_{1t} \\ s_t = q_t = \quad \dfrac{3}{5} p_{t-1} + \upsilon_{2t} \quad \text{with} \quad E(q_t | p_{t-1}) = \dfrac{3}{5}\ p_{t-1} \end{cases}$$

The reduced form:

$$(66a\text{-}b)\quad \begin{cases} p_t = -\rho\ p_{t-1} + \omega_{1t} \quad » \quad E(p_t | p_{t-1}) = -\rho\ p_{t-1} \\ q_t = \dfrac{3}{5}\ p_{t-1} + \omega_{2t} \quad » \quad E(q_t | p_{t-1}) = \dfrac{3}{5}\ p_{t-1} \end{cases}$$

and

(67a-c)

$$E(\upsilon_{1t}\ \upsilon_{1,t\pm k}) = E(\upsilon_{2t}\ \upsilon_{2,t\pm k}) = E(\upsilon_{1t}\ \upsilon_{2,t\pm k}) = 0\,; \quad k = 1,\ 2,\ \ldots$$

The corresponding PFUE-system. The primary form:

$$(68a\text{-}b)\quad \begin{cases} d_t = q_t = -\dfrac{4}{5} p_t + \nu_{1t} \quad \text{with} \quad E(q_t | p_t) = -\dfrac{4}{5} \\ s_t = q_t = \quad \dfrac{3}{5} p_{t-1} + \nu_{2t} \quad » \quad E(q_t | p_{t-1}) = \dfrac{3}{5} p_{t-1} \end{cases}$$

and

(69a-c)

$$E(\nu_{1t}\ \nu_{1,t\pm k}) = E(\nu_{2t}\ \nu_{2,t\pm k}) = E(\nu_{1t}\ \nu_{2,t+k}) = 0; \quad k = 1, 2, \ldots$$

The reduced form:

(70a-b)
$$\begin{cases} p_t = -\dfrac{3}{4}\, p_{t-1} + \nu'_{1t} \\ q_t = \dfrac{3}{5}\, p_{t-1} + \nu_{2t} \quad \text{with} \quad E(q_t | p_{t-1}) = \dfrac{3}{5}\, p_{t-1} \end{cases}$$

The corresponding BEID-system. The primary form:

(71a-b)
$$\begin{cases} d_t = q_t = -\dfrac{3}{5\rho} \overset{*}{p}_t + \overset{*}{\nu}_{1t} \quad \text{with} \quad E(q_t | \overset{*}{p}_t) = -\dfrac{3}{5\rho} \overset{*}{p}_t \\ s_t = q_t = \dfrac{3}{5} p_{t-1} + \overset{*}{\nu}_{2t} \quad \text{»} \quad E(q_t | p_{t-1}) = \dfrac{3}{5}\, p_{t-1} \end{cases}$$

and

(72a-b)

$$E(\overset{*}{\nu}_{1t}\ \overset{*}{\nu}_{1,t\pm k}) = E(\overset{*}{\nu}_{2t}\ \overset{*}{\nu}_{2,t\pm k}) = E(\overset{*}{\nu}_{1t}\ \overset{*}{\nu}_{2,t\pm k}) = 0; \quad k = 1, 2, \ldots$$

Reduced form:

(73a-b)
$$\begin{cases} p_t = -\rho\ p_{t-1} + \omega_{1t} \quad \text{with} \quad E(p_t | p_{t-1}) = -\rho\ p_{t-1} \\ q_t = \dfrac{3}{5}\ p_{t-1} + \omega_{2t} \quad \text{»} \quad E(q_t | p_{t-1}) = \dfrac{3}{5}\ p_{t-1} \end{cases}$$

giving

(74)
$$p^* = -\rho\ p_{t-1}\,.$$

The first point we wish to illustrate is that all four relations (71) and (73) make *eo ipso* predictors. It will be noted that in any BEID-system the expectational variables y^*_{it} are linear expressions in the predetermined variables z_{it}. Owing to the very simple structure of the model (71)-(74) the expectational variable p^*_t is in the present case nothing else than $-\rho\ p_{t-1}$. As a consequence, the demand relation (71a) coincides with the supply relation (71b). This last feature illustrates how the reduced form may in the BEID-approach contain more information than the primary form.

Another point for which the three models provide clearcut illustration is that once the stochastic structure of the model is specified the parameter estimation is technical matter and therefore, in principle, a noncontroversial problem. For *eo ipso* predictors least squares regression provides consistent estimates; hence, for example, when applied to time series data generated from the stochastic process specified by (61)-(63) the regression of p_t on d_t will provide a consistent estimate for the coefficient -0.8 in the demand relation (68a) of the PFUE-system, but in general not for the coefficient $-0.6/\rho$ in the demand relation (65a) of the ID-system. We see that if the least squares regression is applied to (65a) the bias may be quite substantial, depending on the numerical value of ρ, and that the least squares estimate will be unbiased only in the special case when $\rho=0.75$.

3. Predictive testing of nonexperimental models [18]

In the big arsenal of statistical methods, the techniques for the design and analysis of experiments are on the whole much more developed and refined than the techniques available for nonexperimental data. This is in particular so for the statistical

[18] The general argument of this section borrows from Ref. 34.

procedures of hypothesis testing. In experimental situations the empirical testing of a model can be based on replications under controlled conditions. It is here a recognized principle to treat the experiment as self-contained, not allowing the test procedure to exploit any information outside the observed sample ([19]). For the testing of a variety of models a great many routine techniques are available that are of maximum power under the specified experimental conditions. In nonexperimental situations the principle of the self-contained experiment is an unattainable ideal. Instead, approaches come to the fore that in a more or less systematic manner exploit other sources of information than the theoretical model and the empirical observations used for the estimation of its parameters. Such sources include comparisons with the results of similar or related models constructed for other regions or time periods; testing the validity of the model by ancillary theoretical arguments *ad hoc*; and, first and last, *predictive tests* where forecasts from the model are followed up by observation *ex post* and comparison with the actual course of events. Confrontation with fresh evidence is, clearly, the real touchstone for the scientific validity of the model as well as for its practical use.

Specific reference is made to KLEIN-BALL-HAZLEWOOD-VANDOME's macroeconomic quarterly forecasting system for UK, Ref. 37, and its predictive testing, Refs. 38 and 39. Here is a keen follow-up study that without hesitation sets forth how the predictions conform or fail to conform to the actual developments, and the ensuing lucid and instructive comments sort out the weak and strong points of the model. Such follow-up studies are highly important, and indeed an indispensable supplement to the published models, both for improving the model subject to scrutiny, and as a guide for other related forecastiug projects. Some twenty models have been constructed in seven different countries for predicting the boom-recession pulsations,

([19]) Stated by R.A. FISHER (1935), the principle has been brought to full significance by J. TUKEY (1954).

and the number of models is rapidly increasing ([20]). The models are huge and complex systems, with scores of relationships, and up to hundred or more variables, and often with shifts of emphasis in the design that render it difficult or impossible to make comparative studies on a theoretical basis. In this situation predictive tests are at a premium because they bring the models on the same line in comparing the forecast performance. In the present stage of development the need for predictive testing is especially urgent. Once a model is in permanent use the ensuing forecasts will in due course automatically give material for predictive tests. As yet, however, very few, if any, of the published models are going concerns, and therefore it takes a nonautomatic decision — not to speak of the courage — to plan and carry through predictive tests.

In view of their key importance I wish to make a plea for the systematic use of predictive tests in the construction of dynamic macroeconomic models. This last section of my report will have fulfilled its main purpose if it can stimulate to a joint move in this direction by the participants of the Study Week.

In making this plea I wish to emphasize that the entire area of dynamic model building is as yet in an early stage of development. It is perhaps too early as yet to expect forecasts that look neat in the sharp light of a predictive test. So much the more pressing, however, is the need for predictive tests for the guidance of research in the many branching complexities of macroeconomic model building.

3.1. *The pluralism in model building for different purposes.*

The rest of this report takes up some few specific aspects of the techniques of predictive testing. The various questions

([20]) The many-faceted and rapid developments are well pictured in M. NERLOVE's survey, Ref. 40.

are dealt with from the point of view of model building in non-experimental situations in general, not only in econometrics. The first point refers to the basic pluralism when it comes to model building for different purposes. Typical in this respect is the difference between short range and long range forecasting and the ensuing differences in the model construction. To bring the general perspective in relief, reference is made to similarities in this respect in economics and meteorology.

	Short range forecasts	Long range forecasts
Meteorology . .	24 or 48 hours	4 or 6 weeks
Economics . . .	6 or 12 or 18 months	5 or 10 years

In meteorology the short term forecasts are fairly reliable over 24 to 48 hours. The principal basis of the short term forecasting techniques is the thermodynamic theory of cyclons of the BERGEN school ([21]). The importance of this theoretical innovation can be read off in the gradual increase in the reliability of the forecasts from 1920 or thereabout.

Meteorological forecasts over the « long range » of 4 or 6 weeks is a more recent development. Here the forecasting has to be based on other phenomena than the cyclons and their individual paths. The technique is still in its beginnings, and the reliability is much lower than for the short range forecasting.

In economic forecasting, « short term » means ranges from 3 or 6 months up to 6 or 8 quarters, and the model building here focusses on the boom-recession pulsations. « Long range » usually means something like 5 or 10 years, and the all-important purpose of the model is to analyze and forecast economic growth. Hence, just as in meteorology, the subject-matter content of the economic model is radically different in short range and long range forecasting. On the other hand

([21]) J. BJERKNES (1919) is a basic reference. For a recent review by one of the co-founders, see T. BERGERON (1959).

there is a notable disparity, for in meteorology the emphasis on forecasting is the same in both approaches, whereas in econometrics this emphasis is more pronounced in short range than in long range forecasting. As is well known, econometric models of economic growth often are a hybrid between strict forecasting and economic programming and policy making.

3.2. *The Janus quotient: A predictive test criterion.*

The Janus quotient, an adaptation of the FISHERIAN F-ratio, has recently been proposed for purposes of predictive testing (22). Its formula is

$$(75) \qquad J^2 = \frac{\frac{1}{m} \sum_{t=n+1}^{n+m} (y_t - a_t)^2}{\frac{1}{n} \sum_{t=1}^{n} (a_t - y_t)^2} \quad \text{or } J = +\sqrt{J^2}$$

depending on whether we prefer to compare variances or standard deviations. The test refers to a specified model, say M. The notations are $a_1, a_2, \ldots$ for the actual observations, and $y_1, y_2, \ldots$ for the corresponding theoretical values as obtained from the model. Conceptually, the JANUS quotient refers to the instant between the past and the future, and as indicated by its metaphoric name, the JANUS quotient looks in two directions, backward in time over the observation range $t=1, \ldots, n$ to form the denominator, forward in time over the prediction range $t=n+1, \ldots, n+m$ to form the numerator.

The design of the JANUS quotient can be generalized and varied so as to adapt to different types of application. Let

(22) See Ref. 43, also for further details. Cf. the U-criterion earlier proposed by H. THEIL, Ref. 33, from which the J-criterion differs in being invariant to linear scale transformations.

us consider a situation often encountered in applied work, namely, when the forecasts $y_{t+1}, \ldots, y_{t+m}$ from model M are formed by means of ancillary forecasts of one or more exogenous variables (23); let $y^*_{t+1}, \ldots, y^*_{t+m}$ be the quasi-forecasts obtained when the exogenous variables are known at the end of the forecast period and substituted for the ancillary forecasts in M; then

$$(76) \qquad J^2_* = \frac{\frac{1}{m}\sum_{t=n+1}^{n+m} (y^*_t - a_t)^2}{\frac{1}{n}\sum_{t=1}^{n} (a_t - y_t)^2} ,$$

is a measure of the accuracy of the forecast model M when those forecasting errors are removed which arise from imperfect ancillary forecasting.

Since the numerator and denominator of the JANUS quotient measure the deviations between theoretical and observed values in the observation range and the forecasting range, respectively, the JANUS quotient may be regarded as a criterion of *stable model structure* in the two ranges. To elaborate this point we shall consider two types of forecast.

(1) *Forecasting by extrapolation* (24).

This approach includes *forecasting by deterministic extrapolation,*

$$(77\text{ a}) \qquad y_t = f(t) + \upsilon_t \quad \text{with} \quad E(y_t) = f(t)$$

where $f(t)$ is a specified function, usually with parameters estimated from the observation range. For example, $f(t)$ may

(23) Cf. HAZLEWOOD-VANDOME (1961), where forecasts of type y^*_{t+k} are referred to as being obtained by *extrapolation*. For applications of the same device in unirelation models, see R. BENTZEL (1959).

(24) See Ref. 43 for a more elaborate treatment, including applications of (75)-(76) to unirelation models.

be a linear or curvilinear trend, a sinusoid or a sum of periodic components. Now assuming that the model (76) is valid both in the observation range and the forecasting range we have, as a first approximation,

$$E(J^2) \approx 1 \tag{77 b}$$

valid if the forecast range is short relative to the observation range.

The present approach further includes *forecasting by exogenous variables.* Considering the linear case, model M is

$$y_t = \beta_0 + \beta_1 x_{1t} + \ldots + \beta_h x_{ht} + v_t \tag{78}$$

with

$$E(y_t | x_{1t}, \ldots, x_{ht}) = \beta_0 + \beta_{1t} x_{1t} + \ldots + \beta_h x_{ht} \tag{79}$$

where the coefficients β_i of the exogenous variables x_{it} usually are estimated from the observations. Assuming stable model structure we have in this case

$$E(J^2_*) \approx 1 \tag{80}$$

provided the forecast range is short.

The criteria (75) and (76) can be developed in the direction of significance tests. The following result is due to S. MARTINELLE ([25]). As applied to a model (77 a) with k linear compo-

([25]) Ref. 45. I am indebted to Mr. MARTINELLE for kindly placing his unpublished results at disposal for the present report.

nents $f_i(t)$ and with residuals υ_t independently and normally distributed, the JANUS quotient (75) is distributed as

$$(81) \qquad J^2 = \frac{n}{m} \frac{\chi^2(m-k) + \sum_{i=1}^{k} (1+\lambda_i)\chi_i^2(1)}{\chi^2(n-k)}$$

Here the χ^2-variates are independent with degrees of freedom indicated within paranthesis; $\lambda_1, \ldots, \lambda_k$ are the roots of equation

$$\det (C_p - \lambda C) = 0$$

where C is the product sum matrix

$$C = X' X$$

with

$$X = \begin{bmatrix} f_1(1) & f_2(1) & \ldots & f_k(1) \\ \ldots & \ldots & \ldots & \ldots \\ f_1(n) & f_2(n) & \ldots & f_k(n) \end{bmatrix}$$

and C_p is the corresponding product sum matrix for the prediction range. It will be noted from (81) that the JANUS quotient tends to increase with the prediction span and with the number of free parameters.

(2) *Forecasting by the chain principle*

We shall consider the case of a stationary unirelation model M which we specify by the representation

$$(82a\text{-}b) \qquad y_t = \beta_0 + \beta_1 y_{t-1} + \beta_2 y_{t-2} + \ldots + \upsilon_t$$
$$= \alpha + \upsilon_t + \alpha_1 \upsilon_{t-1} + \alpha_2 \upsilon_{t-2} + \ldots$$

with

(83) $$E(y_t|y_{t-1}, y_{t-2}, \ldots)=\beta_0+\beta_1 y_{t-1}+\beta_2 y_{t-2}+\ldots$$

which is a special case of the predictive decomposition of y_t referred to earlier in this paper [section 1.4 (4)]. The forecasts $y_{n+1}, y_{n+2}, \ldots$ are obtained by the chain principle, making iterated use of (83). Thus when y_{n+i} has been obtained, y_{n+i+1} is calculated from (83) in the basis of $y_{n+i}, y_{n+i-1}, \ldots$ The variance of the resulting forecasts is given by

(84) $$E(y_{n+m}-a_{n+m})^2=(1+\alpha_1{}^2+\ldots+\alpha_{m-1}^2)\ \sigma^2$$

showing that the accuracy of the forecast will decrease as the forecast span m increases. This last feature is reflected also in the JANUS quotient, inasmuch as (84) gives

(85) $$E(J^2)=1+(1-\frac{1}{m})\ \alpha_1^2+(1-\frac{2}{m})\ \alpha_2^2+\ldots+\frac{1}{m}\ \alpha_{m-1}^2$$

The predictive decomposition (82a-b) has the property that the variance (84) is the smallest possible of all representations of type (82b). This is the fundamental property of *minimum-delay*, established by E. ROBINSON, Ref. 24, and referred to earlier in this paper.

The approach (82)-(85) extends to the general stationary case when y_t allows the predictive decomposition

(86a-b) $$y_t=\Psi_t+\beta_1\ y_{t-1}+\beta_2\ y_{t-2}+\ldots+\upsilon_t$$
$$=\Psi_t+\upsilon_t+\alpha_1\ \upsilon_{t-1}+\alpha_2\ \upsilon_{t-2}+\ldots$$

where Ψ_t is the *deterministic* (also called *singular*) *component* of y_t. The procedure of forecasting first settles the prediction of the deterministic component over the entire forecast range, and then applies the chain principle in the same way as in (82)-(83) to forecast $y_t - \Psi_t$. We see that from the point of view of forecasting, the deterministic component Ψ_t of the model is, in principle, equivalent to an exogenous component.

Furthermore, the approaches (82) and (86) extend to multivariate models; Ref. 46. The corresponding predictive decomposition (82a-b) will then yield a representation in the form of a CC-system. The minimum-delay property extends to multivariate predictive decomposition. Hence the ensuing forecast variance (84) is smaller than in other linear forecasting models, such as ID- or BEID-systems.

The application of the JANUS quotient extends to multivariate systems, and in particular to VR- CC- ID- and BEID-systems. By suitable adaptations the JANUS quotient can test the entire system or a specific behavioural relation. It can also, as illustrated below, be adapted so as to focus on the extrapolation aspect of specific relations by removing those forecasting errors which arise from imperfect forecasting of the explanatory variables.

ILLUSTRATIONS

(1) *Market model for pork, US 1932-1956.* (G. STOJKOVIC, Ref. 20).

We shall consider the recalculated model as given by (36a-c). For the demand relation the fit in the observation range is moderately close,

$$s(v') = 0.69 \ s(d)$$

The JANUS quotient (76) as calculated for a forecast span of three years, treating the explanatory variables as known *ex post,* gives the yearly values

(87) $J_*^{(1957)} = 1.08 \quad J_*^{(1958)} = 0.94 \quad J_*^{(1959)} = 0.68$

and the aggregate value

$$J_* = 0.92$$

The price mechanism gives a similar fit in the observation range,

$$s(\upsilon'') = 0.58 \; s(p)$$

Here the extrapolatory JANUS quotient (76) gives

(88) $J_*^{(1957)} = 0.69 \quad J_*^{(1958)} = 0.91 \quad J_*^{(1959)} = 0.02$

and the aggregate quotient

$$J_* = 0.66$$

Using the three relations of the system (36) to generate forecasts by the chain principle, and applying the JANUS quotient (75) to the ensuing forecasts for price p_t, the material gives the following quotients

(89) $J^{(1957)} = 0.36 \quad J^{(1958)} = 1.56 \quad J^{(1959)} = 0.04$

and the aggregate

$$J = 0.92$$

According to (80) and (85) the JANUS quotients (89) could be expected to be higher than in (88), but the forecast refers to only one sample series and here it so happened that the tendency did not materialize very clearly.

(2) *Macroeconomic quarterly model of UK, 1946-1956.* (KLEIN-BALL-HAZLEWOOD-VANDOME, Refs. 37-39).

As applied to a model like (36) for a sector of an economy, a predictive test of type (75) or (76) is of course of limited relevance because the sector is liable to exogenous influences that may disturb or upset the model and thereby the forecasts. So much the more relevant is a predictive test of economic models that comprise the economy of an entire nation, although here too there are external disturbances on the international plane. The following figures report briefly an attempt to apply the JANUS quotient to the abovementioned model for UK 1946-1956. The model has not been published in such form as to give the theoretical values obtained from the model in the observation range; hence the quotient has only been calculated for three variables, and is partly based on reading off the graphs of the residuals. Since the tests refer only to a small fraction of the model it need not be emphasized that the figures are only given to illustrate the technical procedure of the predictive test [26].

Year	Quarter	Janus quotient	Industrial production P	Price index of final output p	Interest rate r
1957		J_*	0.46	2.4	1.45
1958		J	0.21	8.3	0.90
1959	(1)	J_*	1.35	4.2	—
»	»	J	0.20	7.0	—
»	(2)	J_*	2.85	2.8	—
»	»	J	3.42	7.0	—

The various generalizations and adaptations of the JANUS quotient focus on just one aspect of forecasting accuracy, na-

(26) My thanks are due to Miss INGRID AGERHOLM and Mr. K. NAZIMUDDIN for assistance in the computations.

mely the relative size of deviations between theoretical and observed values. It goes without saying that this is a serious limitation, and that the JANUS quotient therefore by no means is a panacea in the testing of forecasting accuracy. Specific reference is made to the importance of paying special attention to turning points in the phenomena under analysis. To quote a wellkown example from meteorology, Ref. 47, the use of digital computers in short range forecasting was tried out on a cyclone which on Thanksgiving Day 1950 swept the US continent in a wide and softly curved swing, and when coming to the Atlantic made a sharp turn northwards through New England. It was no difficulty to simulate and forecast the wide swing on the computer, but for a considerable time all of the trial models led the forecast path of the Thanksgiving Day cyclone right out into the Atlantic, and it took a qualified combination of meteorological thinking and data compilation to construct a model that reproduced the sharp turn.

3.3. *Overfitting.*

In the nonsense department of statistical method everybody has seen the pitfall of *overfitting* — the situation when a model gives illusively close fit to the given data because the available observations are outnumbered by the parameters. A case in point that is actually on record is the time series analysis of a sea level, in which study 122 annual data were graduated by a sum of 40 sinusoids with different periods, phases and amplitudes. The resulting fit in the observation range was very very close, and the forecast for the next year was included in the report. Just as the report was published the next observation emerged, showing an ample deviation from the forecast. The comment of the author was that unfortunately he had forgot to include the 41st component.

The parameter estimation of ID- and BEID-systems by the two-stage method of least squares sets the trap of overfitting in a new disguise. The transformation (22a) tends to carry all parameters of the entire primary form into each relation of the reduced form (27). Thus if there are some 3 or 4 parameters in each behaviour relation of the primary form, and the primary form involves some 50 behaviour relations, each relation of the reduced form will involve some 150 parameters, far more than the number of observations usually available. Well to note, I am not saying that the model builders walk straight into the pitfall of overfitting. On the contrary, from the beginnings of the theory of ID-systems it has been a rule to specify each relation of the reduced form as involving all predetermined variables of the entire system, and the ensuing dangers of collinearities in the empirical parameter estimation have been recognized for a long time (28). The specific point I wish to make is that the risk of overfitting is tangible already with a modest number of variables in the reduced form, owing to autocorrelation and inertia effects in most or all variables of the system. Such overfitting will blur the distinction between an ID-system and the corresponding BEID-system, with risk that the resulting parameter estimates will be biased. To assess and evaluate the bias by an aprioristic analysis is extremely difficult or — at least in practice — impossible, because it leads into overwhelming complexities even for forecasting systems of moderate size. The approach of a predictive test will however reveal the overfitting, and thereby the test will also reveal the difference between the ID- and BEID-systems and the ensuing bias in the parameter estimation.

(27) Explicit illustrations are given in Ref. 13, p. 484, and with more detail in Ref. 27.

(28) The difficulties at issue were amply emphasized by Professor D.W. JORGENSON in the oral presentation of his report, Ref. 48, to the Copenhagen meeting of Econometric Society, July 1963.

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DISCUSSION

FISHER

As always, Professor WOLD has given us an interesting paper. My remarks are in the nature of supplements.

In the first place, I am interested in the case of bi-expectational interdependent systems. As Professor WOLD has shown, in this case the elasticity of demand for example becomes elasticity in terms of expected rather than actual price, where expected is interpreted as the expected value of price given by the reduced form equations. Now there is of course another sense of expectation in economics; that is the sense in which a variable is expected by people who make a decision based on it. An interesting question, it seems to me, is under what circumstances and in what types of models this will in fact be the same as the expected price given from the reduced form. Only in such circumstances will it be the case that elasticity with respect to expected price in fact is a meaningful parameter which describes interesting behavior. I suspect that the two coincide in a rather general framework. There is in the literature a hypothesis known as the rational expectations hypothesis due largely to JOHN MUTH, according to which it is assumed that the decision makers who are being studied expect the values of the relevant variables to be those on the average which will be predicted by the model. MUTH has shown that this is not logically circular. Further, in this context it sounds like the sort of behavior which would lead decision makers to expect that price predicted by the reduced form.

Secondly, it is of course possible that Professor WOLD's J^2 can be less than 1. This can happen if structural change takes the form of only a small change in the parameters but a large downward change in the variance of the disturbance terms.

My next point is that Professor WOLD has been careful to avoid an error which is occasionally made in the literature, namely that least squares provides an unbiased forecast of the dependent variable. That is false unless the least squares parameters are themselves unbiased and this is not the case if, for example, a lagged dependent variable appears on the right-hand side of the equation. Professor WOLD, however, has not said this although he has said something which sounds like it. What he has said is that least squares is a consistent estimator of an unbiased predictor and this, as he has shown, is true.

Finally, it seems to me that Professor WOLD is unduly worried about what he calls the danger of over-fitting in the reduced form. The circumstance in which the reduced form cannot be estimated by ordinary least squares because there are too few observations relative to the number of exogenous variables in the model, is quite a common one in dealing with large econometric models. This is not of great consequence as a fundamental matter, however, because one then uses intrumental variables methods which drop some of the exogenous variables for purposes of estimation. No difficulty of principle arises, although there is then a problem of how one ought to choose the instrumental variables to be retained. This is a question which I cover in my paper.

THEIL

1. Regarding the difficulty of over-fitting in the reduced form, Messrs. T. KLOEK and L.B.M. MENNES formulated a procedure (in a recent issue of « Econometrica ») which is designed to handle this problem, which is indeed serious when the number of predetermined variables is not small compared with the number

of observations. Their procedure amounts to replacing predetermined variables by a certain number of their principal components. The specification of that number remains an arbitrary choice.

2. I like the idea of the JANUS quotient. Its application requires that the forecasts are generated by some kind of probabilistic model, e.g., a regression model. It is therefore not applicable when there is no such model, e.g., when we wish to determine the accuracy of entrepreneurial investment forecasts derived from an investment survey.

3. As to your bi-expectational procedure, I would like to suggest that you subtract the reduced-form disturbances, not only from your right-hand dependent variables, but also from your left-hand dependent variable . Doing so, one finds that there is no disturbance left in the equation at all, because all random parts are removed.

WOLD

The comments by Professors FISHER and THEIL reflect that simultaneous equations as an area of research cover a wide range of theoretical and applied problems. The discussion is mainly oriented towards the general foundations of the approach. Specifically, the following aspects are referred to:

1) The rationale of the approach from the point of view of economic theory, probability theory, statistics, and the theory of knowledge. Hereunder, much of the discussion is concerned with:

   a) Predictive aspects of simultaneous equation systems;

   b) Causal aspects of the systems.

2) The statistical estimation of the parameters of simultaneous equations.

The discussion further reflects that the theory and application of simultaneous equations is in rapid progress along several lines of development. Hence the research situation was not quite the same during the Study Week as now ten months later when the replies are edited. In my replies I shall stick to the notes and tape record from the round table discussion; when reference is made to later developments, they will be made by way of footnotes (1).

With gratitude and satisfaction I note that the discussants of my paper have to a large extent been concerned with my approach of defining interdependent systems in terms of conditional expectations; Refs. 12, 13, 30. The ensuing approach of bi-expectational interdependent systems is in an early stage of development, and so much the more I welcome a thorough scrutiny of its foundations and implications. For easy reference in my replies, let an interdependent system be written:

$$\text{(A)} \qquad y = \beta\, y + \Gamma\, z + \varepsilon$$

when defined in accordance with the classic assumptions of the approach, and

$$(\mathrm{B}_1) \qquad y = \beta\, y^* + \Gamma\, z + \varepsilon^*$$

with

$$(\mathrm{B}_2) \qquad \varepsilon^* = \varepsilon + \beta\,(y - y^*) = (\mathrm{I} - \beta)^{-1}\, \varepsilon$$

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(1) Reference will be made to the following two papers:

(a) L. R. KLEIN, *Problems in the estimation of interdependent systems*. Forthcoming in the « Transactions des Entretiens de Monaco 1964 »; Centre International d'Etudes des Problemes Humains, Monaco.

(b) H. WOLD, *A fix-point theorem with econometric background*. Forthcoming in « Arkiv f. Matematik ».

and

$$(B_3) \qquad y^* = E\,(y|y^*,\, z) = \beta\, y^* + \Gamma\, z$$

when defined as a bi-expectational system. We see that an interdependent system when written in the classic form (A) has the same numerical parameters as the corresponding bi-expectational system (B). Hence the problem of parameter estimation is precisely the same for the two versions of the model.

Professor FISHER's reference to the pioneering work of J. F. MUTH, « Econometrica » 29 (April 1961), is greatly appreciated. MUTH's hypothesis of rational expectations opens up vistas towards highly fruitful syntheses by assuming that expectations in the economic-psychological sense are in the first proxy equal to expectations in the sense of probability theory. In the context of my paper, the conceptual distinction between the two notions of expectation is referred to on page 32. To simplify matters in a first approach, MUTH considers market models of the cobweb type and assumes that they are deterministic except for the supply relation, and he makes a most interesting comparison with other theories of economic expectation in the realm of cobweb models. The illustration in terms of cobweb models makes a point of contact with my own studies; see especially Ref. 30. The contact is tangential, and there is little or no overlapping between the problems under analysis. MUTH has explored the models with regard to economic-psychological expectation, and economic-psychological vs. probabilistic expectation; my own interest has focused entirely on the rationale of probabilistic expectation, and in particular on the general rules for operating with probabilistic expectations.

Professor FISHER comments that the coefficient of an expectational variable is a meaningful parameter « only » in case there is no difference between the economic-psychological and the probabilistic expectation. I have here put « only » within quotation marks because I think the statement is somewhat too strong. Specifically, another case I am thinking of is when we use current GNP as an endogenous explanatory variable y_i but are aware that the statistical assessment of GNP is not quite adequate in the explanatory context of the model; it may then be meaningful to use the expectation y_i^* as explanatory variable, in the hope that it gives a better proxy to the nonobserved explanatory variable than the observed value of GNP. Another potential application that comes to mind is that if y_i is individual consumer income, y_i^* might serve as a proxy for permanent income in the sense of M. FRIEDMAN's well known theory.

In specifying the subject matter content of his models J. F. MUTH makes use of causal notions, and the expectational variables enter both as causal factors and as effect variables. His use of causal notions makes for a general affinity with my own work, which to a large extent has been concerned with the much debated questions that arise if we wish to provide a causal interpretation for the relations and individual parameters of interdependent systems. When an interdependent system (A) is respecified by way of (B_1), this transition makes for a clearcut interpretation of the expectational variables y_i^* as causal factors, and from (B_3) we see that the variables y_i^* will also play the part of effect variables. It will be noted that J. F. MUTH's model is not quite in accordance with the bi-expectational framework (B_1)—(B_3), for he specifies the demand relation as deterministic by not including an error term, and in the customary manner of cobweb models he treats the demand relation as causally reversible by taking current demand to determine current price. It would seem however that MUTH's line of argument only requires some slight qualification to be in accordance with the bi-expectational form (B_1)—(B_3) of interdependent systems.

Professor FISHER's comment on J^2 is certainly to the point, and it brings in relief that the simple proxy $J^2 \sim 1$ primarily refers to the case of stationary deviations from the theoretical model.

I appreciate very much that Professor FISHER emphasizes a pitfall about least squares: If a theoretical relation is an *eo ipso* predictor, it can be consistently estimated by least squares regression, but it does not follow that the reverse is true (that is, if least squares regression is chosen for the estimation of a specified theoretical relationship, the choice of estimation method will not make this relationship an *eo ipso* predictor). I take the opportunity to emphasize the truth of a related negative statement: If least squares regression is not a consistent estimate of a specified theoretical relationship, then this relationship is not an *eo ipso* predictor.

Coming to the last paragraph of Professor FISHER's comments, I am afraid it reveals rather deepgoing differences between our views. As regards the dangers of overfitting, they are certainly a real headache (see e.g. the comments by H. THEIL, Ref. 33, section 6 D), and the trouble does not become less real because it is « quite a common one in dealing with large econometric models. » (2)

As to the approach of instrumental variables, this is a surrogate of an *ad hoc* nature, inasmuch as the instrumental variables are not specified a priori in the model. It remains to be seen whether the results of the approach are as a rule good enough to pass the test of confronting the ensuing forecasts with actual evidence by way of predictive tests. Hoping for the best, I have no desire to disencourage; all I want to say is that this is one of the open questions in the present stage of development.

In reply to the first point made by Professor THEIL, I am confident that KLOEK-MENNES' adaptation of the principal components approach goes a long way to overcome the difficulty of overfitting and related headaches in the statistical estimation of interdependent systems. The approach has the nature of a shortcut, however, and obviously it runs the risk that in sieving forth the principal components of the predetermined variables z_i it may throw away one or more z_i's that contribute relatively little to the total variability of the predetermined variables, and yet are highly important as

(2) Professor L. KLEIN in the paper (*a*) referred to in footnote (1) strongly emphasizes how troublesome the current techniques for parameter estimation of multi-relation models are in the present stage of development. To quote from the Monaco discussion of Professor Klein's paper, the results he reports from the estimation of a 12 relation model give clear evidence of overfitting, inasmuch as most of his OLS (ordinary least squares) and TSLS (two stage least squares) estimates coincide up to the third figure.

sources of variability of the endogenous variables, the variables which the system has for purpose to explain.

As to the second point, I feel sure we see the J^2 quotient in the same light, inasmuch as it is only a slight modification of Professor THEIL's coefficient of inequality, J^2 being more similar to the Fisherian F ratio. The J^2 is designed to exploit the information obtained when a model is confronted with past observations on the one hand, and future comparisons between forecasts and actual developments on the other, and it is clear that if there is no model such information is not available.

In his third and last point, Professor THEIL expresses relation (B_3) in words. We note the sharp contrast relative to model (A), which in general implies

$$\text{(C)} \qquad E\,(y|y,\, z) \neq \beta\, y + \Gamma z\,.$$

The respecification (B_1) - (B_3) gives a clearcut answer to those questions which I have seen as obscure issues in my studies into the rationale of multi-relation models. I would fain to repeat that the respecification (B_1) - (B_3) dates only from a few years ago; see Ref. 13, Theorem 10, also Ref. 12, remark 3.2.26. In a first phase of my studies, Refs. 23, 34, a main theme was the comparison between recursive (also known as causal chain) systems and interdependent systems (A), a key point being that while recursive systems have a form that is directly amenable to a causal interpretation of behaviour relations and individual parameters, this is not so for interdependent systems (A). This first phase includes the joint attempt with R. STROTZ, Ref. 18, to provide a causal interpretation for the individual parameters of interdependent systems (A). In a second phase, Refs. 27-30, my approach was to specify the models in terms of conditional expectations, called *eo ipso* predictors, or briefly predictors. One type of model considered, called CCC (conditional causal chain) systems, was a straightforward respecification of interdependent systems (A) into predictor relations, giving

$$\text{(D)} \qquad E\,(y/y,\, z) \neq \beta\, y + \Gamma z\,.$$

Unlike (B), the respecification (D) in general involves a change of the numerical parameters β, Γ of the system. It would seem that the respecification (B) is more fruitful than (D). Respecification (B) is however so recent that only some of its implications have been explored (³).

ALLAIS

I wonder if the distinction between the three cases you have denoted as « vector regression, causal chain, interdependent system » corresponds to a real difference from the point of view of the facts. What we observe in nature is continuous. Thus if we consider discrete series instead of continuous series, we introduce something which is handy for the calculation, but something which does not correspond to reality and which may result in the artificial creation of a certain number of difficulties.

HAAVELMO

I enjoyed listening to Prof. WOLD's paper. I think it was extremely clear in its presentation. I only have one comment — about something that bothers me a little bit. When he says to use this or that method of approach, does he mean to use this or that

(³) Respecification (B) provides a new approach for the statistical estimation of parameters in interdependent systems; see reference (*b*) in footnote (¹).

statistical method to handle a certain economic theory? Or does he mean that he is talking about methods of constructing a certain kind of theory? I think many of us consider the construction of an economic theory, including the specification of stochastic elements in the theory, as one thing, and to confront it with the facts as something different. If we have constructed a stochastic economic theory based on certain principles of economic behaviour, we have a model which can be interpreted as an indirect specification of the joint probability law of the observable economic variables. From such a model we may derive various kinds of « relations », expected values, and other kinds of statements concerning the properties of this joint probability law. It is perhaps unfortunate that we talk about relations between economic variables, when actually we should regard our economic theories as just indirect ways of saying something about the probabilistic aspect of the variables we are talking about. What do we actually mean when we say that a model or theory fits the facts? Do we mean that it fits the facts as these will be if the economy is not disturbed by changes in economic policy? Or do we mean that the model would fit the facts as these would be after a new kind of policy? The meaning may be one or the other, depending on what we are after. Moreover, a model that fits the facts in the first sense may often be used as a basis for extracting information concerning a model that fits the facts in the second sense. Some of the parameters to be estimated may be the same in both cases. But ultimately, we may not be so much interested in the joint probability law of the economic variables as it has been in a past period, our final interest may be related to a future joint probability law which has been modified by certain economic-political actions. From this I think we may draw two conclusions, first that it is extremely important to specify the economic meaning of a theory or relation and, secondly, that closeness of fit for forecasting purposes is by no means a decisive feature of a « good » economic theory.

WOLD

Professors ALLAIS and HAAVELMO comment upon econometric models from the point of view of epistemology, the general theory of knowledge. The main point I wish to make in my reply is that this is a highly important aspect of econometric model building. Several of the much debated issues about econometric models are not specific to econometrics, they are fundamental issues in the social sciences by and large; and many of the econometric techniques are pioneering in the still wide realm of nonexperimental model building.

As to the first part of Professor ALLAIS' comment, I would like to emphasize the sharp distinction between on the one hand the hypotheses that constitute the theoretical part of a model, on the other hand the actual facts that the model serves to explain. There is always a pluralism of models, and there is never perfect agreement between a model and the facts as actually observed. The choice between different types of model is to a large extent a matter of economy of thought, to quote ERNST MACH. More specifically, it is often a matter of choosing the model that uses the smallest number of parameters when representing the facts. I do not think that at this point there is really any difference between our views. And similarly with the last part of Professor ALLAIS' comment. Experience has shown that it is sometimes (and in economics quite often) convenient and useful to approach the reality by way of models where time is a discrete variable, but if it turns out that if discrete time gives rise to difficulties, then of course we should respecify time as a continuous variable.

Professor HAAVELMO's comments and five question marks have direct bearing on the epistemological foundations of econometrics; more specifically a bearing upon econometrics as nonexperimental model building. This is a most important topic for our round table discussion, and I welcome his remarks so much the more as they give me an opportunity to state agreement all through, except perhaps on one point which I think requires some qualification.

I gather that the two first question marks are largely rhetoric, and in any case I agree with the answer he gives in the next sentence. To paraphrase, the construction of economic theories is one thing (namely, the theoretical part of model building), and to confront it with the facts is something different (namely, the empirical part of model building). Thus at this point I would only emphasize more clearly that although the theoretical and empirical aspects of model building should be kept distinct, at least in principle, we should always keep in mind that a fullfledged scientific model is a synthesis of theoretical and empirical knowledge.

As to the second group of Professor HAAVELMO's question marks, it seems to me that they will get clearcut answers if the model builder has taken care to specify in not too vague terms for what broad array of facts, applications, his model is designed to be valid, and if the applications include policy making he should specify what changes in policy, if any, it is the purpose of the model to cover. If not for anything else, such specification is essential when it comes to the verification and testing of the model. Furthermore, the spectrum of potential changes of policy is extremely wide, and the substance of a model would in many cases become too diluted if the model tried to cover more than a relatively small sector of potential changes. For example, a relation of consumer demand may remain the same under very different regimes of economic policy, whereas many other parts of economic life are quite susceptible even to small changes of policy.

A more specific reply to the last sentence of Professor HAAVELMO's comments is that in case an economic forecasting model influences government policy, this creates a feedback problem which in principle belongs under the construction of a more comprehensive model that includes the interaction between forecasting and policy. Feedback phenomena may be more or less difficult to handle, but even if they are difficult they do not make model building impossible. For example, feedback models are commonplace in the theory of servomechanisms.

Since the argument about a change in policy has been in frequent use in the debate on the rationale of interdependent systems, I should like to comment a little more on this type of application. Nonexperimental model building cannot be based on the results of controlled experiments; speaking generally, the empirical basis of the model is instead some kind of regularity in the observed phenomena, regularities that the model builder tries to explore and explain by his model. In presenting his model, he should broadly specify these regularities as the intended domain of validity of his model. And if a scientific model is to be used for forecasting the results of a change in economic policy, the observed regularities should include some evidence from earlier changes in policy. There is here a fluid border between science and politics. Several aspects of science and politics have come to the fore in other sessions of our Study Week. The only point I wish to make in the present context is that politics has other social functions than science, and therefore political activity can never be completely rationalized as an application of scientific model building.

Coming finally to my point of disagreement with Professor HAAVELMO's comments, it lies in his broad statement that a stochastic economic model is nothing else than a joint probability law, and he even goes as far as to put between quotation marks the « relations » that can be derived as properties of the joint probability law that constitutes the model. True, the stochastization of deterministic models is a key development in modern econometrics, and in this connection I was nearly to say that the part played by joint probability laws in the specification of nonexperimental models cannot be exaggerated — but the point I wish to make is just that Professor HAAVELMO's statement is such an exaggeration. Joint probability laws can express much, but they cannot express everything. They are symmetric in the variables involved, and as such they cannot express asymmetric features of the model, and in particular they cannot express causal relations that enter as part of the model, for causal relationships are directed (from cause to effect) and thereby asymmetric. Causal relations in general are directed and asymmetric both in deterministic and stochastic models, and the stochastization of a deterministic model brings the asymmetry in further relief; in fact, as illustrated by (B_3), in a stochastic specification of the model the causal relations are expressed in terms of predictors, that is, conditional expectations, and conditional expectations always are irreversible, and thereby asymmetric.

ALLAIS

To express myself more clearly may I comment briefly using an example? I recently studied hyperinflations. My formulation was a continuous one but for simplicity I used only monthly data. It was therefore impossible to represent the last months of the hyperinflations correctly, and with monthly data the conclusion would have been that the assumptions made were incorrect. But when weekly data were considered the verification of the model was very good and the hypothesis confirmed.

Thus my conclusion is that some models can introduce artificial difficulties which could otherwise have been avoided.

WOLD

Yes, this is surely an illuminating example to show that the choice of time period is an important element in the specification of hypotheses in a model. But it does not really bring home the previous point about continuous time, for monthly data are discrete in time, and so are weekly data. From the theory of stochastic processes it is easy to give examples of problems that are easier to handle in discrete time than in continuous time.

HAAVELMO

Let $P(x, y, \beta)$ be the joint probability law of the variables x and y in the past, or under one kind of economic regime. β is a parameter. Let $P^*(x, y, \beta)$ be the corresponding probability law for the future, under a new economic regime, β being *the same parameter* in both cases. Then the link, the essential element of invariance, as between the past and the future may be the value of β rather than e.g. the expected value of y for given x, the latter relation depending on the form of the probability laws P and P^*.

KOOPMANS

In the discussions comparing inter-dependent systems and other systems, one element has been important which I have not heard Professor WOLD mention in this summary. This is the idea of the autonomy of individual equations of the interdependent systems.

My question is whether the concern with autonomy can be conserved when we go from the ID to the BEID and alternatively when we go from the ID to the CC system.

WOLD

Since Professor KOOPMAN's question is closely related to Professor HAAVELMO's comment, I shall reply to them jointly.

Professor HAAVELMO's clarifying example has the advantage that it is so simple that there could not possibly arise any misunderstanding about the mathematical aspects. Yet there are at least three possible interpretations of the example to consider.

1) Parameter β is symmetric with respect to the variables x, y; for example, β is the correlation coefficient of x and y. This is the case I thought of in the first place when finishing my previous reply to Professor HAAVELMO by a critical remark. Parameters that are symmetric in this sense make a conceptual category that is far too narrow to represent all meaningful parameters. For example, a demand elasticity with respect to price, or an interest rate, do not possess this kind of symmetry.

2) Parameter β is a demand elasticity, an interest rate, or some other parameter that typically enters as a coefficient for one of the explanatory variables in an economic relationship. In the simplest case we have

$$y = \beta x \,. \tag{E}$$

It is unclear to me what Professor HAAVELMO means when he says that β can be an essential element of invariance rather than the expected value of y for given x, for what could the right hand member of (E) be assumed to give if not just the expected value of y for given x?

And in the last two lines of his comment I am afraid Professor HAAVELMO is not only unclear, but actually mistaken. It is easy to give examples where (E) gives the expected value of y for given x, and the relation is an invariant that does not depend on the form of the probability laws P and P*. It is even so that this kind of invariance has been exploited for assessing the direction of a causal relationship (a first approach of this type was initiated by H. WORKING in 1934; see Ref. 12, section 6).

3) In the theory of interdependent systems (A) it is a characteristic feature that the behavioural relations are dealt with as being reversible with regard to the current endogenous variables y. This feature being in sharp contrast to the irreversibility of ordinary regression relations, I have many times voiced scepticism about interdependent systems on this basis (see e.g. Refs. 23, 34). Now the respecification (B_1) goes some way to clarify the situation. The reversibility at issue requires that if we rearrange the current endogenous variables by shifting two or more of them from the one side of the relations to the other, then the rearranged system should satisfy the corresponding relations of type (B_2). On the classic as-

sumptions of interdependent systems, this requirement will actually be fulfilled (¹).

I welcome very much Professor KOOPMANS' questions about the notion of *autonomy*. Prof. FRISCH's original concept of autonomy refers to an economic feature, for example a parameter β, that remains invariant when other things change; this concept is closely related to the notion of invariance as formulated in Professor HAAVELMO's previous comment. Professor KOOPMANS refers to autonomy in a related sense that emphasizes the model aspects, a relation being called autonomous if it can be broken out of a model and inserted in some other specified model. In such autonomous relations the parameters could be called autonomous in the sense of the above points (2) and (3). As regards the argument about a change in policy, the autonomy refers to the case when the model builder uses different models before and after the change.

In reply to Professor KOOPMANS, it is my understanding that the notion of autonomous relations is highly relevant for the theory of multirelation models in general, and in particular so for ID, CC and BEID systems. The difference in approach may perhaps call for some slight modification, *mutatis mutandis*, depending upon what type of model we are considering. Thus for ID systems, the autonomy of a behavioural relation would require that it remains the same if it is broken out and inserted in another system which includes those current endogenous variables that enter as explanatory in the autonomous relation. For CC and BEID systems the concept of autonomy might well be generalized somewhat so as to require only that the residual-free part of the relation remains the same when it is inserted in some other model. This last remark emphasizes the point I wish to make in (2), namely that the invariants of primary importance in non-experimental model building are directed predictors such as (E) rather than joint probability laws.

(¹) This is however not the whole story, for it turns out that the classic assumptions constitute a special case that covers only a subspace of lower dimension than the entire parameter space; see reference (*b*) in footnote 1.

LEONTIEF

The structure of an analytical system which one decides to use in any particular instance must to a very large extent depend on the type, that is, amount and accuracy of factual information actually available for its implementation. Refinements of statistical methodology are often wasted on all too crude raw material of primary data.

FRISCH

I have been listening with interest to the discussions in a field in which I took great interest some yars ago and which I think I was at that time qualified to speak. Today I do not think I am qualified, quite to the same extent, but since I have been called upon let me just give one example which is connected with what Professor LEONTIEF said right now. Suppose for a moment that you have a curve whose mathematical form you actually know. It is for instance a second degree parabola. For a moment you forget this mathematical knowledge and you look for some *numerical data* to determine the tangent at a specific point A. You have observation in the vicinity of this point. Let each of these observations be affected by an error of measurement. In order to have actually the *tangent* (not the secant) in the point considered, you want to use observations that are as close as possible to the point A. If you really want the *tangent* (not the secant) at this point you would have to creep up to the point A as close as you can. But in so doing you expose yourself more and more to the inaccurancies involved in the numerical observations and if you get two points that are *very* close to A you get something absolutely absurd. So you have to make a compromise between giving up a little of the ideal you are looking for, i.e. the tangent instead of the secant, you have to do it in order to get a method which is more robust. You must make a compromise, and a practical man will understand more or less intuitively how far he should deviate from the mathematical ideal in order to have a result which is useful for his purposes.

ALLAIS

In fact, I agree completely with what Professor FRISCH said; but my point was different. It was that for the discussion and for the analysis of the difficulties we meet we must distinguish between real difficulties and artificial difficulties which arise only from the consideration of discrete series. That is quite different.

WOLD

This last group of comments refer to shortcomings of model building that arise because the empirical observations for some reason or other do not match the theoretical model.

Professor LEONTIEF very rightly emphasizes that there must be a sound balance between the accuracy of the statistical observations and the degree of refinement of the statistical methods applied. A caution in the same vein is that the application of refined statistical methods should not become an end in itself, and thereby become futile. Or « sieving moscitoes, but swallowing camels », as the Swedish proverb goes, the moscitoes being sampling errors that are reduced by refined techniques but tend to zero anyway in large samples, whereas the camels of specification errors are ignored although they are finite entities that do not tend to zero with increasing sample.

Professor FRISCH very instructively points out a crucial feature in the transition from deterministic to stochastic models, namely that when it comes to differentials versus finite differences, it is often easier to work with differentials if the approach is deterministic and with differences if it is stochastic.

ECONOMETRIC ANALYSIS FOR ASSESSING THE EFFICACY OF PUBLIC INVESTMENT (*)

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In my country and in most countries of the world the role of government activity in the economic sphere has become increasingly important during the last few decades. We no longer grant even lip service to the doctrine that « That government is best which governs least ». Governments are expected nowadays to intervene vigorously in economic affairs in the interests of general prosperity and economic advancement.

A major part of this enhanced, or at least more candid, concern with economic development on the part of governments has been an increase in the importance of government-operated enterprises and, consequently, of government investment. Today, socialist countries apart, we are all mixed economies.

For this reason, and for a number of others about which I claim no particular competence, the process of deciding on government investments has become increasingly self-conscious. In the good old days when some local or special need or opportunity made its appearance the legislators debated the

(*) This paper was written in conjunction with research sponsored by the Corps of Engineers, U.S. Army and Resources for the Future. They are not, however, responsible for any of the opinions expressed in it.

possibilities and might appropriate the funds for the requisite investments. Beginning in the 1930's in my country, and at other dates elsewhere this process has become increasingly formalized and bureaucratized. Now a project proposal must be accompanied by an elaborate dossier setting forth estimates of its various economic and non-economic consequences and purporting to justify it by appeal to objective criteria. If the American Congress or other local legislature doesn't insist on such a formal justification the World Bank will. We have even extended the range of activities counted as government investments. Expenditures on education, for example, are now frequently considered under that heading.

It is therefore appropriate for us to consider the application of quantitative methods of economic analysis to the appraisal of proposed public investments. We shall assume throughout, as a background, a mixed economy, one in which the public and private sectors are both significant. The issue of public investment cannot arise, by definition, in a pure private economy, if there is one anywhere in the world. It takes an entirely different form and meaning in a pure public economy. Thus we shall assume a mixed economy, and though we shall concentrate on the public sector the private sector will always be visible in the background.

The broad subject of government investment has already accumulated an imposing literature, which I shall not survey. Rather I shall advance a proposal concerning one central issue that the literature, as I know it, treats unduly lightly.

The analysis of public investments is strongly analogous to the problem of capital budgeting in the context of a private firm. Both are motivated by the need for wise allocation of scarce resources, and similar conceptual issues arise in the two cases. In both cases the benefits promised by the project must be weighed against the costs it entails and competing projects laying claim to the same resources must be compared. But in the case of government undertakings both the benefits and the costs, but particularly the benefits, are far more difficult to ascertain and measure than in the case of private investments. This follows from the very natures of the firm and the government and from the kinds of enterprise that a government is likely to engage in in a mixed economy.

When a firm contemplates an investment it will be concerned very predominantly with the increase in the salable product or the decrease in the operating costs that will result from it. Both of these are readily expressible in monetary terms and the conversion from physical units to economic value is relatively easy because prices for the commodities at issue can be obtained, with some degree of haziness and extrapolation to be sure, from the free markets on which those commodities are traded.

A government's interests and the results in expects from its investments are quite different. Indeed, the greater the extent to which the kind of appraisal that a firm uses is adequate, the more likely is the government of a mixed economy to leave the undertaking to the private sector. The kinds of results in which a government is interested can be outlined as follows:

1. Frequently government enterprises do produce salable products, comparable to those of private enterprises. This is likely to be the case where the product is a « natural monopoly » or is particularly important to the health and safety of the community or to the operation of the government itself.

2. In recent years, especially, governments have entered fields characterized by significant economies of scale, to gain which requires larger commitments of capital than private individuals are able or willing to mobilize.

3. The government is likely to undertake the production of goods for which there is a marked discrepancy between market price and social value, or for which a private enterprise

would find it difficult to collect adequate recompense because of either technological or institutional peculiarities. So-called « collective goods » fall within this category.

4. The gap between social value and market price mentioned above may be due to external economies of many different kinds, which deserve to be mentioned explicitly. One important category consists of external economies of consumption: types of consumption that are deemed to confer benefits on the community over and above those that are perceived by the individual consumer. Education is the leading example, but there are many other instances. Without government action, undesirably small quantities of such goods would be produced and consumed. The appropriate government action may take many forms: subsidization of production or consumption, or direct government provision, with or without charge.

5. Another type of external benefit is the development of economic skills. A government may undertake specific forms of enterprise or enterprise in specific localities in order to promote the growth of technical and managerial skills, i.e. to introduce modern industries into regions that lack them.

6. In emerging economies, the government frequently invests in « social overhead », roads, port facilities, urban housing, and the like. The external economies sought in such projects are reductions in operating costs in the private enter prises that are hoped to follow.

7. Turning, now, from external economies, a frequent motive for public investment is improvement in the distribution of income, either by reducing costs of inputs to impoverished sectors of the nation or by providing consumer goods at low prices to various low-income groups. Such subsidies in kind are often more feasible administratively than direct transfer

payments and are likely to contribute more to general economic advance.

8. Traditionally, of course, governments have always invested largely in the facilities required by their own minimal functions of national defense, maintenance of law and order, administration of justice, revenue collection, and the like.

9. Finally, and again this is a recent development, governments invest in enterprises intended to enhance the prestige of the nation. Atomic power plants and steel mills are typical examples.

This is a list of motives, undoubtedly not complete, that induce governments to undertake investments. It is also a list of the considerations that must be applied to any government investment, because it is a rare project indeed that contributes to only one of these objectives.

Thus the appraiser of a government project, as contrasted with the appraiser of a private one, must be concerned with many kinds of consequence, not all measurable in monetary units and not all comparable among themselves in any natural unit. Even in dealing with the consequences that are measurable in monetary units, in principle, he is not likely to find that market prices are an adequate guide, partly because of the prevalence of unpriced external effects and partly because consumers' surplus (a treacherous concept that cannot be avoided here), though not reflected in the markets, is important to governments.

There are similar complexities in the consideration of costs, though not as severe. To be sure, a government must recognize, while a private firm can ignore, such external diseconomies as congestion and smoke contamination, but these are usually not of the essence. The government is more likely to be concerned with discrepancies between the market prices and social values of certain factors of production: labor, when there is substantial unemployment, is a famous instance. It is also constrained to take account of the effects of investments on the foreign balance of the nation, and, indeed, the general budgetary position. If financing the project will lead to a more inflationary (or less deflationary) budget, these are external consequences that must be taken into account. Furthermore, there is the problem of the « social rate of discount », an issue I intend to avoid as much as possible. If the social rate of discount differs from the market rate, then the social value of investments that will be displaced by the government investment will be different from the market value. In this circumstance, the appraiser of a government investment is confronted by the problems of estimating how much private investment the project will displace, if any, and of evaluating the social value of this displaced investment. In short, even when it comes to costs, the usual accountants' and engineers' estimates are likely to be unsuitable for economic analysis. Nevertheless, we shall concentrate on the problems posed by benefit evaluation, though some of the more important problems on the cost side will force themselves upon us.

The essential problem on the benefit side is that the benefits expected to flow from a public investment tend to be diverse, non-monetary, incommensurable, and difficult to measure in any units. Several expedients for meeting this problem are available or conceivable. The only one much used in practice has come to be called « benefit-cost analysis », though, as we shall see, it hardly deserves this proud name. The first step in this procedure is to have engineers prepare preliminary designs for the physical facilities and to estimate the costs of construction and the outputs and other physical characteristics of the system. Frequently the engineers will submit two or three alternative designs. The economic analysis, which follows, concentrates in the first instance on the monetary results of those designs. It consists essentially in placing values upon those physical outputs of the system that either have market prices or to which monetary values can be imputed readily.

The estimated benefits of the project are then sum of the values of the physical outputs produced during each year of its life, discounted to the date of inception at the social rate of discount. The costs are the construction cost plus estimated annual operating costs similarly discounted. It will be seen that this procedure is, in essence, very similar to the one followed in capital budgeting by private firms. The only divergence comes now: note is taken of the various nonmonetary effects of the project, frequently referred to as « intangibles ». No attempt is made to incorporate these « intangibles », which may be very tangible indeed, into the analysis: that task is left for higher authority and, in consequence, frequently is never performed. It is a fact of government sociology that as a result of this procedure the monetary effects of a project receive more emphasis in decision making than they should in comparison with the nonmonetary effects, which tend to be slighted because they are difficult to measure and express. Nevertheless, that is how things are with the current state of the art.

The econometrician has two or three suggestions to make for coping with this inadequacy, and the main object of this paper is to consider them. One expedient that I shall not consider is that we attempt to ascertain a full-fledged social utility function in which the various objectives I have listed above enter as arguments. I am interested only in devices that might conceivably be implemented.

The first suggestion that comes to mind, however, comes pretty close to that. It is that the analyst should attempt to establish « shadow prices » for the various objectives, thereby becoming able to compute a value-sum that can be compared unidimensionally with a value-sum of costs or value-sums of benefits from alternative projects. These shadow prices would reflect the willingness of the community to trade off one type of benefit against another. For example, redistribution effects could be incorporated by ascertaining that the community was willing to sacrifice a dollar's worth of national income in order to increase the income of some impoverished group by \$.75. In that case, income to the group to be favored would receive a shadow price of \$1.33.

This proposal may seem visionary, but it contains the essence of all the proposals I am about to make, and, in fact, expresses a kind of comparison that is ineluctable and is made every day. For this reason it may not be as impracticable as it seems at first blush. Since these decisions are made frequently, a study of administrative or legislative records should disclose bounds, at least, on these shadow prices by showing the rates of trade-off between different objectives on projects that are accepted and rejected. Such a study would have to assume that the shadow prices remain stable for reasonable periods. Perhaps they do, but perhaps also they change with changes in the political and economic climate. There is another difficulty, too, for this line of empirical research. Though in every project plan, choices of the sort at issue have to be made somewhere along the line, the form of benefit-cost analysis rather obscures them. As I mentioned, two or three variants of a project at best are submitted for decision by responsible authorities. These tend to differ in many dimensions, and the amount of trade-off between different objectives is not likely to be brought out very clearly. If variant A of a flood-control project provides more protection to property than variant B, it may also cost more, provide less protection to life, and provide better by-products in the form of recreational facilities but less hydro-electric power. Knowing that the legislature has preferred variant A tells little about the shadow prices; though more may be disclosed by inspecting the record of the debate. I emphasize this difficulty because it suggests that the entire process of generating a benefit-cost analysis which I sketched above — the sharp distinction between the spheres of the engineer and the economist, the small number of variants produced — may be unsuitable for analyses of public investment projects even though it serves adequately for private investment decisions.

On the positive side, one should not underestimate how sophisticated government officials and legislators can become in economic matters. It won't do to say they will never understand a technical concept like shadow prices. They already understand very well indeed the parallel concept of a social rate of discount and, though I can't vouch that they use that very phrase, debate it very intelligently. If the concept of shadow prices for different objectives were introduced in any government it would, in its early years, lead a stormy existence. But pretty soon the responsible officials would learn what is at stake and start to debate about what the shadow prices should be rather than what features particular projects should incorporate. This would be a constructive improvement in the decision-making process.

Another proposal that comes naturally to an econometrician is that projects might be designed from the very beginning to meet certain specified target value for the various objectives that they are intended to serve. In particular the designers might be instructed to meet the specified targets at minimum possible capital cost. This would require a revolutionary change in the whole design procedure because, as inspection of the list of objectives suggests, they are not all of the sort that engineers feel at home with. Formally speaking, the task to be imposed is this: An engineering design is a choice of a vector of specifications. Given this vector, estimating the cost of building a structure that meets them is fairly straightforward, though it should be mentioned that engineers do not have a proud record as cost predictors. The prediction of physical results is less straightforward because economic and other nonengineering factors have to be taken into account. If the project produces electric power, its annual output will depend on the load-factor which, in turn, depends on the economic composition of its market. If the project produces irrigation water its usable output will depend on local hydrology and on the relation of the timing of the local hydrologic cycle to the timing of irrigation demand in its market area. And so on. The design typically determines capacity, but economic and social factors determine output.

Be that as it may, the ancillary factors are usually not subject to decision (or if they are they should be regarded as part of the design), so that the economic performance of the project is determined (leaving uncertainty aside) once the design specifications have been settled. We can denote the relationship between design and output by writing $f^i(x_1, \ldots, x_n)$ for the extent of performance with respect to the ith objective or target, where $x_1, \ldots, x_n$ is the design vector. Similarly we can write $c(_1, \ldots, x_n)$ as the cost of meeting the design specifications, $x_1, \ldots, x_n$. In this notation the designers are charged with the task of choosing $x_1, \ldots, x_n$ so as to minimize $c(x_1, \ldots, x_n)$ while satisfying $f^i(x_1, \ldots, x_n) \geq T_i$, $i = 1, \ldots, k$, T_i being the target level of the ith objective, and k the number of objectives considered.

You will leap at once to several objections. One is: where can we find these ambitious production functions, $f^i(x_1, \ldots, x_n)$? On this, I hope that you will be willing to suspend your disbelief; I want to discuss that topic after we have seen what we can do with these functions if we have them. A second objection is: the design specifications do *not* determine the outputs. By varying the way in which a given structure is operated, one form of output can be substituted for another. A third is: how are the various target levels, T_i, to be established? I shall deal with the second and third objections immediately, and more or less together.

Let us reformulate slightly our model of a design problem to create room for inserting some more complicated considerations. Let $F^i(x_1, \ldots, x_n, u_1, \ldots, u_q)$ denote the production or performance function with respect to the ith objective, where, in addition to the previous notation, $u_1, \ldots, u_q$ describe a particular operating policy. For example, if the structure is a school, the u's would specify the average size of class, the number of sessions per day, and the like; if it is a dam, the u's would specify the release rules as a function of reservoir content, etc. The target values, T_i, need not denote deterministic results if the process is stochastic; they should denote parameters of the probability distribution of outputs. For a hydroelectric project, T_6 might be the expected level of power output in June and T_7 might be its variance. The problem is then to minimize $c(x_1, \ldots, x_n)$ subject to $F^i(x_1, \ldots, x_n, u_1, \ldots, u_q) \geq T_i$, $i = 1, \ldots, k$.

This is a standard constrained minimization problem to be solved by any of the usual methods. In general it will be a very difficult problem to solve, but when projects are planned with costs expressed in eight or nine digits, the expense of solving a minimization problem of any imaginable difficulty is as dust in the balance. Indeed, the expense of computation is likely to be insignificant in comparison with the cost of gathering the data.

You will note that though project selection and design are at issue, the determination of operating policy has intruded itself into the problem. This is inevitable, as has long been recognized. « Operations studies » are a standard component of project design work.

Assume this minimization problem to be solved. A by-product of the solution is a set of shadow prices associated with the assigned targets. In the early stages of the work these by-products are the main product of the analysis. They inform us how much costs could be reduced by a one-unit relaxation iu each of the targets. Ratios between them are the trade-off ratios between different objectives. If several projects are being considered simultaneously, discrepancies between their shadow prices for the same objective indicate misallocations and inconsistences in the overall investment plan.

Thus the shadow prices are an instrument for appraising the wisdom of any specification of targets. They disclose one of the major implications of such a specification: the marginal cost of achieving each target. The appropriateness of the assignment of targets can be debated in the light of this information, which is a fruitful improvement over current practice which often requires responsible officials to make policy decisions at the level of design specification (the x_j in our notation). Judgements about the magnitudes of incommensurables, like the diverse objectives of a public investment undertaking, can be made in a more than off-hand way only when responsible officials confront the trade-offs implicit in their decision.

The process here proposed begins with any a priori plausible selection of target levels, which are revised and refined as the marginal costs of achieving them become clearer. This procedure envisages that the design and its objectives will evolve together: the design following pretty mechanically from the objectives; the objectives following from a critical appraisal of the design.

This same model and approach can be formulated in a somewhat different, and instructive, way. The construction costs, which are to be minimized, are a function of the design specifications, but the outputs depend on the operating policy. The role of the design, as far as outputs are concerned, is largely to make desirable operating policies feasible. For instance, one cannot have a policy that calls for dispatching 100,000 kw. of electric power from a plant whose installed capacity is much below that figure. Therefore, it generally (not quite always) fits the structure of the problem best to regard output in each dimension as a function of operating policy alone, and the design as setting limits to the choice of operating policy. The problem then takes this form: Choose design specifications to minimize $c(x_1, \ldots, x_n)$ subject to the constraints

$$\varphi_i(u_1, \ldots, u_q) \geq T_i, \qquad i = 1, \ldots, k,$$
$$\Theta_j(x_1, \ldots, x_n) \geq u_j, \qquad j = 1, \ldots, q.$$

This form, which emphasizes the lack of parallelism between design choices and operating policy choices, has empirical advantages. The major one is that it poses the problem in a manner that technicians find manageable. It asks, « If you had a structure with specification $x_i, \ldots x_n$, how would you operate it and what would the resultant outputs be? ».

This basic formulation has to be modified clearly to fit the particular circumstances of particular projects. For roads, for example, achievement of objectives depends directly on structural characteristics (lane width, maximum grade, etc.) as well as on operating policy (speed and weight limits, level of maintenance, etc.). For reservoirs, structural characteristics will set limits to simple functions of the operating parameters as well as to the parameters themselves. In all these variants, however, the logical structure of the model will remain the same.

The final approach that econometrics suggests to the problem of handling non-comparable benefits is closely related to the second. Instead of meeting specified targets at minimum cost, one can pose the problem of maximizing performance with respect to some one objective, subject to meeting targets with respect to the other dimensions of performance. In this approach the most likely objective to choose for maximization is the discounted present value of the net benefits that have convenient monetary equivalents. Construction cost is likely to enter as one of the constraining targets.

The sacrifice of symmetry in the treatment of objectives is probably more apparent than real, and this approach has some compensating advantages. One advantage is that it reduces by one the number of target outputs that have to be specified in advance of serious analysis. The need to specify a maximum construction cost is only a partial offset; it is much easier generally to make a plausible guess at an allowable construction cost than to guess at an efficient output level. Another advantage is that there is some obscurity about the proper costs to minimize in the cost minimization approach. The most plausible choice is total cost — the cost used in the denominator of a conventional benefit-cost ratio — but this kind of cost does not constitute a drain on any definable scarce resource, it is an amalgam of construction costs in the near future and operating costs extending through the life of the project. The present value of the monetary net benefit stream has much more appeal as the dimension of performance to be singled out for special treatment.

The net benefit maximization approach also leads to shadow prices, which may be even more usefully interpretable than the shadow prices yielded by cost minimization. One of the shadow prices will pertain to the construction cost constraint. This price should surely be approximately the same for all projects that are to be initiated in any brief time period, thus facilitating inter-project comparisons and allocations.

My entire discussion has concentrated on the problem of incommensurable benefits. There are also often incommensurable costs, and these can be handled in precisely the same way: by establishing target levels and revising them in the light of trial results. A few special cases of costs not measured adequately in dollar terms deserve explicit mention. These are all cases in which, perversely, a dollar is not worth a dollar.

Foreign exchange drain is an obvious instance: a country may well be willing to forego more than a dollar in discounted net benefits to save a dollar in foreign exchange. Inflationary budgetary impact is another case. Most public investments require heavy expenditures in some pattern extending over a number of years. Anticipated budgetary tightness or inflationary pressure may well be substantially different in some years in the near future than in others. In such a case, total construction cost is not the only relevant cost figure (even if discounted); separate attention should be given to fiscal drains in individual years. As a final instance, the impact of the financing of a project on the private sector gives rise to a similar distinction. To the extent that the financing induces retrenchment in consumption expenditures there is one kind of cost (one might even want to distinguish among the socio-economic strata which retrench). To the extent that the financing induces a reduction in private investment, there is another kind of cost, especially if the social and private rates of interest diverge. The two cannot be added meaningfully dollar for dollar, nor is it easy to assign an exchange rate between them a priori.

In short then, what I as an econometrician contemplate is that the assessment of a public investment be based on a model of that investment that recognizes its consequences in many dimensions and that exhibits the full range of choice and substitutability among these dimensions. Final adoption or rejection of a project can be decided only when the best design it is feasible to produce is at hand, one that takes account of all the significant dimensions.

This approach requires a new kind of cooperation among engineers, economists, fiscal analysts, and senior policy officials. The engineer is not asked to submit a design as a kind of fait accompli to be analyzed and perhaps accepted by the other officials. He is rather made more integral to the appraisal and decisiou process. He is to collaborate in the formulation of the model of the investment and to present estimates of the requisite functions, where possible. Where that is not possible, a frequent situation, the engineer will have to contribute and analyze some more detailed technological relationships that I shall discuss more fully below. The economist contributes his guesses of plausible targets and estimates of money value for those outputs for which monetary values are appropriate. All contribute to the construction, the testing, the appraisal, and the revision of the model of the contemplated investment. The result of the analysis will be, not a single benefit-cost ratio with some marginal comments, but model of the investment whose performance can be ascertained in respect to any targets that higher policy may dictate.

Is such an ambitious mode of analysis really practicable? I believe it is, and should like to submit some technical suggestions for its implementation.

The critical sticking point is the various performance functions which are very complicated, unknown, and hard to ascertain. In most cases there is a great deal of pertinent technical knowledge, but it is not in the proper form. Consider, for example, one of the more favorable cases: the relationship between the height of a dam and the amount of irrigation water it can provide. In the first instance the height of the dam controls the amount of water that can be impounded in the reservoir behind it. Given any height, a hydrologist can, by studying the contours of the land to be flooded, estimate the the volume of water that can be retained. By making such studies for a number of dam heights he can generate a functional relationship between height and contents. Because of irregularities in contours this is likely to be a very complicated function but, generally, reasonable smooth and simple approximations to it can be found.

But this is only a half-way step because the relationship between reservoir capacity and usable irrigation water supply — « yield » for short — is even more intricate. There are at least two complicating features. One is that the reservoir may not fill annually. As a general rule, the larger the reservoir. the lower the probability that it will fill in any year. Therefore, although usable water supply is an increasing function of capacity up to a point, it increases at a diminshing rate. The other complicating feature has to do with timing. If the annual inflow to the reservoir is concentrated in a rainy season that does not coincide with the time of year at which irrigation water is demanded then, clearly, the contents of the reservoir and the amount of usable irrigation water are the same. That is one extreme. The other extreme occurs when the natural inflow happens to coincide with the period of irrigation demand; then the reservoir may do no good at all, the water would be available when needed even without it. In that case the yield is zero. Of course, zero-yield reservoirs are never built; they are simply a conceptual possibility. Many genuine cases lie between these two extremes, and there are patterns of inflow and demand in which the yield is greater than the capacity. All this is complicated enough, but the case here described — of an isolated reservoir operated for irrigation supply only — is excessively simple. These considerations make it clear that the relationship between height of dam (which costs resources) and usable output is by no means an easy one to ascertain.

Current practice avoids determining this functional relationship. Instead it determines one or two points on it by postulating a specific height for the dam and then estimating the usable yield provided by that height by means of quite elaborate calculations.

The instance of the dam height-usable output relationship is cited to illustrate the complexities hidden in every one of the performance functions required for the analysis of an investment project. There seem to be two ways to proceed and it appears that in practical analysis both should be followed.

One is to impose drastically simplified functional relationships. For example, the relationship between dam height and average reservoir content might be approximated by a quadratic function, and the other relationships would similarly be held to the simplest expressions that state the problem in a meaningful way. This procedure would both facilitate empirical estimation of the parameters required and would simplify the formal problem of finding the optimal values of the design specifications. It would also, of course, be extremely unreliable.

But the purpose of the simplified model is not to produce a decision or a design, but rather to establish plausible ranges of values for the design and operating parameters. When this has been done the second approach can be invoked. It depends on the fact that the elaborate calculations required to obtain a single point on the performance functions can be mechanized and thereby made quick and cheap. Thus although it may not be possible to write out these complicated functions explicitly, or even to manipulate them if written out, it is feasible to ascertain a finite number of points on any of the functions. Even these mechanical calculations may be quite elaborate. In particular, if there is a large stochastic element it may be necessary to simulate the operation of the investment over a substantial period of time in order to estimate a single point on the function. But such simulations are now feasible in the great majority of practical instances and are rapidly becoming cheaper to carry out.

Since it is possible to estimate points it is possible to estimate differences between points, i.e. first derivatives. And therefore it is possible to apply any one of a number of steepest ascent procedures to determine iteratively the solution to any of the problems I discussed earlier. I shall forebear to go into details since the procedures I have in mind are fairly well standardized and improved ones are appearing constantly. All the procedures have the property of producing the shadow prices required for testing preassigned target level along with the solution for each target assignment.

I do not wish to give the impression that the approaches I have suggested solve all the problems of deciding on public investment. Far from it. This is a very difficult field with many, many open questions. To mention only two of the pressing controversial issues, there is the problem of chosing the social rate of discount, and the problem of incorporating uncertainty into decision criteria. Besides, many problems remain to be solved with respect to the kinds of henefits that can be assigned monetary values. What this procedure does is provide a format into which the best current understanding of the conceptual issues can be inserted.

You will have noted that I have dealt at least as much with problems of design as with problems of assessment of efficacy, my assigned topic. This is because these two problems are inseparable: a fair apprisal of a project requires a good design, a good design must be based on the standards to be applied in making the appraisal. In essence, the designer must also be an appraiser; a separate appraiser if there is one, has only an auditing function.

DISCUSSION

MAHALANOBIS

I have a small question. I believe, at the beginning of your paper you point out that we are concerned not so much with a choice of public investment in a completely planned economy but in an economy in which there is both a private sector, and a public sector, and these features raise many difficulties; about this I completely agree. Have the implications of this complicating factor been taken into account in your paper?

DORFMAN

The presence of the private sector is a helpful factor as well as a complicating one. It helps in the preparation of public investment programs and their evaluation by providing a set of market prices for the resources and factors of production needed to construct the public investment projects and also in many cases for the goods and services produced by the public investment. The market prices of the factors required to produce a public investment are, of course, measures of the worths of the goods and services that those factors could produce if they were not devoted to the public investment. Therefore, they measure real cost of the project. The market prices of the goods and services produced by the project measure the worthwhileness of the results. Such valuations are clearly quite essential to a sound appraisal of the worthwhileness of a proposed public investment project, and I think that all project appraisers should be grateful to the private sector for providing them with such values. I know of no other reliable way of obtaining them. I should consider the problem of appraising public investments much more difficult if there were not a private sector that established reliable market prices for the goods and services with which they are concerned.

MAHALANOBIS

Well, your recollection of the paper must be better than mine. If I may make a point here, if you are in a mixed economy, when you are planning investments you would have to take into account the behaviour of the private sector, which you cannot control directly but only indirectly, before making your decisions about public investment. From that point of view, I think the programming of public investment becomes much more difficult than in a fully planned economy in which everything would be decided more or less on mostly endogenous variables.

DORFMAN

Of course the presence of an unpredictable private sector presents the project appraiser with certain difficulties. My impression is that the same difficulties arise in a fully collectivized economy. In a collectivized economy there would not be a private sector to confound and confuse a project designer or appraiser, but there would, of course, be other bureaus and other administrations within the public sector. The project appraiser and planner dealing with undertakings in one branch of a centralized economy, for example, the branch that has to do with irrigation projects, would have somehow or other to foresee plans and responses of the administrators of other sectors of the same economy. Any genuine economy is too large and intricate to be globally planned and globally directed from one center. I think in fact that it may be easier for a project planner to predict the consequences of his plans in a competitive context, such as is afforded by a mixed economy, than to do so where the sectors over which he has no control are other departments of a centralized economy. The problems of economic coordination are always severe, collectivization does not obliterate or solve these problems rather it places them candidly on the doorstep of the planners and deprives them of the assistance of decentralized, on-the-spot, decision makers.

MAHALANOBIS

On the last comment of Professor DORFMAN I should like to say that in a centrally-planned economy you could approximate the competitive equilibrium probably better than you can in a capitalistic economy with many monopolies. In saying this, I am not expressing any political views. I just want to stress my first point about the difficulty which you may face in a private or free economy because it is really not competitive. I agree that if a private or free economy is fully competitive then it has its advantages. I also agree that there are some difficulties for a centrally-planned economy to realize something which would be similar to a good state of competitive equilibrium. It is the question of imperfection of competition which is of concern to me.

DORFMAN

I think that we now understand each other fully. The effectiveness of economic coordination in a mixed economy certainly does depend on the extent of the distortions introduced by monopolies and other deviations from competition in the private sector. If the private sector did not exist, those problems would certainly disappear, but they would be replaced by other serious problems, namely the problems of placing rational valuations on the goods and services dealt with in the economy. In effect, there is no competitive sector in a centralized economy; every sector is monopolized by one government bureau or another. Whether a centralized economy can, in some manner, simulate the operation of a private competitive economy is still open to grave doubt — it has not yet been done.

Whether the difficulties presented by an independent market sector or those resulting from the lack of free market prices are the more serious is an empirical problem that I am not in a position to resolve. I am, however, more impressed than Professor MAHALANOBIS with the inefficiencies that have been experienced when attempts are made to plan an economy without having a frame of reference such as is provided by a system of market prices.

MAHALANOBIS

I was not referring to a centrally-planned economy of the type in which there would not be any market. I was referring to a centrally-planned economy in which everything which a government would like to plan, it could, and everything it would like to leave to a market, it could again do so. I was also referring to the difficulties in an economy in which some particular aspects could not be planned, because of institutional factors.

JOHNSON

There are two comments about the procedure as outlined by Prof. DORFMAN in his ingenious paper. It seems to me that you indicate partly in the manuscript and partly in your discussion that the procedures you outline relate essentially to all types of government investments. By that I mean those government investments where there is essentially a monopoly, on the one hand, and no private individual is engaged in such activities — in most economies — building roads or schools. The questions that I wish to bring up with respect to technique relate to those cases in which the government output is to a degree comparable to private output — public or private electric power and relative to other forms of energy such as natural gas.

And the two points are these: first, though it is true that in making its investments a private firm includes only those returns which it can capture, many of the kinds of returns that come from government investments of the external or social sort where the social and private return diverge, will also exist in the case of private investment. That is, for example, when General Motors builds an assembly plant, there are substantial social gains to the particular community involved — higher property values, various increases for certain other services and so on. And it seems to me that the procedure that you outlined tends to ignore these returns in the case of the private investment and where there is competition between the public and the private sector not really take them into account in comparing private and government investments. If this is true, there is a bias in favour of government investments.

The second point is really along the same lines and this is the question, of how one again creates a real competition between the public and private output, whether it is direct, as in electric power or somewhat indirect, as between electric power and natural gas. How are the taxes paid by the private sector included in the analysis? In some industries, such taxes may amount to a significant fraction of the total value of the output, and here again it seems to me that this is not taken into account, and you again bias the decisions in favour of the government's investments.

DORFMAN

I think that you have raised some very pertinent points which are not adequately dealt with in the paper. What I contemplate is that private investments that are displaced by public investments, as in the case of electric power generation in the United States where both public and private owners are of substantial importance, should be valued in precisely the same way as public investments even to the extent that one ought not to value private investments in the same way that the prospective investors will. For example, one should value private investments, using the rate of discount that you would use in valuing the public investments, normally a lower rate of discount than private investors employ.

I would answer your question about taxes in the same spirit. Taxes are purely a transfer payment and should be taken account of in dealing with competitive private investments just as they are in dealing with public investments that do not generate taxes.

JOHNSON

Another point relates to the paper, but was not referred to specifically in your presentation this afternoon One of your reasons for government investments was to improve the distribution of income by providing goods at low prices (below marginal cost) to various low income groups. « Such subsidies in kind are often more feasible administratively than direct transfer payments, and are likely to contribute more to general economic advance »[3]. I don't know of many cases in which a government in mixed economy can produce goods which it can make available generally only to certain lower income groups; housing is perhaps one example, but this is about the only one. Governments can make direct transfer payments to low income groups on a very broad scale and for all purposes that you want to have such payments. Do we want to rely upon the judgements of the recipients on the ways of expending the income given to them or do we want them to rely on the decisions

[3] *Dorfman* - pag. 26

of committees or of local administrators of public aid on what you have and how you should spend the income?

DORFMAN

The two cases that I had in mind in introducing that motivation were public housing and education. In both cases it is generally felt that there are very important external economies of consumption so that not only does the public want to provide those services to the lower income groups, but also it is the public will that the beneficiaries of such subsidies spend them on the goods that they are intended for and on no others. This is candidly a form of sumptuary legislation in which the community is consciously and explicitly substituting its judgment for the preferences of some of its consumers because it is felt that the community as a whole will benefit from the types of consumption that are being subsidized.

KOOPMANS

I should first confess that I have to apologize for having read only one half of the paper. However from the beautiful exposition that Mr. DORFMAN gave, I understood that his discussion was concerned with evaluating specific projects, one by one, so to say, or three at a time, and he described TINBERGEN's proposal of an iterative procedure by which to get the optimizing quantities and their shadow prices in line. My question is, how does the rate of return itself, or if a different rate is applied to different parts of the future, how does the sequence of rates of return applicable to individual one-year periods in the future get determined? Is this also part of the iterative process of dialogue between the economic planner and the political decision maker? If so, is that problem not of a higher order of complexity in the data requirements in that really all relevant projects, including those likely to be forthcoming from the private sector of the economy, enter also?

DORFMAN

Professor KOOPMANS has asked a very searching question dealing with a very central issue. I should like to try to respond to it in two stages. First, it seems to be essential that there should be some rate of discount to be used in comparing results that mature at different times. If there is none, inconsistencies will certainly creep into the governments' investment program. Different and conflicting choices will be made in designing different projects and projects in different sectors of the economy. In some cases substantial resources will be expended to secure early accrual of benefits, and in others, the opposite choice will be made. The wastes resulting from such discrepancies can be avoided only by applying a uniform rate of time preference to all projects. I believe that Professor KOOPMANS would subscribe to that, but then he asks where are the necessary rates of time preference to come from. The second part of my response deals with that issue.

In some project planning, in the United States, for example, an explicitly formulated rate of time preference is incorporated. More usually there is no explicit rate, but if one reviews the choices made in the project design, often an implicit rate can be discovered. The annals of project design and of legislative debate of projects submitted to legislatures should, I am conjecturing, disclose what these implicit rates of time discount are. I do not believe that there is any economic market from whose behavior the social rates of time preference can be ascertained. As a substitute for such a market, I suggest that we review from this point of view the behavior of governments, and particularly of their legislative branches.

MAHALANOBIS

I should agree with Professor KOOPMANS on technical points which I may briefly mention; but I have got some points of a very different nature. On p. 4, education has been given as a leading example. I presume education does include science and scientific research.

DORFMAN

I am afraid that I think that it would strain the conventional meaning of « education » to use that word to include science and scientific research. The promotion of science and scientific research is, of course, an important function of government. These activities do have important external economies. The discoverer of an important scientific truth cannot usually retain the benefits of his discovery even by means of a patent system. This kind of external economy, however, is sufficiently different from an external economy of consumption, as exemplified by tbe benefits to society of having its members better housed and better educated, so that it is best to deal with them separately and not to combine science and scientific research with education for analytic purposes.

MAHALANOBIS

I meant when education is treated as an item of external economies, I simply enquired whether science would be included as an essential part.

Another point on p. 5 is a practical one. In para 9 I agree that governments may invest in uneconomic enterprises with the intention of enhancing the prestige of the nation, which would be undesirable. Steel plants are sometimes mentioned as prestigeous but uneconomic enterprises. Although I have been trying for a long time, I have not found any adequate discussion of the circumstances in which the setting up of a steel plant would be economic and proper. I could well imagine that a very small country like Panama should not do so. But it would be extremely useful to find out in specific cases, for example, for Ceylon which has rather a small population, or for countries with even a smaller population of two millions or so, whether it would be advisable to set up a steel mill and this also in an extreme case when domestic resources of iron or coal are not available. I am giving this particular example to widen the scope of the discussion.

I am in agreement with the use of shadow price in principle. There is however great difficulty in practice in isolating one project from another. It seems to me that it is often necessary in making practical decisions in a country like India, to focus attention not only on the project but also to consider the external economies at a national level. The time factor also seems to me to be extremely important, that is, the span of time over which the benefits would accrue. An interesting case which I analysed several years ago was the choice in India between importing foodgrains, or importing fertilisers, or manufacturing fertilisers with imported machinery, or finally, building machinery in India for the manufacture of fertilisers. The answer would depend essentially on the time horizon.

The really important point which I should have stated earlier, is where do you want to go and when? We have to set up certain targets at the national level which have to be achieved over a certain time period, in 10 years or in 15 years or in 20 years. Without such targets and the time period over which these have to be realised, I doubt whether shadow prices would be useful. Firstly, a set of target at the national level and secondly a given time period over which these have to be achieved have to be supplied for optimization.

DORFMAN

I think Professor MAHALANOBIS is raising two distinguishable questions here. The question of the steel plant is not a particularly difficult one. A steel plant may have some external economies, in particular the educational consequences of helping to train a labor force which is skilled in working with ferrous metals. A steel plant

[3] *Dorfman* - pag. 30

also involves certain external diseconomies because of its impact on air and water pollution. But these are all so conjectural and so minor in comparison with the directly measurable cousequences of an investment in a steel plant that it is best to ignore them and to analyze a steel plant by a straightforward benefit-cost analysis or, much the same thing, a capital budgeting analysis. In this way we could ascertain whether the prospective output was valued highly enough to justify the required expenditures.

The question about time horizons and time periods seems to be very closely related to Professor KOOPMANS' question. The answer lies in the choice of an appropriate rate of time preference which will allow us to evaluate consequences that accrue at different times whether they be expenditures, which normally occur early in the life of a project, or beneficial outputs which normally accrue over a very long period of time. The shadow price idea is simply a device for placing comparative values on consequences which occur on different dates. They express the importance of targets as well as the time periods over which they are realized and are not separable in concept from social targets to be attained by public investment undertakings.

MAHALANOBIS

I agree entirely with what Professor DORFMAN had said; certain value judgments have to be made in some way or other by policy makers. In using econometric models sub-optimization is also necessary. The advantage of a complete aggregate model is that we may be able to reach some of the special decisions when these are not conflicting. But in making calculations for optimization even in the case of a particular project, it seems to me that there will be much gain in objectivity in choosing the shadow price if we set up certain targets which belong to the whole of the national economy over a given period of time.

DORFMAN

I think we are in very complete agreement whenever there is a sound way to approximate the consequences of a particular project. When this is possible those consequences should be inserted in the calcnlations and should be evaluated by means of the shadow prices. It has been my experience that in practice the only consequences that you can estimate reliably are those that are very proximate to the project itself. The more remote conseqnences, which may be very important in some instances, are connected with the direct results in extremely obscure ways that must be left in the realm of judgment.

MAHALANOBIS

We are not in disagreement, bnt I am suggesting, on the basis of some of our own experience in India, it would be useful to attach a time period for pnblic investments, even in sub-optimization in the case of a particular project. Without building in an assigned time period, I do not see how we can get a shadow price. If we build in a time period, the solution may differ for 5 years or 10 years or 15 years, then it will be a question at a national level what should be the proper period of time to be taken into consideration. Whether the decisions are to be implemented by central planning or by wise decision in a free economy is a separate question; I am simply considering what would be the proper model.

DORFMAN

Professor MAHALANOBIS and I have both asserted repeatedly that we are in agreement, but now I begin to feel that also we are not understanding each other very well. Professor MAHALANOBIS insists that « it would be useful to attach a time period for public investments ». Of course it would, and I fail to see why he feels it so necessary to insist on this. Every consequence of a public investment occurs at some date. The importance of that consequence naturally depends on the date or dates at which it occurs. The time period and time sequence is incorporated in the analysis by means of the shadow prices which are different for every date at which something can happen. An event which occurs 10 years hence will receive less weight in an analysis than one that occurs 5 years hence if the shadow prices for events 10 years in the future are lower than those for events 5 years in the future, and this will be the case whenever there is a positive rate of time discount.

SCHNEIDER

When we consider an economic system which is mainly based on the principles of a market economy with a public sector, then I don't see that the problem of shadow prices has so much importance as it has for example in developing countries or in a centrally planned economic system.

DORFMAN

It is very helpful of you to remind us that where we have market prices to guide us we have much less need for shadow prices. In fact, we may have no need at all. But when public investments are being considered, we do not have market prices for all the goods and services that concern us. In particular we do not have market prices for events that will occur in the future or for the external economies and diseconomies that will result from the contemplated investment.

Shadow prices are required for all the consequences of an investment undertaking that are not valued in the ordinary markets.

For example, in designing a road, we may need shadow prices for the saving of lives or for the saving of time that would result from designing and building the road to higher standards.

SCHNEIDER

If you would call this additional cost the shadow price, then I am in agreement with you; I have understood shadow prices as prices that are not paid on the market but which are calculated for internal deliveries from one firm to another firm.

KOOPMANS

The term and concept of shadow price has caught on very much, and like Prof. SCHNEIDER, I am all for its use. I would only point out that the theory on which the concept is based does not cover as many conditions and circumstances as our tendency to use the term would suggest. In particular there is the case of choices in which important indivisibilities are present. I would surmise that the concept of shadow prices can be precisely and carefully extended to such cases, but I have not seen it worked out in the literature. There is one article by BAUMOL and GOMORY (« Econometrica », July 1960) that goes in this direction, but it does not go all the way.

Prof. MAHALANOBIS asked: is there a specific study of a steel mill from this point of view. I may mention a study not of a steel mill, but of a fertilizer plant — the question of whether in the Latin American area there should be one, two, three or up to five or six plants, and if so, for each number what are the best places to put them. There is a study by MANNE and VIETORISZ devoted to this question in a volume called « Studies in Process Analysis », edited by Manne and Markowitz (Wiley, 1963). That study used combinatorial methods because of the indivisibilities of the plants concerned. Again the shadow price concept has not been fully elaborated, and that is how the reference occurred to me in this context.

FRISCH

I am glad that Prof. DORFMAN raised the question of what is a « project », of what is to be understood by the description of a project. What kind of thing should be included in the project description? In my mind there are certain things that can, without difficulty be included in a project description, and which can be specified without relating this « project » to other projects.

The quantity of various types of inputs needed, in order to carry the project out, the distribution of these inputs over time; the capacity increase which the project will cause if accepted, etc., all these things can be specified perfectly well in the project description of this individual project. These data are more or less of the engineering kind. But there are other effects of the decision to accept a project, which can only be described by considering this project as part of the whole economy. Here is where the economist come into the picture.

Some of these effects — particularly the indirect ones — are very difficult to trace. This is just the *raison d'être* of the complete decision model.

There are good effects and bad effects. You must define the preferences of the political authority before you can say what is « good » and what is « bad ». If you really want to go to the bottom of this description of effects, you will by necessity be led more and more in the direction of the global model which I presented.

LEONTIEF

The operational significance of Professor DORFMAN's scheme can possibly be best judged in terms of the amount and uature of factual information that would be required for its practical implementation. He proposes to derive the production function comprising all possible input combinations that could be used for the manufacture of the finished output in terms of which he defines a particular project. Should these inputs be described in terms of intermediate or only primary factors? What source of information can be tapped to determine prices considered as given in the proposed computations? In other words, how should the cut off point or points be determined beyond which he does not intend to push the application of maximizing procedures involving explicit consideration of several well-defined alternatives.

ALLAIS

I want to underline only one point. It is in fact very difficult to choose the right indicative prices without very close connection with the market, without the pressure of the market. And I can give two very striking examples. The first one concerns the calculations of the French National Electricity Board. A few years ago the engineers were calculating projects using a very low rate of interest. When these calculations had been made they had the choice between different projects. The rate of interest chosen for the calculations was something about 3 per cent, I don't remember exactly. Later in order to make a final choice, they used a new rate of interest of 8 per cent, as a result of general instructions issued by the Commissariat au Plan. Correspondingly, the result that each project effectively retained represented an over-investment. The final outcome of this error of principle in the calculations was a waste of resources for the French economy. This waste would have been avoided if the decisions of the Electricity Board had followed the rules of the market economy.

The second example is the indicative price for coal which the Three Wise Men designated by the European Coal and Steel Community in 1957 advised should be used for the energy policy of the Europe of the Six. This shadow price was 22 dollars per ton of American coal c.i.f. European port. But four years later the price was only 12 dollars. If there had been a real and reliable market, such an error could not have arisen. This error was a very regrettable one, for in the meantime there was an incentive to invest much money in projects which in fact implied a waste of scarce resources. Thank you.

DORFMAN

I greatly appreciate the comments. All of them are valid and some deal, of course, with problems that I, myself, encountered in my thinking, but due to defects in my exposition, were not set forth clearly in my discussion. I share with Professor ALLAIS and also with some of the others the feeling that it is extraordinarily difficult to evaluate projects without heavy reliance on prices established in markets. I judge that market prices are much more reliable indicators of social worth than any shadow prices that a group of experts or politicians may establish, and I think that the proposals I have made would not be practical without a substantial substratum of market prices, expecially those dealing with costs and the values of certain salable outputs. But there cannot be reliable free market prices in principle for some of the aspects of the kinds of projects we are here discussing. In particular, this is true of the external consequences of projects, as in the case of education, and of some convenieuce aspects like those relating to speed and safety, which come up in the design of roads. The shadow prices that I am thinking of are used to fill in the gaps, so to speak, in the system of market prices. Take the market prices where you have them and be grateful. Where market prices do not exist, we need a device for estimating social values of types of factors and of consequences that are not priced on the markets.

I think that this response deals also with the issue that Professor LEONTIEF has raised. I should recommend a very businesslike approach to implementing my proposal, in effect drawing up a pro forma profit and loss account for the project being examined. All inputs used should be included on the cost side, be they intermediate or primary factors, and similarly all outputs produced should be assigned values; market prices when available, shadow prices when necessary.

And, finally, if I have another 30 seconds, I should like to say something about the relationship of my thinking with Professor FRISCH's. He asserts that one should begin with a social objective function or some measure of social welfare. My own contribution, if it be a contribution, is merely to set forth a way to discover certain aspects of the social welfare function. I do not feel that we can begin with it literally because at the beginning we do not know what it is. We must discover it by trying out proposals and seeing how the public and political organs react to them. In this way social preferences will be revealed, and a social welfare function will be discovered which can be used in designing and appraising investment projects.

ON THE CONCEPT OF OPTIMAL ECONOMIC GROWTH (*)

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1. Approaches in the literature

The search for a principle from which an « optimal » rate of economic growth can be deduced holds great fascination to economists. A variety of attitudes or approaches to this problem can be discerned in the literature.

One school of thought, represented among others by Professor Bauer [1957], favors that balance between the welfare of present and future generations that is implied in the spontaneous and individual savings decisions of the present generation. A policy implementing this preference would merely seek to arrange for tax collection and other government actions affecting the economy in such a way as to distort or amend the individual savings preferences as little as possible.

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Contrasting with this view is the position, expressed among others quite explicitly by Professor ALLAIS [1947, Ch. VI, X], that the balancing of the interests of different generations is an ethical or political problem, in which the competitive market solution has no valid claim to moral superiority over other solutions that depend for their realization on action by the state. A more specific optimality concept is implied in the strictures of Professor HARROD [1948, p. 40] and of FRANK RAMSEY [1928, p. 543] against any discounting of future utilities. These authors leave little doubt that they regard only equal weights for the welfare of present and future generations as ethically defensible.

The purpose of the present paper is to do some « logical experiments », in which various mathematical forms of the optimality criterion are confronted with a very simple model of technology and of population growth, to see what their maximization leads to. Our study is similar in purpose to RAMSEY'S classical paper, and to TINBERGEN'S recent exploration [1960] of the same problem. The underlying idea of this exploratory approach is that the problem of optimal growth is too complicated, or at least too unfamiliar, for one to feel comfortable in making an *entirely* a priori choice of an optimality criterion before one knows the implications of alternative choices. One may wish to choose between principles on the basis of the results of their application. In order to do so, one first needs to know what these results are. This is an economic question logically prior to the ethical or political choice of a criterion.

What is a suitable mathematical formalization of the idea of an optimality criterion? The most basic notion is that of a preference ordering of growth paths. Such an ordering states for each pair of alternative growth paths whether they are equally good, and if not, which is preferred. Indifference, preference and preference-or-indifference are usually required to be transitive.

An important class of preference orderings is that representable (1) by a continuous preference function (utility function, indicator, etc.). A particular function which has been frequently used has the form

$$U = \sum_{t=1}^{\infty} \varkappa^{t-1} u(x_t)$$

for consumption paths $(x_1, x_2, \ldots)$ of infinite duration with discrete time $t = 1, 2, \ldots$. This form can be interpreted as a discounted sum of future one-period utilities $u(x_t)$ with a discount factor of $\varkappa$ per period. This form has been derived by the present author (2) from postulates expressing, among other requirements,

(*a*) noncomplementarity of consumption in any three subperiods into which the future may be partitioned;

(*b*) stationarity in the sense that the ordering of any two paths is not altered if both consumption sequences are postponed by one time unit and identical consumptions are inserted in the gaps so created in each path.

The utility function so obtained is « cardinal » only in the limited sense that the simple form of a discounted sum is conserved only by *linear* transformations of the utility scale. If below we occasionally use the expressions « utility difference », « marginal utility », these must be interpreted as elliptic

(1) Conditions of continuity under which a given preference ordering permits such a representation have been studied by WOLD [1943] and by DEBREU [1954].

(2) KOOPMANS [1960], especially Section 14.

phrases referring to a preference indicator of that particularly simple form. There is no intent to claim that, even in the absence of risk or uncertainty, there is some physical or psychological significance to the comparison of utility *differences* in such a scale.

There still remains a logical gap between the derivation of the above utility function from the postulates referred to and its use in the present study: For present purposes a continuous time concept is more appropriate.

2. Plan of the Present Paper

We shall freely borrow from PHELPS [1961] and others mentioned below the assumptions of the main model considered in Section 4, from RAMSEY [1928] a device for maximizing utility over an infinite horizon without discounting, together with methods for applying the device, from SRINIVASAN [1962] and from UZAWA [1963] information about the results of maximizing a discounted sum of future consumption, and from INAGAKI [1963] results about the generalization of the present problem to the case of predictable technological progress (1). If this particular brew has not been served before, it is not put together here for any novelty of the combination. Rather, our eclectic model appears to have in it the minimum collection of elements needed to serve the two main aims of the present paper.

The first aim is to illustrate the usefulness of the tools and concepts of mathematical programming in relation to the problem of optimal economic growth.

The second aim is to argue against the complete separation

(1) Note added in proof: A study by PUGACHEV [1963], which has several similarities with the present study, has been brought to my attention by J.M. MONTIAS.

of the ethical or political choice of an objective function from the investigation of the set of technologically feasible paths. Our main conclusion will be that such a separation is not workable. Ignoring realities in adopting « principles » may lead one to search for a nonexistent optimum, or to adopt an « optimum » that is open to unanticipated objections.

In connection with the first aim, Section 3 recalls a few results of the theory of linear and convex programming in a finite number of variables, that bear on the problem of optimum growth. The reading of this section is believed to be helpful rather than essential for what follows. Indeed, in most of its formulations, the problem of optimal growth is a special problem in mathematical programming. The main new element arises from the open-endedness of the future. If one adopts a finite time horizon, the choice of the terminal capital stock is as much a part of the problem to be solved as the choice of the path. Terminal capital, after all, represents the collection of paths beyond the horizon that it makes possible. An infinite horizon is therefore perhaps a more natural specification in many formulations of the problem of optimal growth. The mathematical complications so created are the price for the greater explicitness of long run considerations thus made possible.

Sections 4-6 analyze a model with a single producible good serving both as capital in the form of a stock, and as a consumption good in the form of a flow. It is produced under a constant technology by a labor force growing exogenously at a given exponential rate. Proofs for many of the propositions labeled (A), (B), ... in Section 4 are given under the same label in an Appendix (¹).

In Section 7 the findings of the logical experiments of Sections 5, 6 are examined. The main conclusion is that some

(¹) Approximate equivalents of propositions (E), (F), (H), (I), (J) were obtained independently by DAVID CASS [1963]. The connection between the limiting case of a zero discount rate and the « golden rule of accumulation » (see Section 5) is also observed and discussed in CASS's paper.

utility functions that on a priori grounds appear quite plausible and reasonable do not permit determination of an optimal growth path even in a constant technology. Tentative and intuitive explanations for this finding are offered.

Section 8 discusses in a tentative way, and without proofs, possible extensions of the analysis to a changing technology and/or a variable rate of population growth, with none, one, or both of these regarded as policy variables.

3. Pertinent Aspects of Linear and of Convex Programming

Let linear programming be applied to an allocation problem in terms of the quantities x_j, $j = 1, \ldots, n$ of a finite number n of commodities. Then the *feasible set* D is given by a finite number of linear inequalities

$$\sum_{j=1}^{n} a_{ij} x_j \leqq b_i , \qquad i = 1, \ldots, m . \tag{1}$$

The objective function, or maximand, is a linear form in the x_j,

$$U = \sum_{j=1}^{n} c_j x_j . \tag{2}$$

The feasible set D is always closed, and may be bounded (as in Figure 1) or unbounded (Figure 2).

The range R of the objective function on the feasible set (the set of values assumed by the maximand on the points of the set D) is an interval. If D is bounded (contained in some hypercube), then R is necessarily also bounded. If D is un-

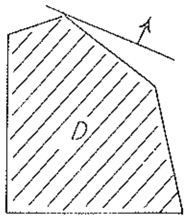

FIG. 1

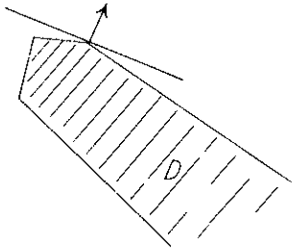

FIG. 2

bounded, then R may still be bounded, but may also be unbounded from below, from above, or both. If R is bounded from above, an optimal point exists (Figure 2). If R is unbounded from above, no optimum exists (Figure 3). Both cases can arise on the same feasible set D through different choices of the maximand.

A highly special form of linear programming has been used by KANTOROVICH [1959]. In this case the objective is defined by prescribing the ratios of the quantities of all *desired goods,*

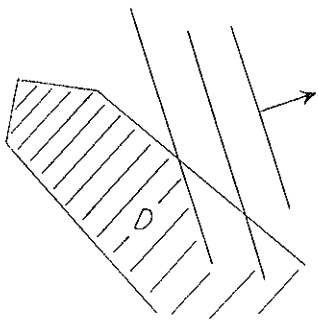

FIG. 3

i.e., goods entering into the objective, and by maximizing a common scalar factor applied to these quantities (Figure 4). This problem can also be formulated in linear programming terms: One adds to the constraints (1) linear *equalities* expressing the prescribed ratios, and chooses as a maximand (2) the qnantity of any one desired good, say.

In convex programming the feasible set is defined by

$$(3) \qquad g_i(x_1, \ldots, x_n) \geqq 0 \,, \qquad i = 1, \ldots, m,$$

where the g_i are *concave* (1) functions, and the maximand

$$(4) \qquad U = U(x_1, \ldots, x_n)$$

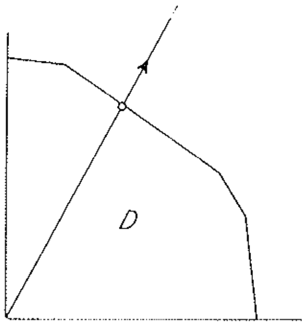

FIG. 4

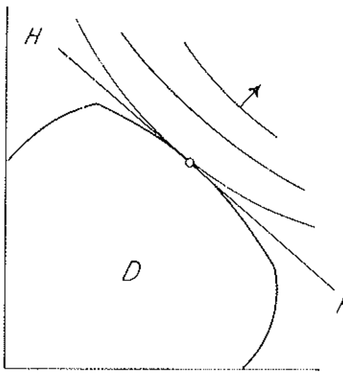

FIG. 5

(1) A concave function $g(x_1, \ldots, x_n)$ is represented by a hypersurface $y = g(x_1, \ldots, x_n)$ in the space $\{y, x_1, \ldots, x_n\}$ that is never « below » any of its chords (if the $+y$ direction is « up »).

is another concave function (Figure 5). The term *convex programming* derives from the fact that the feasible set, and each set of points on which the maximand attains or exceeds a given value, are convex ([1]). Linear programming is a special case of convex programming.

With any optimal point in a convex programming problem one can associate a hyperplane H through that point, which separates the feasible set from the set of points in which the maximand exceeds its value in the optimal point (H is a line in Figure 5). The direction coefficients of such a hyperplane define a vector of relative prices implicit in the optimal point. One interpretation of the implicit prices is that the opening up of an opportunity to barter unlimited amounts of commodities at those relative prices does not allow the attainment of a higher value of the maximand. Moreover, if the maximand is a differentiable utility function, one may be able, by treating utility as an additional « commodity » and choosing its « price » to be unity, to interpret the implicit prices of the other goods as their marginal utilities either directly in consumption, or indirectly through the extra consumption made possible by the availability of one more unit of that commodity as a factor of production.

4. A One-Sector Model with Constant Technology and Steadily Increasing Labor Force

We assume that output of the single producible commodity is a twice differentiable and concave function $F(Z, L)$, homogeneous of degree one, of the capital stock Z and the size of

([1]) A convex set is a set of points containing every line segment connecting two of its points.

the labor force L. These assumptions imply full employment of labor and capital, constant returns to scale, and nonincreasing returns to an increase in only one factor of production. Since capital is treated as a stock of the single producible commodity, output is at any time t to be allocated to a positive rate of consumption X_t, and to a positive, zero, or even negative rate of net investment Y_t. Hence, if we use a continuous time concept, and denote derivatives with respect to time by dots, we have

$$X_t + Y_t = F(Z_t, \; L_t) \; , \tag{5}$$

$$Y_t = \dot{Z}_t \; . \tag{6}$$

$F(Z, L)$ is defined for all $Z \geqq 0$, $L \geqq 0$. We assume further that both labor and capital are essential to production, that either factor has a positive marginal productivity, and that returns to increases in only one factor are strictly decreasing,

$$(7) \quad \begin{cases} (7a, b) & F(0, L) = 0 \; , & F(Z, 0) = 0 \; . \\ (7c, d) & \dfrac{\delta F}{\delta Z} > 0 \; , & \dfrac{\delta F}{\delta L} > 0 \; , \\ (7e, f) & \dfrac{\delta^2 F}{\delta Z^2} < 0 \; , & \dfrac{\delta^2 F}{\delta L^2} < 0 \; . \end{cases}$$

Finally, we assume that the labor force increases at a constant positive exponential rate λ, from a given initial magnitude L_0,

$$(8\ a,\ b) \qquad L_t = L_0 \, e^{\lambda t} \; , \qquad \lambda > 0 \; .$$

The homogeneity of the production function enables us to go over to per-unit-of-labor-force concepts. Calling the unit of labor force briefly a « worker », let x denote consumption per worker, y ditto net investment, z ditto capital stock, and

$$(9) \qquad f(z) = \frac{1}{L} F(Z, L) = F\left(\frac{Z}{L}, 1\right) = F(z, 1)$$

output per worker. Since we then have

$$\dot{Z}_t = \frac{d}{dt}(z_t L_t) = \dot{z}_t L_t + z_t \dot{L}_t = (\dot{z}_t + \lambda z_t) L_t ,$$

the feasible set in the space of per-worker variables x_t, z_t becomes

$$(10) \qquad \begin{cases} (10a) & x_t + \dot{z}_t = f(z_t) - \lambda z_t , \\ (10b,c,d) & x_t > 0, \qquad z_t > 0 , \quad z_0 \text{ given.} \end{cases}$$

The term λz represents the (net) investment needed if one wants merely to supply the growing labor force with capital at the existing ratio of capital per worker.

To be specific we shall call a path (x_t, z_t) satisfying (10) *attainable* (for the given z_0), and use the term *feasible* path in the wider sense of a path attainable for some $z_0 > 0$.

It is implied in (7 e) that $f(z)$ is strictly concave [1].

[1] A strictly concave function is one that is strictly « above » all its chords.

The per-worker production function $f(z)$ is therefore represented by a curve such as is shown in Figure 6. The curve rises from the value $f(0)=0$ with a decreasing slope. In particular, any line λz through the origin and of slope λ such that $0<\lambda<f'(0)$ will ultimately intersect the curve and continue above it ([1]),

(11) for any $\lambda > 0$ such that $0 < \lambda < f'(0)$ there is a $\bar{z} > 0$ such that

(11*a*) $f(\bar{z}) = \lambda \bar{z}$,

(11*b*) $f(z) < \lambda z$ for $z > \bar{z}$.

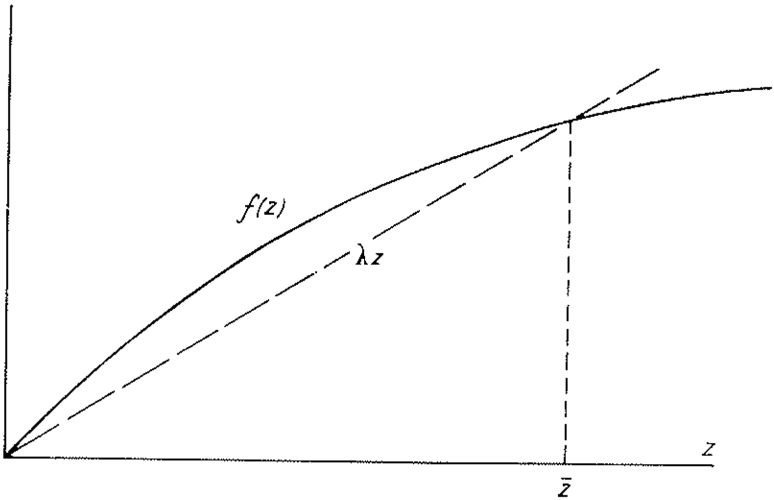

Fig. 6

([1]) To obtain (11) suppose that, for some such λ, $f(z) \geq \lambda z$ for arbitrarily large values of z. Then, by $f(0)=0$ and the concavity of f, $f(z) \geq \lambda z$ for all $z \geq 0$. But then, for any $Z>0$, (7 *b*) and the continuity of $F(Z, L)$ imply the contradiction $0 = F(Z, 0) = \lim_{L \to 0} F(Z, L) = \lim_{L \to 0} LF\left(\frac{Z}{L}, 1\right) = Z \lim_{z \to \infty} \frac{1}{z} f(z) \geq Z\lambda > 0$.

If λ represents the rate of growth of the labor force, $\bar{z}$ represents a capital stock per worker so large that the investment required to keep it at the same level absorbs all output, leaving nothing for consumption. If $z_0 \geqq \bar{z}$, it will therefore be necessary to allow z_t to decrease at least to some level below $\bar{z}$. To avoid the uninteresting complication arising if $z_0 > \bar{z}$ we shall from here on simply define « feasibility » so as to imply $0 < z_0 \leqq \bar{z}$.

Although we have not yet defined a maximand, it may be observed that the attainable set is now defined in a space where the « point » is a pair of positive functions x_t, z_t of time, defined for $0 \leqq t < \infty$. This is an infinite-dimensional space for the double reason that we use a continuous time concept and an infinite horizon. It remains infinite-dimensional if we limit ourselves (1) to twice differentiable functions z_t and once differentiable functions x_t.

5. The Path of the Golden Rule of Accumulation

To answer an important preliminary question, we first consider a Kantorovich type restriction of the problem to a one-dimensional one. The latter problem has been formulated and solved in the last few years, independently and in one form or another, by (2) Allais [1962], Desrousseaux [1961], Phelps [1961], Joan Robinson [1962], Swan [1960], von Weizsäcker [1962].

Remove from the definition of the attainable set the restric-

(1) Due to twice differentiability of the data functions $f(z)$ above and $u(x)$ below we will not be excluding any optimal paths by that requirement. However, a slightly weaker requirement will be found useful in the Appendix.

(2) Dates are bibliographical only and refer to the list of references below. Some of these authors used somewhat more general models involving an exponential technological improvement factor in the production function.

tion that z_0 is given, thus making initial capital a free good. Restrict the attainable set instead by an arbitrary stipulation that consumption per worker and capital per worker are to be held constant over time,

$$x_t = x \ , \quad z_t = z \quad \text{for all} \quad t \geqq 0 \ .$$

The new « attainable » set then is given by

$$(12\ a,\ b,\ c) \qquad x = f(z) - \lambda z, \quad x > 0, \quad z > 0.$$

Finally, choose z so as to maximize x, the permanent level of consumption per worker. This leads to the choice of that value $\hat{z}$ of z for which.

$$(13\ a,\ b) \qquad f'(\hat{z}) = \lambda \ , \quad \text{so} \quad \hat{x} = f(\hat{z}) - \lambda \hat{z} \ ,$$

where $f'(z)$ denotes the derivative of $f(z)$.

Figure 7 shows the construction. Because, of the essentiality of labor to production, i.e., assumption (7 b) as reflected in (11), there is for any given slope λ such that $0 < \lambda < f'(0)$ a point $\hat{z}$ for which the tangent to the production function per worker has that slope. To interpret the condition (13 a) note that, if we hold L fixed, then by the homogeneity of F,

$$f'(z) = \frac{\partial F(Z/L,\ 1)}{\partial (Z/L)} = \frac{\partial F(Z,\ L)}{\partial Z} \ .$$

Hence (13) expresses equality, at all times t, of the marginal productivity of capital (in producing capital, say) to the growth rate λ — a prescription known as the golden rule of accumulation [1].

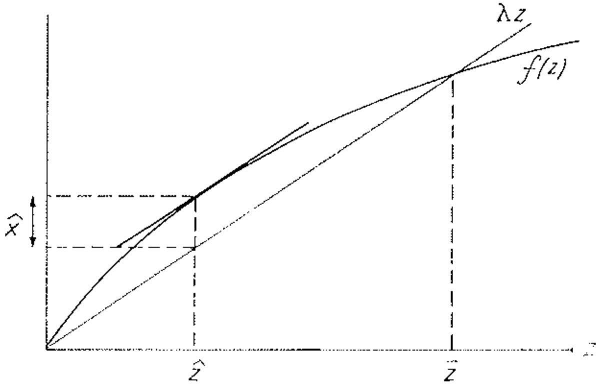

FIG. 7

6. Existence and Characteristics of Optimal Paths

We now return to the original problem that allows x_t and z_t to vary in time and recognizes the restriction (10 d) of a historically given initial capital stock, and look about for a suitable maximand. We admit to an ethical preference for neutrality as between the welfare of different generations. After some hesitation, we tentatively and arbitrarily resolve another ethical conundrum by interpreting this « timing neutrality » in a percapita sense. That is, we assume first of all that labor force and population grow in proportion. Furthermore we thus imply

[1] PHELPS [1961].

that, starting from the golden rule path $\hat{x}$, $\hat{z}$ of the preceding section as a base line, we welcome equally a unit increase in consumption *per worker* in any one future decade, say. Mere numbers do not give one generation an edge over another in this scheme of values.

The next difficulty we face is a technical one. A previous investigation by KOOPMANS [1960], continued by KOOPMANS, DIAMOND and WILLIAMSON [1962], has shown that there does not exist a utility function of all consumption paths, which at the same time exhibits timing neutrality and satisfies other reasonable postulates which all utility functions used so far have agreed with. A way out of this dilemma was shown by RAMSEY [1928]. One can define an *eligible* set of consumption paths on which a neutral utility function can be defined. Moreover, the eligible set is a subset of the feasible set such that the remaining, *ineligible*, paths are clearly inferior to the eligible ones, in a sense still to be defined. In RAMSEY's case, in which population was assumed stationary, the criterion of eligibility was a sufficiently rapid approach over time to what he called a state of *bliss*. This state was defined as either a saturation of consumers with consumption goods, or a saturation of the productive system with capital to the point where its marginal productivity has vanished — whichever state would be encountered first. We shall find that in the present case of a steady population growth the golden rule path can take the place of RAMSEY's state of bliss in defining eligibility. Thus RAMSEY's device can be applied to our case with what seems a lesser strain on the imagination in regard to situations outside the range of experience.

We have one more technical choice to make. For reasons of mathematical simplicity, and at some cost in « realism », we shall model our utility function after the finite-horizon example of

$$(14) \qquad V_T = \int_0^T u(x_t)\, dt \;.$$

As explained already in Section 1, this simple integration of an instantaneous utility flow $u(x_t)$ implies noncomplementarity between consumption in any two or more parts of the future.

We shall assume that the instantaneous utility flow is a strictly concave, increasing and twice differentiable function $u(x)$ of the instantaneous consumption flow x. This function does not change with time, and is defined for all $x>0$. Strict concavity implies that we attribute greater weight to the marginal unit of per capita consumption of a poor generation as compared with a rich one. To assume $u(x)$ increasing rules out saturation. Finally, instead of introducing a subsistence minimum, we shall require that

$$\lim_{x \to 0} u(x) = -\infty \,, \tag{15}$$

a strong incentive to avoid periods of very low consumption as much as is feasible.

Let $\hat{u} = u(\hat{x})$ denote the instantaneous utility flow derived from the consumption flow per worker of the path $x_t = \hat{x}$, $z_t = \hat{z}$, of the golden rule. We shall now work with the difference between the integral (14) for any given feasible path and its value for the golden rule path, and study the behavior of this difference as T goes to infinity. The following propositions can be proved (for proofs see Appendix).

(A) *There is a number* $\bar{U}$ *such that*

$$U_T = \int_0^T \left(u(x_t) - \hat{u}\right) dt \leqq \bar{U} \tag{16}$$

for all feasible paths (x_t, z_t) *and for all horizons* T.

Thus, if utility is measured in conformity with (14), no path is « infinitely better » than the golden rule path. In particular, no feasible path x_t can indefinitely maintain or exceed a level u of utility flow that exceeds $\hat{u}$. Thus the golden rule path continually attains the highest indefinitely maintainable utility flow.

(B) *For every feasible path, either* $\lim_{T\to\infty} U_T$ *exists (is a finite number), or* U_T *diverges to* $-\infty$ *as* T *tends to* ∞.

In the first case, we call the path *eligible*, in the second *ineligible*. Then (B) establishes a clear superiority of each eligible path over each ineligible one. On the eligible set we choose as the utility function

$$U = \int_0^\infty \left(u(x_t) - \hat{u}\right) dt \; . \tag{17}$$

In propositions (C), (D), an *optimal* path is defined as a path maximizing U on the set of eligible and attainable paths.

It is not hard to find eligible and attainable paths for every admissible initial capital stock z_0. If $z_0 > \hat{z}$, one only needs to refrain from net investment until the capital stock $\hat{Z}_t = \hat{z} L_0 e^{\lambda t}$ of the golden rule path has caught up with the given initial stock $Z_0 = z_0 L_0$, and to continue along the golden rule path thereafter. If $0 < z_0 < \hat{z}$, one can through a *finite* period of tightening the belt arrive on the same path.

(C) *For any initial capital stock* z_0 *with* $0 < z_0 \leqq \bar{z}$ *there exists a unique optimal path* $(\hat{x}_t, \hat{z}_t)$ *in the set of eligible and attainable paths. For* $z_0 \neq \hat{z}$, *both* $\hat{x}_t$ *and* $\hat{z}_t$ *exhibit a strictly monotonic approach to* $\hat{x}$ *and* $\hat{z}$, *respectively, from below if* $0 < z_0 < \hat{z}$, *from above if* $\hat{z} < z_0 \leqq \bar{z}$. *For* $z_0 = \hat{z}$, *the optimal path is* $\hat{x}_t = \hat{x}$, $\hat{z}_t = \hat{z}$ *for all* t, *the golden rule path.*

(D) *The optimal path satisfies the condition*

$$u'(x_t)\ \dot{z}_t = \hat{u} - u(x_t) \tag{18}$$

that at any time the net increase in capital per worker multiplied by the marginal utility of consumption per worker equals the net excess of the maximum sustainable utility level over the current utility level.

This condition is similar to the KEYNES-RAMSEY condition [RAMSEY, 1928, equation (5)] formulated in terms of absolute amounts of consumption, and reverts to it for $\lambda = 0$. KEYNES' intuitive reasoning in support of this condition carries over with only slight reinterpretation.

A number of analogous results can be obtained if the utility of a consumption path is defined as an integral over the instantaneous utility flow discounted at a *positive* instantaneous rate ρ.

(E) *The utility function*

$$V(\rho) = \int_0^\infty e^{-\rho t}\, u(x_t)\, dt, \ \textit{where}\ \ \rho > 0 \ , \tag{19}$$

is defined for all feasible paths for which $x_t \geqq \underline{x}$ *for all* t, *whenever* $\underline{x} > 0$.

RAMSEY's device is therefore unnecessary in this case. We shall however obtain an economy of notation if instead of $V(\rho)$ we use the utility function

$$U(\rho) = \int_0^\infty e^{-\rho t}\left(u(x_t) - \hat{u}\right) dt, \quad \rho > 0 \ , \tag{20}$$

which differs from $V(\rho)$ by a constant. As before, we shall write $U_T(\rho)$ if the integral in (20) extends from 0 to $T<\infty$.

The stipulation in (E) that keeps consumption from becoming altogether too small is necessitated by (15), merely to prevent $U_T(\rho)$ from diverging to $-\infty$ as $T\to\infty$. However, we shall for $\rho>0$ define as the eligible-and-attainable set the set of *all* paths with the prescribed z_0 for which $V(\rho)$ exists. (E) assures us that no paths worth consideration are excluded from the eligible set. If z_0 were to be very small, we could still allow for growth by taking $\underline{x}$ correspondingly smaller.

In the following propositions (F) through (J) *optimality* is defined by maximization of (20) on the appropriate eligible-attainable set. It is assumed in propositions (F), (G), that an eligible-attainable path $(\hat{x}_t, \hat{z}_t)$ is given, which is under scrutiny for its possible optimality. The propositions associate with such a path tentative implicit prices of the consumption good and of the use of the (identical) capital good. Once optimality of the path $(\hat{x}_t, \hat{z}_t)$ is confirmed, these prices are no longer tentative, and generalize to an infinite-dimensional space the idea of a hyperplane separating attainable from better-than-optimally-attainable programs, illustrated in Figure 5. The (dated) price of the consumption good is defined from (20) by

$$p_t = e^{-\rho t}\; u'(\hat{x}_t)\;, \tag{21}$$

the present value of the marginal instantaneous utility of consumption at time t if the given path $(\hat{x}_t, \hat{z}_t)$ is followed. The price of the use of the capital good is similarly defined by

$$q_t = p_t\; g'(\hat{z}_t) \tag{22}$$

as the present value of the marginal productivity of capital at time t multiplied by the marginal utility of consumption at that time. Finally, we denote by

(23)

$$\hat{U}(\rho) = \hat{U}_{\infty}(\rho)\,, \quad \text{where} \quad \hat{U}_T(\rho) = \int_0^T e^{-\rho t}\left(u(\hat{x}_t) - \hat{u}\right)dt \text{ for } T \leqq \infty\,,$$

the utility of the path $(\hat{x}_t, \hat{z}_t)$ under scrutiny.

Propositions (F) and (G) hold for any eligible-attainable path $(\hat{x}_t, \hat{z}_t)$ and its corresponding prices p_t, q_t, regardless of whether that path passes the test for optimality. Propositions (H), (I) together express that test.

(F) *If* $(\hat{x}_t, \hat{z}_t)$ *is an eligible and attainable path, and if* (x_t, z_t) *is any path, feasible or not, then*

(24)
$$U_T(\rho) - \hat{U}_T(\rho) \leqq \int_0^T p_t(x_t - \hat{x}_t)\,dt$$

for all finite T, *and for* $T = \infty$ *whenever the integral converges.*

Since both members vanish if $x_t = \hat{x}_t$ for all t, this means that both

(i) the utility $U_T(\rho)$ of the path (x_t, z_t) is maximized, subject only to the « budget constraint »

$$\int_0^T p_t(x_t - \hat{x}_t)\,dt \leqq 0\,,$$

if $(x_t, z_t) = (\hat{x}_t, \hat{z}_t)$, and

(ii) « consumption expenditure at implicit prices »

$$\int_0^T p_t\, x_t\, dt$$

reaches its minimum, on the set of paths with utility $U_T(\rho)$ equal to or exceeding that of the path $(\hat{x}_t, \hat{z}_t)$, if $(x_t, z_t) = (\hat{x}_t, \hat{z}_t)$.

(G) *If* $(\hat{x}_t, \hat{z}_t)$, (x_t, z_t) *are two eligible and attainable paths,*

$$(25) \qquad \int_0^T p_t(x_t - \hat{x}_t)\,dt \leqq \int_0^T \left(q_t(z_t - \hat{z}_t) - p_t(\dot{z}_t - \dot{\hat{z}}_t)\right) dt =$$

$$= \int (q_t + \dot{p}_t)(z_t - \hat{z}_t)\, dt - p_T(z_T - \hat{z}_T)$$

for all finite T, *and for* $T = \infty$ *if the integrals converge and the last term has a limit.*

Again, all three members vanish if (x_t, z_t) is itself the path $(\hat{x}_t, \hat{z}_t)$. The inequality in (25), rewritten as

$$\int_0^T p_t(x_t + \dot{z}_t - \hat{x}_t - \dot{\hat{z}}_t)\, dt - \int_0^T q_t(z_t - \hat{z}_t)\, dt \leqq 0 \ ,$$

says that, at prices implicit in the path $(\hat{x}_t, \hat{z}_t)$, « revenue » from total output minus « rental cost of use of capital » is maximized in that path.

(24) and (25) together give rise to Propositions (H), (I), (J).

(H) *Let* $\hat{x}(\rho)$, $\hat{z}(\rho)$ *be defined as the solution* x, z *of*

$$f'(z) = \lambda + \rho, \quad f(z) - \lambda z = x, \quad \textit{where} \quad 0 < \lambda \leqq \lambda + \rho < f'(0). \tag{26}$$

Then if $z_0 = \hat{z}(\rho)$, *the unique optimal path is* $\hat{x}_t = \hat{x}(\rho)$, $\hat{z}_t = \hat{z}(\rho)$ *for all* $t \geqq 0$.

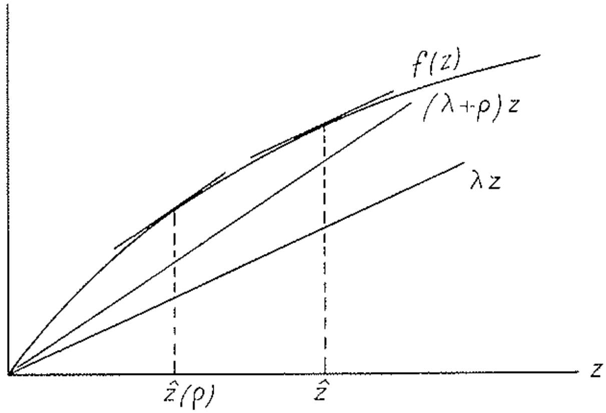

FIG. 8

The determination of $z(\rho)$ is shown in Figure 8. Because of the strict concavity of $f(z)$, $\hat{z}(\rho)$ and $\hat{x}(\rho)$ exist and are unique, and $0 < \hat{z}(\rho) < \hat{z}(\rho^*) \leqq \hat{z}$ for $f'(0) - \lambda > \rho > \rho^* \geqq 0$, and hence, since $f(z) - \lambda z$ increases for $0 \leqq z \leqq \hat{z}$,

$$\hat{x}(\rho) \equiv f(\hat{z}(\rho)) - \lambda \hat{z}(\rho) < \hat{x}(\rho^*) \leqq \hat{x} \text{ for } f'(0) - \lambda > \rho > \rho^* \geqq 0. \tag{27}$$

The constant optimal path made possible by the initial capital stock $z_o = \hat{z}(\rho)$ is found to be an asymptote for the optimal paths associated with other values of z_o.

(I) *For all* z_0 *with* $0 < z_0 \leqq \bar{z}$, *the unique optimal path* $(\hat{x}_t, \hat{z}_t)$ *is uniquely characterized by the two conditions*

(α) $\lim\limits_{T \to \infty} \hat{z}_T = \hat{z}(\rho)$,

(β) *the prices* (21), (22) *implicit in the path* $(\hat{x}_t, \hat{z}_t)$ *satisfy the differential equation*

$$q_t + \dot{p}_t = 0 \quad \text{for all} \quad t \geqq 0. \tag{28}$$

To interpret condition (β), let (x_t, z_t) be a path which differs from the optimal path only slightly and only on a short open interval $\mathcal{I}$, on which $z_t > \hat{z}_t$ (see Figure 9 *a*). Then x_t will differ from $\hat{x}_t$ first because the slightly higher capital stock on $\mathcal{I}$ allows a slightly higher product, and secondly because acceleration of investment during the first part of $\mathcal{I}$ and deceleration during the second part leads to some postponement of consumption within $\mathcal{I}$. In the light of (21), (22), the condition says that, for an arbitrarily small difference $z_t - \hat{z}_t$ of arbitrarily short duration, the utility effects of these two components of $x_t - \hat{x}_t$ must cancel if the path $(\hat{x}_t, \hat{z}_t)$ is to be optimal.

If condition (β) of Proposition (I) is satisfied, the inequality between the first and third members of (25) becomes

$$\int_0^T p_t(x_t - \hat{x}_t)\, dt - p_T(z_T - \hat{z}_T) \leqq 0 \ . \tag{29}$$

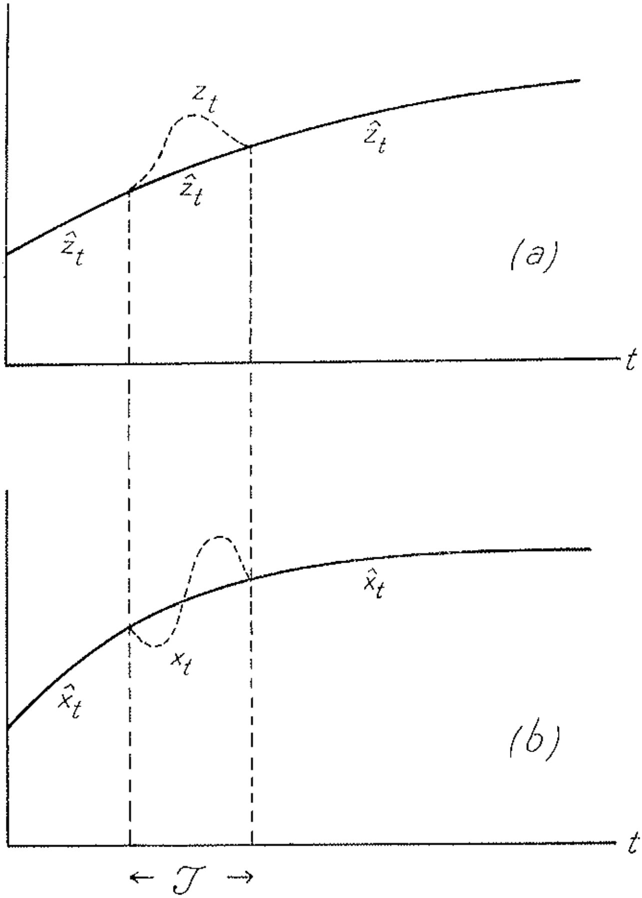

Fig. 9

The interpretation of this inequality is that, in an economy that keeps its own capital accounts (rather than renting the capital it employs), the revenue from deliveries to consumption plus the value of the capital stock at the end T of the planning horizon is maximized in the optimal path $(\hat{x}_t, \hat{z}_t)$, in comparison with all eligible-attainable paths. This is so for all finite T, and in the limit for $T \to \infty$ if such limit exists.

Finally, (24) and (29) together yield

$$(30) \qquad U_T(\rho) - \hat{U}_T(\rho) \leqq \int_0^T p_t(x_t - \hat{x}_t)\, dt \leqq p_T(z_T - \hat{z}_T) \ .$$

If condition (α) of Proposition (I) is also satisfied, then (21) and Proposition (J) below imply that the price p_t associated with the optimal path approaches zero as $t \to \infty$. In that case $\lim_{T\to\infty} p_T(z_T - \hat{z}_T) = 0$, and capital disappears from the accounts for an infinite horizon. The inequality between the first and last members of (30) carried to the limit for $T \to \infty$ then confirms the optimality of the path $(\hat{x}_t, \hat{z}_t)$. In the Appendix we show that the middle member of (30) also converges. We can therefore supplement statements (i), (ii), made in interpreting (F) above (with $T \to \infty$) by the statement

(iii) « revenue from deliveries to consumption

$$\int_0^\infty p_t\, x_t\, dt$$

is maximized, on the set of eligible-attainable paths, if $(x_t, z_t) = (\hat{x}_t, \hat{z}_t)$.

Proposition (J) describes the path characterized by propositions (H), (I).

(J) *In the unique optimal path* $(\hat{x}_t, \hat{z}_t)$ *for any initial capital stock* z_0 *with* $0 < z_0 \leqq \bar{z}$, $z_0 \neq \hat{z}(\rho)$, *both* $\hat{x}_t$ *and* $\hat{z}_t$ *exhibit a monotonic and asymptotic approach to* $\hat{x}(\rho)$ *and* $\hat{z}(\rho)$, *respectively, from above if* $z_0 > \hat{z}(\rho)$, *from below if* $z_0 < \hat{z}(\rho)$.

For later discussion, we note from (27) that the asymptotic level $\hat{x}(\rho)$ of consumption per worker, while independent of the initial capital z_0, is reduced as the discount rate is increased. The maximum of $\hat{x}(\rho)$ for $\rho \geqq 0$ is attained at $\rho = 0$. We shall not examine the cases where $\rho \geqq f'(0) - \lambda$.

Finally, a word about the case where one tries to apply a negative discount factor $\rho < 0$. Writing $-\rho = \sigma$, this means looking for a utility function extending the finite-horizon example

$$V_T(-\sigma) = \int_0^T e^{\sigma t}\, u(x_t)\, dt$$

to an infinite horizon. This is not as far-fetched as it may seem. After all, we have so far given no weight at all to mere numbers in comparing generations. If we were to weight each generation in proportion to its number, and otherwise seek neutrality with regard to timing, the population growth parameter λ would take the place of σ above.

In order to apply RAMSEY's device in the present case, one would have to find a feasible path (x_t, z_t) such that

$$(31) \qquad W_T^*(-\sigma) = \int_0^T e^{\sigma t} \left(u(x_t^*) - u(x_t) \right) dt$$

is uniformly bounded from above for all feasible paths (x_t^*, z_t^*) and all values of T. The following statement says that no such path exists.

(K) *For each* $\sigma > 0$, *for each attainable path* (x_t, z_t), *where* $0 < z_0 \leq \bar{z}$, *and for each number* $N > 0$, *there exist another attainable path* (x_t^*, z_t^*) *and a number* T^* *such that*

$$(32) \qquad W_T^*(-\sigma) > N \quad \textit{for all} \quad T \geq T^* .$$

This says, essentially, that there is no upper bound to the range, on the attainable set, of a utility function of the type we are seeking to define. The case $\rho < 0$ is therefore analogous to the case in ordinary linear programming illustrated by Figure 4. The same difficulty was noticed and discussed by TINBERGEN [1960] and by CHAKRAVARTY [1962] in connection with the case $\rho = 0$ for a model with constant returns to increases in the amount of capital alone.

In the present case, the reasons for the absence of an optimal path for $\rho < 0$ can be illustrated in terms of the path $(x_t, z_t) = (\hat{x}, \hat{z})$, optimal if $\rho = 0$ and $z_0 = \hat{z}$. From (21) we see that the implicit price of the unit of consumption good per worker, associated with this path would have to be a constant,

$$p_t = u'(\hat{x}) \quad \text{for all} \quad t .$$

This means that a sacrifice of one unit in *per capita* consumption, now made for a short period as a slight departure from this path, can be taken out by any future generation in the form of an equal augmentation of *per capita* consumption beyond that provided by the path, for a period of the same short duration. Now if either the discount rate $\rho < 0$, or if $\rho = 0$

but some weight is explicitly given to population size, it will always increase utility to delay still further the time at which the fruit of the initial sacrifice is reaped.

In the proofs of Propositions (A) - (K), given in the Appendix, one common characteristic of the problems considered is repeatedly used without explicit mention. At any time in an optimal path $(\hat{x}_t, \hat{z}_t)$, the capital stock $\hat{z}_t$ is the only link between the past and the future. This is due, on the one hand, to the utility function being an integral over time of instantaneous utilities (discounted or not). On the other hand, it arises from the fact that the feasibility constraint (10 *a*) restricts z_t but not $\dot{x}_t$. Hence the function x_t is in principle free to vary discontinuously (even though it is found optimal for it not to do so). However, $\dot{z}_t$ is bounded by (10 *a, b*), hence z_t can only vary continuously. The resulting property can be expressed formally as follows: If $(\hat{x}_t, \hat{z}_t)$ is an optimal path for given z_o, then, for any T, the path (x^*_t, z^*_t) defined by

$$x^*_t = \hat{x}_{T+t} \ , \quad z^*_t = \hat{z}_{T+t}$$

is optimal for $z^*_0 = \hat{z}_T$.

7. Adjusting Preferences to Opportunities

What have we learned from our « logical experiments »? We have confronted a simple model of production with a utility function representing a sum of future per-capita utilities, discounted by a positive, zero, or negative instantaneous rate of discount ρ. We have found that $\rho = 0$ is the smallest rate for which an optimal path exists.

Let us assume for the sake of argument that the present model is representative enough to be looked on as a tentative test of the applicability of the ethical principles under consideration. Then we have just managed to avoid discriminating against future generations on the basis of remoteness of the time at which they live. However, this close escape for virtue was possible only by making welfare comparisons on a per capita basis. If instead we should want to weight per capita welfare by population size, then we are forced to discriminate on the basis of historical time by positive discounting. There seems to be no way, in an indefinitely growing population, to give equal weight to all individuals living at all times in the future.

This dilemma suggests that the open-endedness of the future imposes mathematical limits on the autonomy of ethical thought. The suggestion may come as a shock to welfare economists, because no such logical obstacles have been encountered in the more fully explored problems of allocation and distribution for a finite population. It is true that the mere fact that we are considering an infinite number of people does not fully explain the dilemma. For RAMSEY was able, albeit by artificial assumptions, to indicate a fair solution to the problem for the infinite future of a population of constant size. Our difficulty is therefore connected with the assumption of an indefinite growth in the population.

The following reasoning may further illuminate the reasons for the nonexistence of an optimal path with negative ρ. Assume that $0>\rho>f'(\bar{z})-\lambda$. (Of course, $\rho=-\lambda$ would correspond to equal weights given to the utilities of all individuals. However, $f'(z)-\lambda>-\lambda$, and our illustration is simpler if we do keep $\hat{z}(\rho)<\bar{z}$ by taking $\rho>f'(\bar{z})-\lambda$. Consider now an optimal path for the finite time period $0\leqq t\leqq T$, defined by initial and terminal per-worker capital stock levels $z_0=z_T=\hat{z}$ both equal to that level $\hat{z}$ which, *if maintained at all times,* would secure the maximum maintainable consumption per head. The analysis associated with the proofs of (H), (I), (J) in the appendix now indicates that, if the level $\hat{z}$ is prescribed only for $t=0$ and $t=T$, the optimal path bulges out toward the level of $\hat{z}(\rho)$, as indicated in Figure 10. The interpretation is roughly as

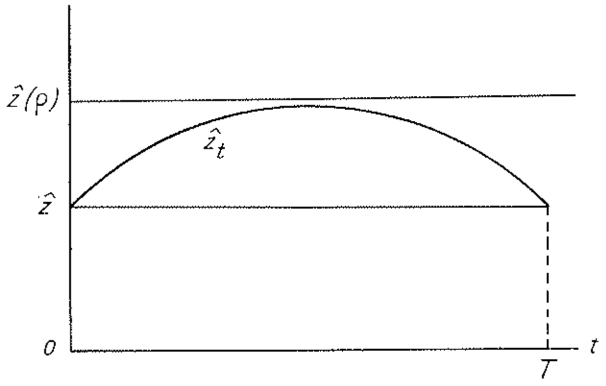

FIG. 10

follows. The negative discount rate gives the greatest weight to the per capita utility of the last generation living within the planning period [0, T]. In response to this weighting system, the optimal path provides for a reduction in capital per worker (a « disinvestment » in a per capita sense) during a terminal segment of the planning period, in order to allow for high consumption at that time. To make this possible, all preceding generations make a sacrifice. For the first generation, this takes the form of heavy investment needed to increase the capital stock more than in proportion to population growth. For the intermediate generations, it consists in approximately maintaining the capital stock — by continued proportional growth — at a per capita level in excess of that which would maximize per capita consumption.

Now if T is increased, the benefitted generation becomes a more and more distant one. If $T=\infty$, there is no benefitted generation, and the limiting position of the curve in Figure 10, while mathematically well-defined, merely describes a path of indefinite and fruitless sacrifice.

The problem appears in even sharper light if technological progress is also recognized. A study by INAGAKI [1963] uses a COBB-DOUGLAS production function

$$F(Z, L, t) = \text{const. } e^{\beta t} Z^{\alpha} L^{1-\alpha}$$

subject to exogenous technological progress at the constant proportional rate β, an instantaneous utility function

$$(33) \qquad u(x) = \int^{x} \frac{ds}{\log s - \log \underline{x}}$$

exhibiting suitable behavior for large values of x, and a labor force growing exponentially at the rate λ. Among other results, INAGAKI finds that, for the integral $V(\rho)$ as defined in (19) to converge on the counterpart of our path $(x_t, z_t) = (\hat{x}(\rho), \hat{z}(\rho))$, it is necessary that

$$(34) \qquad \rho > \frac{\beta}{1-\alpha}$$

Let us assume that RAMSEY's device can be used also in this case, and that it would again merely result in adding the borderline value $\rho = \frac{\beta}{1-\alpha}$ to the set of discount rates defining a utility function for which an optimal path exists. Then a predictable positive lower bound to the rate of technical progress, valid for an indefinite future period, precludes application of the ethical principle of timing neutrality in terms even of per capita utility — not to speak at all of weighting generations by their numbers.

Thus, if in the face of technological progress we want to hold on to the idea of maximizing a utility integral such as (33) over time, we must invent a discount rate ρ satisfying (34), or its equivalent for another production function. Such a discount rate might just have to be a pragmatic one having no basis in a priori ethical thought. While it might well be a result, conscious or unconscious, of political processes or decisions, it would have to be revised upward if it is estimated that technological progress will accelerate to such an extent as to « overtake it », and could be revised downward if it is expected that progress will slow down.

One might instead conclude that the whole idea of maximizing a utility integral is not flexible enough to fit the inequality of opportunity between generations inherent in modern technology. Two alternative notions have been partially explored by the present author, using a discrete concept of time. In one of these [KOOPMANS, 1960, see also KOOPMANS, DIAMOND and WILLIAMSON, 1964], the utility function of a consumption path x_t, $t = 1, 2, \ldots$, can be defined by a recursive relation

$$U(x_1, x_2, \ldots) = V\big(u(x_1), U(x_2, x_3, \ldots)\big)$$

in terms of a one-period utility function $u(x)$ and an aggregator function $V(u, U)$. This formulation allows the (scale-invariant) discount factor

$$\left(\frac{\delta V(u, U)}{\delta U}\right)_{u = u(x),\quad U = U(x, x, \ldots)}$$

associated with a constant path to increase or decrease with the level x at which the path proceeds. The second alternative [KOOPMANS, 1962] is an attempt to express formally the idea of a present preference for flexibility in future preferences between different commodity bundles of the same timing, or between physically the same bundles spread out differently over time, or between bundles differing in both respects. Further analysis will be needed to determine whether the first idea is sufficiently flexible to enable us to avoid the difficulties we have encountered, or, if not, whether the second idea can be made workable.

8. TECHNICAL PROGRESS AND POPULATION GROWTH AS POSSIBLE POLICY VARIABLES

So far we have treated both technical progress and population growth as exogenously given. It should now be recognized that both variables can be, and are in many countries, influenced by public and private policies and attitudes. Technical change is furthered by government conduct or support of research and of education, by the tax treatment of depreciation and obsolescence, and by business policies witb regard to research and development. Population growth is influenced by expenditures for public health, by family allowances, by government policies toward family planning, and by general cultural and religious attitudes toward the idea of population control. Iu addition, both variables are in part endogenously affected by the level of income.

Both possibilities of partial control raise new conceptual problems in formalizing the idea of optimal economic growth. In the middle of the scientific explosion, it is hard to assess whether technological progress can go on forever, so that also its rate can be raised or lowered forever. It is conceivable that a higher rate of discovery and invention in the present will entail a lower rate of progress at some later time when the fund of knowledge usable in production nears completion. Another consideration is that technological progress raises transition and dislocation difficulties that affect the relative welfare of different individuals within the same generation.

The possibility of influencing population size raises the question of the value of population size in itself — as distinct from the question of the weight given to numbers in aggregating utility over generations, discussed above. It should be noted that all utility functions discussed in this paper imply neutrality with regard to population size as such. The question is of some importance because a different attitude might lead to a different balance between the « value of numbers » and the loss of per capita income that may result from an increase in the ratio of population to land and/or other resources. This problem did not come up in the more formal analysis of the preceding section because the assumption of constant returns to proportional increases in both labor and capital precluded the recognition of resource limitations.

APPENDIX

A 1. NOTATIONS

Instantaneous discount rate . . . . $\rho \equiv -\sigma$

Exponential growth rate of labor force λ

	At time t absolute	At time t per worker	Integrated over time (per worker)
Consumption flow	X_t	x_t	
Capital stock	Z_t	z_t	
Labor force	L_t		
Production function	$F(Z, L)$	$f(z)$	
Utility		$u(x)$	U, V, W

Derivatives with respect to time are denoted by dots, $\dot{z}_t = \frac{dz_t}{dt}$, other derivatives by dashes, $f'(z) = \frac{df}{dz}$, $u'(x) = \frac{du}{dx}$.

$\wedge$ generally denotes optimal paths and their asymptotic levels.

$\equiv$ denotes equality by definition.

A 2. Assumptions

(*a*) = (8) $L_t = L_o\, e^{\lambda t}$ for all $t \geqq 0$, where $0 < \lambda < f'(0)$,

(*b*) = (9) $F(Z, L) = L\, f(\frac{Z}{L}) = L\, f(z)$ for all $L > 0$, $Z \geqq 0$,

(*c*) $f(0) = 0$, $f'(z) > 0$, $f''(z) < 0$ for $0 \leqq z$,

(*d*) = (11 *a*) for each $\lambda > 0$ such that $0 < \lambda < f'(0)$ there is a $\bar{z}_\lambda > 0$ such that $f(\bar{z}_\lambda) = \lambda \bar{z}_\lambda$ (the subscript λ of $\bar{z}_\lambda$ is omitted in what follows),

(*e*) $u'(x) > 0$, $u''(x) < 0$ for $0 < x < \infty$, $\lim_{x \to 0} u(x) = -\infty$.

A 3. Some implications of feasibility

Given the initial stock z_o of capital per worker, the *attainable set* of growth paths (x_t, z_t) is now given, in terms of per-worker variables, by the requirements that, for all $t \geqq 0$,

$$
(35) \quad \begin{cases} (35a)\ x_t > 0,\ z_t \geqq 0\ , \\ (35b)\ z_t \text{ is continuous,} \\ (35c)\ z_t,\ x_t \text{ are differentiable to the right,} \\ (35d)\ \dot{z}_t \text{ and } \dot{x}_t \text{ are continuous to the right,} \end{cases}
$$

$$
(36) \quad x_t + \dot{z}_t = f(z_t) - \lambda\, z_t \equiv g(z_t)\ , \quad \text{say,}
$$

$$
(37) \quad z_o \text{ is prescribed, where } 0 < z_o \leqq \bar{z}\ .
$$

The *feasible set* is the union of all attainable sets with $0 < z_o \leqq \bar{z}$.

We note that, by Assumption (c), both the feasible set and the attainable sets are convex, and that the function $g(z)$ defined in (36) is strictly concave. Since $g(z)$ vanishes for $z=0$ and for $z=\bar{z}$, it reaches its maximum $\hat{x}$ in a unique point $\hat{z}$, so that

$$(38) \quad \begin{cases} (38a) \ \hat{x} \equiv g(\hat{z}) > g(z) \text{ for all } z \neq \hat{z}, \text{ where } 0 < \hat{z} < \bar{z} , \\ (38b) \ g'(z) > g'(\hat{z}) = 0 > g'(z^*) \text{ whenever } 0 \leqq z < \hat{z} < z^* \leqq \bar{z} . \end{cases}$$

From (35 a), (36), we have

$$(39) \qquad \dot{z}_t < x_t + \dot{z}_t = g(z_t)$$

and hence for all feasible paths, using (35 a), (37), and the fact that $g(z)>0$ only for $0<z<\bar{z}$,

$$(40) \qquad 0 < z_t \leqq \bar{z} \quad \text{for all} \quad t \geqq 0 .$$

Here $0=z_t$ has been ruled out because it would not allow the positive consumption $x_{t'}$ for $t' \geqq t$ required by (35 a).

A 4. A BASIC INEQUALITY AND ONE APPLICATION

The concavity Assumption (e) of $u(x)$ implies that

$$(41) \qquad u(x) - u(x^*) \leqq u'(x^*) \cdot (x - x^*) \quad \text{for all} \quad x, \ x^* ,$$

and the concavity of $g(z)$ implied in Assumption (c) and (36) that

$$g(z) - g(z^*) \leqq g'(z^*) \cdot (z - z^*) \quad \text{for all} \quad z,\ z^* . \tag{42}$$

We shall make many comparisons of utility integrals for feasible growth paths (x_t, z_t) and (x_t^*, z_t^*), based on (41) and on either (38) or (42). To avoid repetition we state this comparison here in its most general form, where $0 \leqq T < T^* \leqq \infty$, and ρ is as yet unspecified.

$$(43)\left\{\begin{aligned}
&(43\,a)\left\{\begin{aligned}
{}_{T}U^*_{T^*}(\varrho) &\equiv \int_T^{T^*} e^{-\varrho t}\left(u(x_t) - u(x_t^*)\right)dt \leqq \int_T^{T^*} e^{-\varrho t}\, u'(x_t^*)\,(x_t - x_t^*)\, dt = \\
&= \int_T^{T^*} e^{-\varrho t}\, u'(x_t^*)\left(g(z_t) - g(z_t^*) - \dot z_t + \dot z_t^*\right) dt = \\
&= \int_T^{T^*} e^{-\varrho t}\, u'(x_t^*)\left(g(z_t) - g(z_t^*)\right) dt - \Big[e^{-\varrho t}\, u'(x_t^*)\,(z_t - z_t^*)\Big]_T^{T^*} + \\
&+ \int_T^{T^*} \left[\frac{d}{dt}\left(e^{-\varrho t}\, u'(x_t^*)\right)\right](z_t - z_t^*)\, dt \leqq
\end{aligned}\right. \\
&\leqq \int_T^{T^*} e^{-\varrho t}\left[u'(x_t^*)\left(g'(z_t^*) - \varrho\right) + u''(x_t^*)\,\dot x_t^*\right](z_t - z_t^*)\, dt - \\
&- \Big[e^{-\varrho t}\, u'(x_t^*)\,(z_t - z_t^*)\Big]_T^{T^*} .
\end{aligned}\right.$$

If $T^* = \infty$ the validity of (43) depends on convergence of the integrals involved.

One application of (43) will be used repeatedly. We define a *bulge* in a growth path (x_t, z_t) as an interval $[T, T^*]$ such that

$$(44) \quad \begin{cases} (44a) & 0 \leqq T < T^* < \infty, \quad z_T = z_{T^*} = z^*, \text{ say, and} \\ (44b) & \text{either } z^* \leqq \hat{z}(\rho) \text{ and } z_t < z^* \text{ for } T < t < T^* \\ & \text{or } z^* \geqq \hat{z}(\rho) \text{ and } z_t > z^* \text{ for } T < t < T^*, \end{cases}$$

where the definition (26) of $\hat{z}(\rho)$ is extended to all values of ρ,

$$(45) \quad \begin{cases} (45a) & \hat{z}(\rho) = \bar{z} \text{ for } \rho \leqq g'(\bar{z}), \\ (45b) & g'(\hat{z}(\rho)) = \rho \text{ for } g'(\bar{z}) < \rho < g'(0) \\ (45c) & \hat{z}(\rho) = 0 \text{ for } g'(0) \leqq \rho . \end{cases}$$

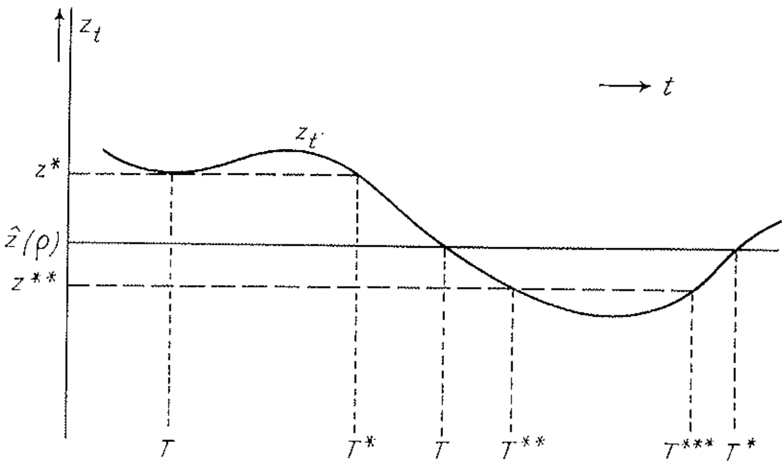

FIG. 11

Figure 11 shows z_t for a path with two bulges, both denoted [T, T*]. The effect on the utility integral of « straightening out » a bulge is found from (43) by taking $z_t^* = z^*$, $x_t^* = x^* \equiv g(z^*)$, and satisfies

(46)

$$-\int_{T}^{T^*} e^{-\varrho t}\left(u(x_t) - u(x^*)\right) dt \leqq -u'(x^*)\left(g'(z^*) - \rho\right)\int_{T}^{T^*} e^{-\varrho t}(z_t - z^*)\,dt > 0$$

if $z^* \neq \hat{z}(\rho)$, because in that case $g'(z^*) - \rho$ and $z_t - z^*$ are opposite in sign. If $z^* = z(\rho)$, and if for instance $z_t < z^*$ for $T < t < T^*$ as in the second bulge in Figure 11, we can by suitable choice of a number $z^{**} < \hat{z}(\rho)$ write the left hand member of (46) as the negative sum of two such integrals, one comparing (x_t^{**}, z_t^{**}) defined by $z_t^{**} \equiv \max \{z^{**}, z_t\}$ with $(x^*, z^*) = (\hat{x}(\rho), \hat{z}(\rho))$ on [T, T*], the other comparing (x_t, z_t) with (x^{**}, z^{**}), where $x^{**} \equiv g(z^{**})$, on an interval [T**, T***] such that $T < T^{**} < T^{***} < T^*$. Since of these integrals the former is nonpositive, the latter negative, (46) is valid also if $z^* = \hat{z}(\rho)$. We thus have

LEMMA 1: *For any ρ, a path (x_t, z_t) optimal on any finite or infinite time interval cannot contain a bulge.*

This conclusion, and the inequality (46) on which it is based, remain valid for $T^* = \infty$ and $\rho \geqq 0$ if the definition of a bulge is extended to read « (44 *b*) and either (44 *a*) or (44 *a'*) »,

(44 *a'*) $0 \leqq T < T^* = \infty$, $\rho \geqq 0$, $z_T = z^*$, and if $\rho = 0$ then $\lim_{t\to\infty} z_t = z^*$,

as illustrated in Figures 12 ($\rho = 0$) and 13 ($\rho > 0$).

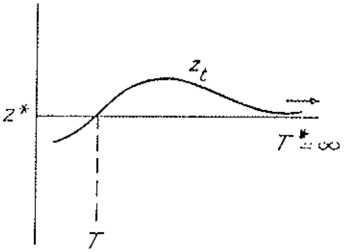

FIG. 12

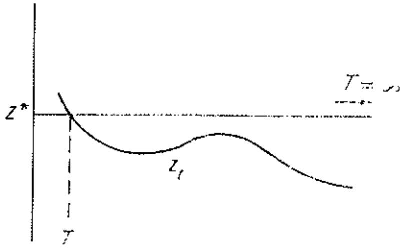

FIG. 13

A 5. INFERIORITY OF INDEFINITELY FLUCTUATING PATHS IF $\rho \leqq 0$

We define the asymptotic range of the path (x_t, z_t) as the nonempty closed interval

$$(47) \qquad [\underline{\zeta}, \bar{\zeta}], \quad \underline{\zeta} \equiv \lim_{T\to\infty} \inf_{t \geqq T} z_t, \quad \bar{\zeta} = \lim_{T\to\infty} \sup_{t \geqq T} z_t .$$

A positive length $\bar{\zeta} - \underline{\zeta}$ of the asymptotic range implies that z_t continues to fluctuate between any neighborhood of $\underline{\zeta}$ and any neighborhood of $\bar{\zeta}$, infinitely often, and for arbitrarily large t.

LEMMA 2: *If* $\rho \leqq 0$ *and if* $\underline{\zeta} < \bar{\zeta}$ *for the attainable path* (x_t, z_t), *then there exists for each* $N > 0$ *an attainable path* (x^*_t, z^*_t) *and a* $T_N > 0$ *such that*

$$(48) \qquad U^*_T(\rho) \equiv \int_0^T e^{-\rho t}\left(u(x_t) - u(x^*_t)\right) dt \leqq -N \text{ for all } T \geqq T_N .$$

For the proof of Lemma 2 we must strengthen (46) to obtain a positive lower bound on the gain ${}_{T}U^{*}_{T^*}(\rho)$ associated with the « straightening out » of a bulge [T, T*]. For this purpose we choose an interval $[z_*, z^*]$ such that

(49) (a) $\underline{\zeta} < z_* < z^* < \bar{\zeta}$ and either (b) $\hat{z}(\rho) < z_*$ or (c) $z^* < \hat{z}(\rho)$,

which is always possible. If for definiteness we assume (49 *c*), we have from Assumption (*c*) of Section A 2

$$(50) \qquad g'(z) - \rho \geqq g'(z^*) - \rho \equiv \gamma > 0 \quad \text{for} \quad z_* \leqq z \leqq z^* .$$

Now z_t has infinitely many bulges [T, T*] with the properties

$$(51) \qquad z_T = z_{T^*} = z^* \ , \quad z_t \leqq z_* \quad \text{for some} \quad t \in [T, T^*] .$$

Because of the continuity of z_t we can for each of these choose an interval $[\tau, \tau^*]$ such that, if we write $z^* - z_* \equiv 2\,\varepsilon$,

$$(52) \qquad T < \tau < \tau^* < T^* \text{ and } z_\tau = z_* < z_t < z_{\tau^*} = z^* - \varepsilon \text{ for } \tau < t < \tau^* .$$

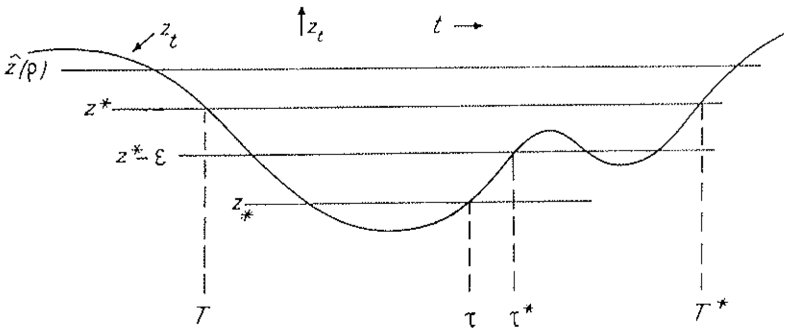

FIG. 14

The construction is illustrated in Figure 14. Since the *last* inequality in (46) holds also for all subintervals of [T, T*], we have from (52), (50), if $x^* \equiv g(z^*)$ and $\rho \leqq 0$,

$$- {}_{\mathrm{T}}\mathrm{U}^*_{\mathrm{T}^*}(\rho) \equiv - \int_{\mathrm{T}}^{\mathrm{T}^*} e^{-\rho t} \left(u(x_t) - u(x^*)\right) dt > - u'(x^*) \gamma \int_{\tau}^{\tau^*} (z_t - z^*)\, dt >$$
$$> u'(x^*) \gamma \varepsilon (\tau^* - \tau) \ ,$$

since $\varepsilon > 0$ and $u'(x^*)\ \gamma > 0$. On the other hand, we have from (52), (39) with $x_t > 0$, (36) and (38 *a*) that

$$\varepsilon = \int_{\tau}^{\tau^*} \dot{z}_t \, dt < \int_{\tau}^{\tau^*} g(z_t)\ dt < (\tau^* - \tau)\, \hat{x} \ .$$

whence $\tau^* - \tau > \varepsilon / \hat{x} > 0$ and

(53) $$- {}_{\mathrm{T}}\mathrm{U}^*_{\mathrm{T}^*}(\rho) > u'(x^*)\ \gamma\ \varepsilon^2 / \hat{x} \equiv \alpha^* > 0.$$

Finally, we define the feasible path $(x^*_t,\ z^*_t)$ by (36) and

$$z^*_t \equiv \max \{z_t,\ z^*\} \quad \text{for} \quad 0 \leqq t \leqq \mathrm{T_N},\ z^*_t = z_t \quad \text{for} \quad t > \mathrm{T_N} \ ,$$

subject to a later choice of $\mathrm{T_N}$ in such a way that $z_{\mathrm{T_N}} \leqq z^*$. Then

$$- \mathrm{U}_{\mathrm{T_N}}(\rho) \equiv - \int_0^{\mathrm{T_N}} e^{-\rho t} \left(u(x_t) - u(x^*_t)\right) dt \geqq n_{\mathrm{T_N}}\, \alpha^* \ , \ \text{where} \ \alpha^* > 0 \ ,$$

if $n_{T'}$ denotes the number of completed bulges in $[0, T']$. But $\lim_{T'\to\infty} n_{T'} = \infty$ because there are infinitely many bulges in $[0, \infty]$. Hence the choice of T_N such that $n_{T_N} \geqq N/\alpha^*$ establishes Lemma 2 in case (49 *c*) holds. The proof from (49 *b*) is similar.

A 6. Proofs for a zero discount rate ($\rho = 0$)

Proof of (A). In (43 *a*) take $\rho = 0$, $z_t^* = \hat{z}$, so $x_t^* = \hat{x} = g(\hat{z})$. Then if we write $u(\hat{x}) \equiv \hat{u}$, $u'(\hat{x}) \equiv \hat{u}'$,

$$(54) \qquad {}_{T}U_{T^*} \equiv \int_{T}^{T^*} \left(u(x_t) - \hat{u}\right) dt \leqq \hat{u}' \cdot (z_T - z_{T^*}) \leqq \hat{u}' \cdot \bar{z} \; ,$$

by (38), (40), regardless of T, T*, hence also for $T = 0$.

Proof of (B). We distinguish three cases regarding the asymptotic range $[\underline{\zeta}, \bar{\zeta}]$ of the given path (x_t, z_t).

Case (1), $\underline{\zeta} < \bar{\zeta}$. In this case we have from Lemma 2 and from (54) applied to (x_t^*, z_t^*), for any $N > 0$,

$$U_T = \int_0^T \left(u(x_t) - u(x_t^*)\right) dt + \int_0^T \left(u(x_t^*) - \hat{u}\right) dt \leqq -N + \hat{u}' \cdot \bar{z}$$

for all $T \geqq T_N$. In this case, therefore, U_T diverges to $-\infty$ as $T \to \infty$.

Case (2), $\underline{\zeta} = \bar{\zeta} \neq \hat{z}$. For definiteness assume $\bar{\zeta} < \hat{z}$ and let $\hat{z} - \bar{\zeta} = 2\,\varepsilon$. Then $\varepsilon > 0$ and there exists $T < \infty$ such that

$$(55) \qquad z_t \leqq \bar{\zeta} + \varepsilon = \hat{z} - \varepsilon \quad \text{for all} \quad t \geqq T\ .$$

If now in (43 *a*) we take $\rho = 0$, $z^*_t = \hat{z}$, $x^*_t = \hat{x} = g(\hat{z})$ for all $t \geqq 0$, then,

$$(56) \qquad {}_{T}U_{T^*} = \int_T^{T^*} \left(u(x_t) - \hat{u} \right) dt \leqq \hat{u}' \left[\int_T^{T^*} \left(g(z_t) - \hat{x} \right) dt + z_T - z_{T^*} \right] \leqq -$$

$$- \alpha\,(T^* - T) + \beta\ ,$$

where by (38), (40),

$$\alpha = \hat{u}' \cdot \left(\hat{x} - g(\hat{z} - \varepsilon) \right) > 0\ , \qquad \beta = \hat{u}' \cdot \bar{z}\ .$$

Hence $U_{T^*} = U_T + {}_{T}U_{T^*}$ diverges to $-\infty$ as $T^* \to \infty$ in this case, and by similar reasoning in the case $\hat{z} < \underline{\zeta}$, hence in the entire Case (2).

Case (3), $\underline{\zeta} = \bar{\zeta} = \hat{z}$. In this case clearly

$$(57) \qquad \lim_{T \to \infty} z_T = \hat{z}\ .$$

It follows from the third member of (54) that

$$G_T = U_T + \hat{u}' \cdot z_T$$

is a nonincreasing function of T. Hence G_T either possesses a limit for $T \to \infty$ or diverges to $-\infty$. In view of (57), the same must then be true for U_T.

This completes the proof of statement (B). In addition, we have found

LEMMA 3: *If $\rho = 0$, a necessary condition for eligibility of the path (x_t, z_t) is that (57) is satisfied.*

Proof of (C). An optimal path $(\hat{x}_t, \hat{z}_t)$ is now defined as one that maximizes

(58) $$U \equiv \int_0^\infty \left(u(x_t) - \hat{u}\right) dt$$

on the attainable-and-eligible set. A beautifully simple procedure used by RAMSEY in his slightly different problem can be adapted to the present problem as long as $\rho = 0$.

From Lemmas 1 and 3 we conclude that, in any optimal path, $\hat{z}_t$ exhibits a nondecreasing, constant, or nonincreasing approach to $\lim_{t \to \infty} \hat{z}_t = \hat{z}$ according as $z_0 < \hat{z}$, $= \hat{z}$ or $> \hat{z}$. This establishes the second and third sentences of statement (C) with the term « weakly monotonic » substituted for « strictly monotonic ». Now consider an attainable-eligible path (x_t, z_t) for which

(59) $$z_t = z^* \neq \hat{z} \quad \text{for} \quad T \leqq t \leqq T^*, \quad \text{where} \quad T < T^* .$$

Then, along the lines of (56),

$$ {}_{T}U_{T^*} \equiv \int_{T}^{T^*} \left(u(x_t) - \hat{u}\right) dt \leqq \hat{u}' (T^* - T) \left(g(z^*) - \hat{x}\right) < 0 $$

by (38 *a*). It follows that the path

$$ (x_t^*, z_t^*) = \begin{cases} (x_t, z_t) \text{ for } 0 \leqq t < T \ , \\ (x_{t+T^*-T} \ , z_{t+T^*-T}) \text{ for } T \leqq t \ , \end{cases} $$

is likewise attainable, and indeed eligible and preferable to (x_t, z_t), because it achieves a utility

$$ U^* = U_T^* + {}_{T}U^* = U_T + {}_{T*}U > U_T + {}_{T}U_{T*} + {}_{T*}U = U \ . $$

Therefore (59) cannot occur in an optimal path.

It follows that, if $z_0 \neq \hat{z}$, an optimal path shows a strictly monotonic approach of $\hat{z}_t$ to the value $z_T = \hat{z}$ for $0 \leqq t < T$, where $T \leqq \infty$. We shall call any eligible path with that property a *superior* path. To complete the proof of the second and third sentences of (C) we only need to show that for an *optimal* path $T = \infty$. This is best obtained as a corollary of the proof of (D).

The proof of the first sentence of (C) will also be combined with that of (D).

Proof of (D). For all superior paths we can now make a useful change of the variable of integration in (58) from t to z. Since, by (36), $z_t = \hat{z}$ for $t \geqq T$ implies $x_t = \hat{x}$, $u(\hat{x}_t) = \hat{u}$, we have for all superior paths, using (36),

$$ (60) \qquad U = \int_{z_0}^{\hat{z}} \frac{u\left(x_{t(z)}\right) - \hat{u}}{\dot{z}_{t(z)}} dz = \int_{z_0}^{\hat{z}} \frac{u\left(x(z)\right) - \hat{u}}{g(z) - x(z)} dz \ . $$

where $t(z)$ denotes the inverse of z_t on [o, T], and $x(z) \equiv x_{t(z)}$. The unknown function is now $x(z)$, defined, like $t(z)$, on the interval $z_o \leq z < \hat{z}$, or on $\hat{z} < z \leq z_o$, as the case may be. The advantage from the change of variables lies in the fact that only $x(z)$ itself, and no derivative thereof, occur in the integrand in (60). Hence (60) is maximized on the set of superior paths if and only if $x(z)$ is given a value $\hat{x}^*(z)$ such that the integrand is maximized for almost every value of z in its domain. This requires $\hat{x}^*(z)$ for almost every z to equal the solution $x = \hat{x}(z)$ of

(61) $$u'(x)\left(g(z) - x\right) = \hat{u} - u(x) \ .$$

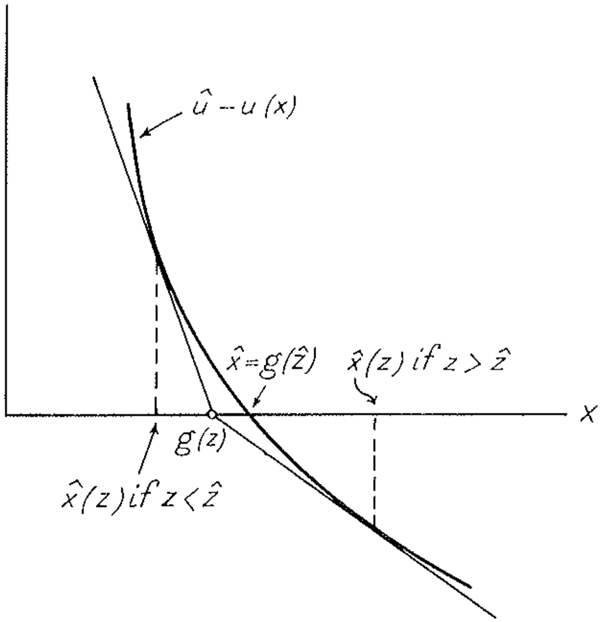

FIG. 15

Figure 15 shows the determination of $x=\hat{x}(z)$ for the two cases $z<\hat{z}$ and $z>\hat{z}$. It is easily seen from the diagram or analytically, using Assumptions (c), (d), (e), that a function $\hat{x}(z)$ can be uniquely determined from (61) for *all* values of z on $0<z\leqq\bar{z}$, so as to be independent of z_o, continuous and increasing for all z, and differentiable for $z\neq\hat{z}$. In particular,

$$\lim_{z\to 0} \hat{x}(z) = 0 \ , \qquad \hat{x}(\hat{z}) = \hat{x} = g(\hat{z}) \ . \tag{62}$$

Moreover, since any feasible x_t is by (35 c) continuous to the right, and since for any superior path $\hat{t}(z)$ is continuous and monotonic, $\hat{x}^*(z)$ must be continuous to the right if $z_o<\hat{z}$, to the left if $z_o>\hat{z}$. Hence $\hat{x}^*(z)=\hat{x}(z)$ for every value of z in its domain, and the asterisk can now be omitted from $\hat{x}^*(z)$.

Once $\hat{x}(z)$ has been determined in the manner indicated, one reintroduces the time variable $t=\hat{t}(z)$ by

$$t = \int_{z_0}^{z} \frac{d\hat{t}(y)}{dy}\, dy = \int_{z_0}^{z} \frac{dy}{g(y)-\hat{x}(y)} \equiv \hat{t}(z) \ . \tag{63}$$

The function $\hat{t}(z)$ and its inverse $\hat{z}_t$ are monotonic and differentiable with the proper range and domain in each case because, by (61) and the monotonicity of $\hat{x}(z)$,

$$g(z)-\hat{x}(z) = \frac{\hat{u}-u\big(\hat{x}(z)\big)}{u'\big(\hat{x}(z)\big)} \begin{Bmatrix} > \\ < \end{Bmatrix} 0 \text{ if } \begin{cases} 0 < z_0 < \hat{z} \\ \hat{z} < z_0 < \bar{z} \end{cases} .$$

Hence $\hat{x}_t$ is monotonic and differentiable. In order to see that $T=\infty$ whenever $z_0 \neq \hat{z}$, one readily computes from TAYLOR expansions of $g(z)-g(\hat{z})$ with respect to $z-\hat{z}$, and of $g(\hat{z}(x))-\hat{x}$ with respect to $x-\hat{x}$, that

$$\lim_{z\to\hat{z}} \frac{g(z)-\hat{x}(z)}{z-\hat{z}} = -\left(\frac{g''(\hat{z})\,u'(\hat{x})}{u''(\hat{x})}\right)^{\frac{1}{2}} ,$$

a negative real number. It follows that

$$\text{if } z_0<\hat{z}\,,\ \lim_{z\to\hat{z}-0} \hat{t}(z)=\infty\ ,\quad \text{if } z_0>\hat{z}\,,\ \lim_{z\to\hat{z}+0} \hat{t}(z)=\infty\ .$$

Therefore $T=\infty$. The proofs of (C) and (D) are thereby complete. In addition, we note that $\hat{x}(z)$ is differentiable also for $z=\hat{z}$.

A 7. PROOFS FOR A POSITIVE DISCOUNT RATE $(0<\rho<f'(0)-\lambda)$

Proof of (E). Let (x_t, z_t) be a feasible path with $x_t \geqq \underline{x} > 0$ for all t. In (43) we insert $x_t^* = \underline{x}$, $z_t^* = \underline{z} < \hat{z}$ such that $g(\underline{z}) = \underline{x}$ Then, if $u(\underline{x}) \equiv \underline{u}$, $u'(\underline{x}) \equiv \underline{u}'$ we have $\underline{u} \leqq u(x_t)$ and hence, for $0 \leqq T < T^* < \infty$,

$$0 \leqq \int_T^{T^*} e^{-\rho t}\left(u(x_t)-\underline{u}\right)dt \equiv {}_T V_{T^*}(\rho) - (\underline{u}/\rho)\left(e^{-\rho T}-e^{-\rho T^*}\right) \leqq$$

$$\leqq \underline{u}' \cdot |g'(\underline{z})-\rho| \cdot (\bar{z}/\rho)\left(e^{-\rho T}-e^{-\rho T^*}\right) + \underline{u}'\,\bar{z}\left(e^{-\rho T}+e^{-\rho T^*}\right),$$

hence $\lim_{T,T^*\to\infty} {}_T V_{T^*}(\rho) = 0$ whenever $\rho>0$.

Proofs of (F), (G). These propositions express, and provide economic interpretation for, the inequalities (43) if we take $T = 0$, $T^* \to \infty$, and if the « candidate-optimal » path $(\hat{x}_t, \hat{z}_t)$ is substituted for (x_t^*, z_t^*). This is seen by reference to the definitions (21), (22) of the implicit prices p_t, q_t of the consumption good and of the use of the same good as capital good, respectively. Proposition F represents the first inequality in (43 *a*), which does not require feasibility of (x_t, z_t). The inequality in Proposition G is obtained from the fourth member of (43 *a*) by using (42), the equality through integration by parts.

Proofs of (H), (I), (J). Proposition (I) states two conditions (α), (β), as necessary and sufficient for the optimality of a path $(\hat{x}_t, \hat{z}_t)$. We shall first look at the implications of condition (β) in isolation. Called the *Euler condition* in the « calculus of variations », this condition is, for a path denoted just (x_t, z_t),

$$(64) \qquad q_t + \dot{p}_t = u'(x_t)\left(g'(z_t) - \rho\right) + u''(x_t) \cdot \dot{x}_t = 0 \quad \text{for all } t \geq 0 .$$

Together with the identity (36) this condition leads to the system of differential equations

$$(65) \qquad \left\{ \begin{array}{ll} (65a) & \dot{z}_t = g(z_t) - x_t \\ (65b) & \dot{x}_t = -\dfrac{u'(x_t)}{u''(x_t)}\left(g'(z_t) - \rho\right) , \end{array} \right\} \; t \geq 0 ,$$

for the solution of which we have a prescribed initial value z_0 of z_t, but as yet no given value of x_0. Figure 16 partitions

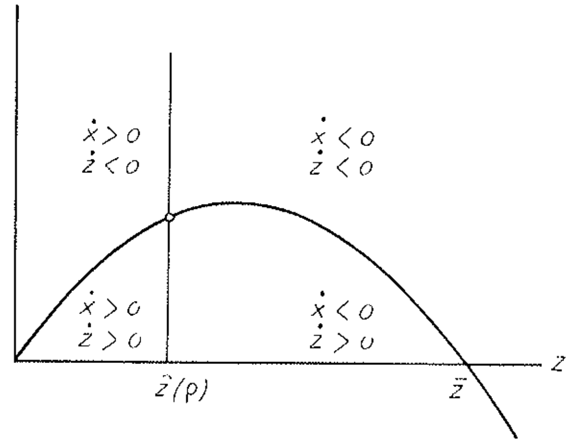

FIG. 16

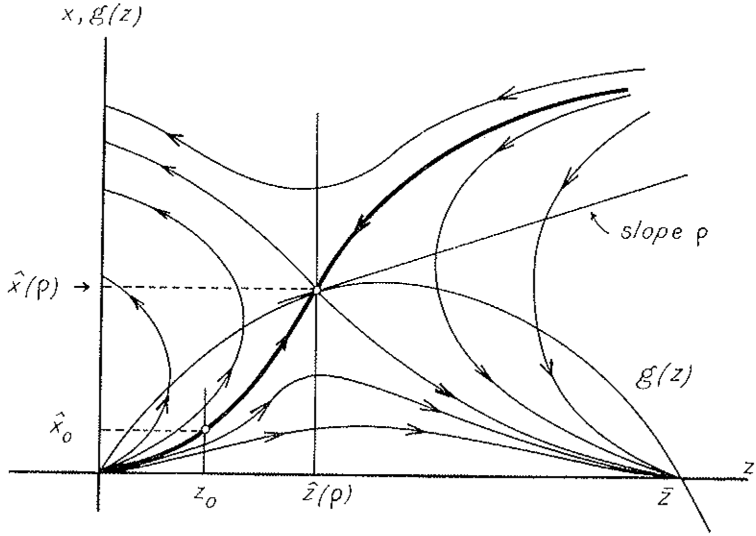

FIG. 17

the quadrant $x>0$, $z\geqq 0$ according to the signs of $\dot{x}_t$, $\dot{z}_t$ that follow from (65). Figure 17 sketches the trajectories of the point (x_t, z_t) starting from arbitrary initial values (x_o, z_o). Each trajectory is defined as the solution $x(z)$ with $x(z_o)=x_o$, or $z(x)$ with $z(x_o)=z_o$, of the corresponding differential equation

$$(66)\qquad \frac{dz}{dx}=-\frac{u''(x)}{u'(x)}\cdot\frac{g(z)-x}{g'(z)-\rho}\,,\quad\text{or}\quad \frac{dx}{dz}=-\frac{u'(x)}{u''(x)}\cdot\frac{g'(z)-\rho}{g(z)-x}\,,$$

respectively, obtained from (65) by elimination of t. Any segment of any trajectory defines a path optimal on a suitable time interval with prescribed initial and terminal values z_o, z_T of z_t. If we prescribe only z_o and examine the trajectories for various x_o, we find that there is one unique value $\hat{x}_o$ of x_o which together with z_o identifies a trajectory (shown in the diagram as a heavier line) that meets condition (α) of Proposition (I) of an asymptotic approach, for $t\to\infty$, to $(\hat{x}(\rho), \hat{z}(\rho))$ as defined in (60), Proposition (H).

We now denote the path resulting from that particular choice $\hat{x}_o$ of x_o by $\hat{x}_t$, $\hat{z}_t$). If $z_o<\hat{z}(\rho)$, the initial consumption flow $\hat{x}_o$ leaves room for growth in the capital stock per worker, and both $\hat{x}_t$ and $\hat{z}_t$ increase with t to approach their asymptotic values $\hat{x}(\rho)$, $\hat{z}(\rho)$, respectively, as $t\to\infty$. If $z_o>\hat{z}$, both $\hat{x}_t$, $\hat{z}_t$ decrease, and approach the same asymptots from above. Finally, if $z_o=\hat{z}(\rho)$ we must have $\hat{x}_t=\hat{x}(\rho)$, $\hat{z}_t=\hat{z}(\rho)$ for all $t\geqq 0$.

Since $\hat{x}_t$ approaches the positive number $\hat{x}(\rho)$ as $t\to\infty$, p_t is by its definition (21) asymptotic to $e^{-\rho t}\,u(\hat{x}(\rho))$. Hence, in (30), $\lim\limits_{T\to\infty} p_T(z_T-\hat{z}_T)=0$ by (40), and $(\hat{x}_t, \hat{z}_t)$ is optimal. Moreover, if (x_t, z_t) differs from $(\hat{x}_t, \hat{z}_t)$, we must have $x_t\neq\hat{x}_t$ for some t, because in the contrary case (36) and $z_o=\hat{z}_o$ would imply $z_t=\hat{z}_t$ for all t. But then, by the attainability condition (35 c), we have a strict inequality in (24) and, by (25), a strict inequality in (30), hence (x_t, z_t) is not optimal. Therefore $(\hat{x}_t, \hat{z}_t)$ is uniquely optimal for the given z_o. This completes the proof of Propositions (H), (I), (J).

For completeness we consider two additional questions. If (x_t, z_t) is eligible and attainable, we have from (2)

$$\int_0^T p_t x_t \, dt = \int_0^T p_t \left(g(z_t) - \dot{z}_t \right) dt = \int_0^T \left(p_t \, g(z_t) + \dot{p}_t \, z_t \right) dt - p_T \, z_T \; .$$

Examining the behavior of p_t and $\dot{p}_t$ for $t \to \infty$ one finds from this formula that all the integrals occurring in statements (i), (ii), (iii) interpreting (F) and (I) converge for $T \to \infty$.

Finally, what rules out the trajectories in Figure 17 for which $x_o \neq \hat{x}_o$? Those with $x_o > \hat{x}$ reach the boundary $z = 0$ at some finite time, making it impossible to satisfy both (65) and (35 a) for all $t \geqq 0$. For each x^*_0 with $0 < x^*_0 < \hat{x}_o$, there is a unique attainable path (x^*_t, z^*_t) satisfying (65) for all $t \geqq 0$, but in such a way that $\lim_{t \to \infty} x^*_t = 0$. This must entail either the ineligibility of (x^*_t, z^*_t), or the unboundedness of p^*_t associated with that path by (21), because otherwise (30) with (x^*_t, z^*_t, p^*_t) substituted for $(\hat{x}_t, \hat{z}_t, p_t)$ would imply the optimality of a path (x^*_t, z^*_t) already proved nonoptimal.

A 8. PROOFS FOR A NEGATIVE DISCOUNT RATE $(\rho < 0)$

We shall need the following lemma.

LEMMA 4. *If* $\varphi(x)$ *is a positive and nonincreasing function of* x *defined for all* $x > 0$, *and if* x_t *is a positive integrable function of* t *on the interval* $[T^1, T^2]$, $T^1 < T^2$, *such that*

$$(67) \qquad \int_{T_1}^{T_2} x_t\, dt \leqq (T_2 - T_1)\,\xi\ , \ \textit{where}\ \xi > 0\ ,$$

then

$$(68) \qquad \int_{T_1}^{T_2} \varphi(x_t)\, dt \geqq \frac{1}{2}\,(T_2 - T_1)\,\varphi(2\xi)$$

Proof: We define

$$\mu(x) \equiv \frac{1}{T_2 - T_1} \cdot \text{measure of } \{ t \mid T_1 \leqq t \leqq T_2 \text{ and } x_t \leqq x \}$$

Then $\mu(0) = 0$, $\mu(\infty) = 1$, and, from (67) and $x_t > 0$,

$$(69) \qquad \xi \geqq \frac{1}{T_2 - T_1} \int_{T_1}^{T_2} x_t\, dt = \int_0^\infty x\, d\mu(x) = \int_0^{2\xi} x\, d\mu(x) + \int_{2\xi}^\infty x\, d\mu(x) \geqq$$
$$\geqq 0 + 2\xi\,\big(1 - \mu(2\xi)\big)\ ,$$

Likewise, from the nonincreasing property of $\varphi(x)$,

$$(70) \qquad \frac{1}{T_2 - T_1} \int_{T_1}^{T_2} \varphi(x_t)\, dt = \int_0^{2\xi} \varphi(x)\, d\mu(x) + \int_{2\xi}^\infty \varphi(x)\, d\mu(x) \geqq$$
$$\geqq \varphi(2\xi)\,\mu(2\xi) + 0\ .$$

Since, from (69), $\mu(2\xi) \geqq \frac{1}{2}$, (70) implies (68).

[4] *Koopmans* - pag. 56

Proof of (K). We again distinguish the three cases with regard to the asymptotic range of z_t, used in the proof of (B).

Case (1), $\underline{\zeta}<\bar{\zeta}$. In this case Proposition (K) is equivalent to Lemma 2.

Case (2), $\underline{\zeta}=\bar{\zeta}\equiv\zeta\neq\hat{z}$. For definiteness assume $\zeta<\hat{z}$ and let $\hat{z}-\zeta\equiv 3\varepsilon$. Since now $\lim_{t\to\infty} z_t=\zeta$ we can choose T such that

$$(71)\qquad \hat{z}-4\varepsilon\leqq z_t\leqq z-2\varepsilon \quad \text{for} \quad t\geqq \mathrm{T}\ ,$$

and at the same time large enough for there to exist an attainable path (x_t^*, z_t^*) on [0, T] such that $z_{\mathrm{T}}^*=z_{\mathrm{T}}+\varepsilon$. For $t\geqq\mathrm{T}$ we choose (x_t^*, z_t^*) according to

$$(72)\qquad z_t^*=z_t+\varepsilon,\quad \dot{z}_t^*=\dot{z}_t,\quad x_t^*=x_t+g(z_t^*)-g(z_t)\ \text{for all}\ t\geqq\mathrm{T}.$$

Then, (x_t^*, z_t^*) is attainable throughout, and from (42), (71), for $t\geqq\mathrm{T}$,

$$(73)\qquad x_t^*-x_t=g(z_t^*)-g(z_t)\geqq g'(z_t^*)\ (z_t^*-z_t)\geqq g'(\hat{z}-\varepsilon)\cdot\varepsilon\equiv\eta>0.$$

Hence, for $\mathrm{T}<\mathrm{T}^*<\infty$ and $-\rho\equiv\sigma>0$,

$$(74)\qquad {}_{\mathrm{T}}\mathrm{W}_{\mathrm{T}^*}(-\tau)\equiv\int_{\mathrm{T}}^{\mathrm{T}^*} e^{\sigma t}\left(u(x_t^*)-u(x_t)\right)dt\geqq e^{\sigma\mathrm{T}}\int_{\mathrm{T}}^{\mathrm{T}^*} u'(x_t^*)\cdot(x_t^*-x_t)\,dt\geqq$$

$$\geqq \eta\ e^{\sigma\mathrm{T}}\int_{\mathrm{T}}^{\mathrm{T}^*} u'(x_t^*)\,dt$$

On the other hand, by (36), (40 *a*), (71),

$$(75)\qquad \int_{T}^{T^*} x_t^* \, dt = \int_{T}^{T^*} g(z_t^*) \, dt + z_T^* - z_{T^*}^* \leqq (T^* - T)\,\hat{x} + 2\varepsilon \leqq (T^* - T)\,\xi$$

provided $T^* - T \geqq 1$ and $\xi \equiv \hat{x} + 2\varepsilon$. It follows from (75) and Assumption (e) that x_t^* and $u'(x^*)$ when substituted for x_t and $\varphi(x)$ in Lemma 4 satisfy the premises of that lemma on the interval $[T, T^*]$. Hence, from (74), (68),

$$_{T}W_{T^*}(-\sigma) \geqq \frac{1}{2}\eta \cdot u'(2\hat{x} + 4\varepsilon) \cdot (T^* - T)\, e^{\sigma T}$$

from which (K) follows directly. The proof for $\hat{z} < \zeta$ is similar.

Case (3), $\underline{\zeta} = \bar{\zeta} = \hat{z}$. For any $\varepsilon > 0$, subject still to later choice, there now exists an integer T such that

$$(76)\qquad \hat{z} - \varepsilon \leqq z_t \leqq \hat{z} + \varepsilon \quad \text{for} \quad t \geqq T\,.$$

It will be useful to write $_{T}W_{T^*}(-\sigma)$ as the difference of two integrals

$$(77)\qquad _{T}W_{T^*}(-\sigma) = \int_{T}^{T^*} e^{\sigma t}\left(u(x_t) - \hat{u}\right) dt - \int_{T}^{T^*} e^{\sigma t}\left(u(x_t) - \hat{u}\right) dt = \\ = {}_{T}U_{T^*}^*(-\sigma) - {}_{T}U_{T^*}(-\sigma)\;.$$

Taking first the second term we have, from (43 a) with $x_t^* = \hat{x}$, (36) and (38 a).

$$
\begin{aligned}
{}_{\mathrm{T}}\mathrm{U}_{\mathrm{T}^*}(-\sigma) &\leqq \hat{u}' \int_{\mathrm{T}}^{\mathrm{T}^*} e^{\sigma t}\left(g(z_t) - \hat{x} - \dot{z}_t\right) dt \leqq -\hat{u}' \int_{\mathrm{T}}^{\mathrm{T}^*} e^{\sigma t} \frac{d}{dt}(z_t - \hat{z})\, dt = \\
(78) \qquad &= -\hat{u}' \left[e^{\sigma \mathrm{T}^*}(z_{\mathrm{T}^*} - \hat{z}) - e^{\sigma \mathrm{T}}(z_{\mathrm{T}} - \hat{z}) - \sigma \int_{\mathrm{T}}^{\mathrm{T}^*} e^{\sigma t}(z_t - \hat{z})\, dt \right] \leqq \\
&\leqq \hat{u}' \varepsilon \left(e^{\sigma \mathrm{T}^*} + e^{\sigma \mathrm{T}} + (e^{\sigma \mathrm{T}^*} - e^{\sigma \mathrm{T}}) \right) = 2\, \hat{u}' \varepsilon\, e^{\sigma \mathrm{T}^*}
\end{aligned}
$$

For the first term in (77) we choose an attainable path (x_t^*, z_t^*) which for $t \geqq \mathrm{T}$ is given by

$$
(79) \quad \begin{cases}
(79a) \quad \mathrm{T} \leqq t < \mathrm{T}^* - 1, & z_t^* = \hat{z} + \eta, \qquad \dot{z}_t^* = 0, \qquad x_t^* = g(\hat{z} + \eta), \\
(79b) \quad \mathrm{T}^* - 1 \leqq t < \mathrm{T}^*, & z_t^* = (\mathrm{T}^* - t)(\hat{z} + \eta) + (t - \mathrm{T}^* + 1) z_{\mathrm{T}^*}, \qquad \dot{z}_t^* = z_{\mathrm{T}^*} - \hat{z} - \eta, \qquad x_t^* = g(z_t^*) - z_{\mathrm{T}^*} + \hat{z} + \eta, \\
(79c) \quad \mathrm{T}^* \leqq t, & z_t^* = z_t, \qquad \dot{z}_t^* = \dot{z}_t, \qquad x_t^* = x_t,
\end{cases}
$$

where $\eta \geqq 3\varepsilon$ and the number $\mathrm{T}^* > \mathrm{T} + 1$ are still subject to later choice. In addition, T should be sufficiently large that, besides (76), there exists an attainable path (x_t^*, z_t^*) on [0, T] such that $z_{\mathrm{T}}^* = \hat{z} + \eta$.

To obtain a lower bound on the first term in (77) we note that, in view of Assumption (*c*) and (38 *b*), there exist numbers $\eta_o > 0$ and $\gamma > 0$ such that, whenever $0 < \eta \leqq \eta_o$,

$$
(80) \qquad |z - \hat{z}| \leqq \eta \quad \text{implies} \quad 0 \leqq g(\hat{z}) - g(z) \leqq \gamma\, \eta^2 .
$$

Hence, if $0 < \eta \leqq \eta_o$ and $u'_o \equiv u'(g(\hat{z} + \eta_o))$, we have from (79 *a*) and (38 *b*), in analogy to (78),

$$(81)\quad {}_{T}U^{*}_{T^*-1}(-\sigma) \geqq u'_0 \int_{T}^{T^*-1} e^{\sigma t}\left(g(\hat{z}+\eta)-g(\hat{z})\right)dt \geqq -u'_0 \gamma \eta^2 \sigma^{-1}\left(e^{\sigma(T^*-1)}-e^{\sigma T}\right) >$$

$$> -u'_0 \gamma \eta^2 \sigma^{-1} e^{\sigma T^*} .$$

For $T^* - 1 \leqq t < T^*$, x^*_t varies and must be boxed in. If $0 < \varepsilon \leqq \varepsilon_o$, $0 < \eta \leqq \eta_o$, we have from (79 *b*), (38),

$$\underline{x}^* \equiv min\ \{g(\hat{z}-\varepsilon_o),\ g(\hat{z}+\eta_o)\} - \varepsilon_o \leqq x^*_t \leqq \hat{x} + \varepsilon_o + \eta_o \equiv \bar{x}^* ,$$

where ε_o, η_o are chosen small enough to make $\bar{x}^* > 0$. Then, because $u'(x)$ decreases with x,

$$\underline{u}' \equiv max\ \{u'(\underline{x}^*),\ u'_0\} \geqq u'(x^*_t) \geqq u'(\bar{x}^*) \equiv \underline{u}' > 0 \text{ for } T^* - 1 \leqq t < T.$$

We therefore have from (79 *b*), (80),

$$(82)\qquad {}_{T^*-1}U^*_{T^*}(-\sigma) \geqq \int_{T^*-1}^{T^*} e^{\sigma t} u(x^*_t)\left(g(z^*_t)-g(\hat{z})-\dot{z}^*_t\right) dt \geqq$$

$$\geqq -\bar{u}' \gamma \eta^2 e^{\sigma T^*} + \underline{u}' \cdot (\eta - \varepsilon)\, e^{\sigma(T^*-1)} .$$

Pulling together these inequalities we have, for any $T^{**} \geqq T^*$, from (79 *c*), (77), (78), (81), (82), since $\bar{u}' \geqq u'_0 > \hat{u}'$,

$${}_{T}W_{T^{**}}(-\sigma) \geqq \left|-\bar{u}'\left(\gamma \eta^2 (1+\sigma^{-1}) + 2\varepsilon\right) + \underline{u}'(\eta-\varepsilon)\, e^{-\sigma}\right| e^{\sigma T^*} \equiv A\, e^{\sigma T^*}, \text{ say} .$$

It is now possible, within the restrictions already imposed, to choose first η and then ε small enough to make $A > 0$, next to choose T to correspond to ε according to (76), and finally, given $N > 0$, to choose T^* large enough to make

$$W_{T^{**}}(-\sigma) = W_T(-\sigma) + {}_T W_{T^*}(-\sigma) \geqq W_T(-\sigma) + Ae^{\sigma T^*} > N$$
$$\text{for all } T^{**} \geqq T^* .$$

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DISCUSSION

DORFMAN

I feel very strongly moved to express my admiration for Prof. KOOPMANS' paper. As Dr. JOHNSON might have said, its profundity is equalled only by its ingenuity. And it reaches some remarkable and exceedingly significant conclusions.

It is important to point to two quite strong assumptions on which the conclusions rest. One of them is that Prof. KOOPMANS has replaced the three factors of classical economics — land, labour and capital — by a two factor model. He omits land. The technical effect of introducing land into Prof. KOOPMANS' model would be to change one of the essential mathematical functions. Since land is in virtually fixed supply, it would no longer be permissible to think of constant returns to scale to the two variable factors. I can only conjecture the consequences of that change, but perhaps it would strengthen the moral case for saving on the part of the present generation because they will be better endowed per capita with the third factor than future generations if population continues to grow.

The other strong assumption made by Prof. KOOPMANS is that capital does not depreciate. I think it was RICARDO who originally defined land to be the original and indestructible powers of the soil. We have since generalized this concept to mean the original and indestructible power of anything that we inherit. The findings of capital theory indicate that if either of those two adjectives is the more important, it is « indestructible ». Whether a good possesses original powers or not is a matter of the past, which is now dead. From the forward looking point of view, the essential difference between capital and land or natural resources is that land does not require maintenance, but capital does, so that what Prof. KOOPMANS has been calling capital here might really come closer to being a kind of land or what would be from the point of view of future generations an increase in their supply of natural resources that they inherit from both their forebears and from nature itself.

Depreciation can be allowed for in this model by reinterpreting equation (10 *a*), the basic feasibility equation on page 11. We need only think of Prof. KOOPMANS' λ as being composed of $\lambda_1 + \lambda_2$, where λ_1 is the rate of population growth and λ_2 is the rate of capital deterioration. With this amendment, would not this same analysis apply to an economy in which population grew and capital deteriorated?

However this may be, the fact that capital depreciates does go against the strong conclusions that Prof. KOOPMANS arrived at because it means that an increment to the capital stock cannot be infinitely productive because it will decay, and it may carry with it a responsibility for permanent maintenance in the face of diminishing returns and perhaps also of diminishing marginal utility in the U functions. In that case, an economy can reach a state of capital saturation in the sense that, although the utility per capita resulting from an increase in the stock of capital does not fall to zero, it falls so low that it does not exceed the social cost of maintaining it. This consideration may lead to some changes in your conclusions and help avoid some of the problems created by the infinite horizon.

KOOPMANS

I agree to the comments made by Prof. DORFMAN that this exercise simplifies matters a great deal by ignoring land and depreciation of capital. Land would undoubtedly become a problem if population were to increase to such an extent that just space on which to exist would become very scarce, and it might actually become a problem well before that. It seems however that technology has so many possibilities for food production that are less land-intensive than the ones that are now being used, that there might be at least a temporary offset in technological progress.

As to the assumption that capital does not depreciate, I think this is a difference in degree but not in kind. As Prof. DORFMAN indicates you can indeed reinterpret λ as the sum $\lambda = \lambda_1 + \lambda_2$ where λ_1 refers to population growth and λ_2 to capital depreciation.

MALINVAUD

I should like to argue that Prof. KOOPMANS has been quite wise in these kinds of simplifications. We are certainly not living in a one commodity world in which capital would not depreciate. But some of the difficulties of intertemporal choices will appear with full clarity in very simple models. At the present stage of our research we are therefore justified to study carefully and exhaustively such simple models.

In particular it seems to me that difficulties occurring in more complex models have not been fully understood in their natures, because several sources of complications mixed their effects, and the origin of each new result was not clear. From the point of view that interests Prof. KOOPMANS, I do not think we should reach different qualitative conclusions if we introduce many commodities, if we assume that capital depreciates, or if we take into account the fact that land is not reproducible.

PASINETTI

I have very much enjoyed Prof. KOOPMANS' skilful, elegant and perspicuous analysis. Yet, I find it hard to accept his conclusions, which seem to me best summarized by the title of his section 7

(« adjusting preferences to opportunities »). Prof. KOOPMANS has found that the traditional approach to optimal growth (which consists of maximizing utility over time, by accepting a certain rate of discount of utility, called ρ, as given by individual preferences) cannot always be applied. More precisely, he has found that it can be applied only when the time horizon considered is finite. When time is allowed to run from 0 to ∞, then $\rho < 0$ becomes impossible (although $\rho \geq 0$ still remains possible) because there simply would not exist a utility function to be maximized.

Thus — Prof. KOOPMANS concludes — the open-endedness of the future imposes limits on individual preferences. He seems to be so surprised and even so afraid of this result as to prefer, at this point, to begin to speculate on the meaning of all this.

I would suggest that the mathematical exercise should be completed, by allowing time to run from $-\infty$ to $+\infty$ (and not only from 0 to $+\infty$). I may add perhaps that to consider time as running from $-\infty$ to $+\infty$ does not mean allowing time to run in reverse. It simply means putting ourselves in a slightly different position with respect to the one Prof. KOOPMANS has chosen. Instead of saying, as he does: suppose we begin our process of maximization at time zero, whatever happened before; we say: suppose that optimization has been taking place since the beginning of time. (This, by the way, appears to me a more logical approach to take in the context of Prof. KOOPMANS' stationary society). Now, if we allow time to run from $-\infty$ to $+\infty$, it is easy to see that, in a stationary economic system, also $\rho > 0$ becomes impossible. The only value of ρ that makes any process of utility maximization over infinity possible is $\rho = 0$.

Prof. KOOPMANS might be even more surprised. For, by following his arguments, we should conclude that individuals have not even a limited inter-temporal preference choice: they have no choice at all.

But is it so? This conclusion — it seems to me — is fallacious, although of course the mathematical results are correct. And the fallacy stems from not bringing out explicitly the implications of the following *theorem*: on the optimum growth path (by which I mean

Prof. KOOPMANS' « golden rule path »), the rate of interest of the economic system is determined independently of consumers' utility functions. This theorem follows from Prof. KOOPMANS' own analysis. For, on the optimum path, the rate of interest must be equal to the *natural* rate of growth (which, in the particular case of a stationary economic system, is equal to zero).

But this simply means that individuals' preferences are not a determinant of the optimum rate of interest. It does not mean (as Prof. KOOPMANS seems to fear) that any restriction comes to be imposed on individuals' preferences. To argue otherwise would sound to me rather similar to saying, in the usual case of a one-period problem of utility maximization, that the fact that market prices are the same for all consumers imposes restrictions on individual preferences. Traditional economic theorists have solved this problem a long time ago, by referring their analysis only to what happens at the margin. Individual preferences are accepted for what they are, however different from one individual to another they may be. Yet, given these preferences, each individual will push the consumption of each commodity to the point at which the ratios of marginal utilities are equal to relative market prices. This means that we can make definite statements about ratios of *marginal* utilities, without imposing any restriction on utility functions.

Our case is similar. Consumers' preferences may be quite different at different levels of consumption, at different times, and for different individuals. Any social preference function expressing all these preferences may be equally different; and it must be accepted for what it is, without any restriction. Yet, if behaviour is to be rational, the consumption of each commodity will be distributed over time so as to equate *marginal* intertemporal rates of substitution in consumption to the externally given rate of interest. This is all we can say.

Professor KOOPMANS' results have been obtained because he has added something else. He has imposed on utility functions *at all levels* (and not only at the margin!) the restriction that utility always differs by ρ at any two adjacent points of time. This restriction is arbitrary, and has no justification; and I would interpret the results of his mathematical analysis as simply showing the impossibility of such an arbitrary restriction. I should conclude, therefore, that the misunderstanding has arisen from having introduced a ρ into the analysis at all. For, this has meant introducing exactly what Prof. KOOPMANS has been afraid of, namely restrictions on consumers' preferences.

KOOPMANS

I do not understand the operational meaning of Prof. PASINETTI's suggestion to maximize utility over a period from $-\infty$ to $+\infty$. The following comments apply therefore to maximization from 0 to ∞, although I may thereby fail to do justice to PASINETTI's thought.

In the sentence in which Prof. PASINETTI refers to the « golden rule path », he uses the term optimum in a sense different from mine. If an optimal path is defined as one that maximizes a utility function of the type I have discussed, the golden rule path is optimal only if both (*a*) the initial ratio of capital stock to labor force happens to coincide with that characteristic of the golden rule path, and (*b*) the chosen utility function has no discounting ($\rho=0$). If both these conditions are satisfied, the golden rule path is optimal in my sense as well, and as PASINETTI observes the interest rate $\rho+\lambda$ equals the exogenously given growth rate λ of the labor force. However, if even only one of these conditions fails to hold, the optimal path, if one exists, differs from the golden rule path, and the interest rate differs from λ most or all of the time, and is determined by the interplay of preferences and production possibilities I have analyzed.

Finally, Prof. PASINETTI's analogy with the one-period problem of utility maximization misses the main point of my paper. In the one-period problem with a finite number of commodities, an optimal consumption choice is bound to exist if the utility function is continuous (a slight restriction on preferences!) and the opportunity set closed and bounded. In the infinite-horizon case, there is a new mathematical situation, and the existence of an optimal program is found to depend on a stronger restriction on the utility function used. To facilitate analysis, I have studied this restriction only within the class of stationary and additive per capita utility functions, expressible as a sum of future per capita utilities derived from a constant one-period utility function $u(x)$ and discounted at a constant rate ρ. Within that arbitrarily chosen class, an optimal path is found to exist if and only if $\rho \geqq 0$. The question of existence of an optimal path is so far unresolved within the wider class of continuous but not necessarily stationary or additive utility functions. It is plausible to assume that within that class there will again be a subclass for which, in a given technology, no optimal path exists.

MORISHIMA

Your argument is based on the assumption that the rate of growth of population is constant. This, together with others, implies the uniqueness of the Golden Rule path. Suppose, instead, that the rate of growth of population is an increasing function of the consumption per capita until it reaches a certain level, after which the population growth-rate will decrease. Then there are possibilities of multi-Golden Rule paths. You shall have to be concerned with the comparison between Golden Rule paths (the best Golden Rule path, the second best Golden Rule path and so on) and also with the locality of the stability of the best Golden Rule path.

You treat capital and labour in an asymmetric way: capital may be unused if too much capital is available, while labour is fully employed throughout the whole process. May I say that a certain degree of optimality has already been presupposed in your assumption of automatic maintenance of the full employment of labour? Is the full employment of labour maintained even if labour is treated in the same manner as capital, i.e. if there is a possibility of unemployment of labour?

KOOPMANS

I want to thank Prof. MORISHIMA for the information he gave us for a model in which the rate of population growth is a function of consumption per head, initially increasing and thereafter decreasing. This is, I think, a realistic generalization that it is important to have.

On his second point, the asymmetry in treating capital and labour with regard to disposal, I would agree that that is something not very consistent in the paper as presented. I do not think it has affected any of the results in any of the optimal paths being considered — paths in which at all times the capital stock is fully used as much as the labour force (1).

MAHALANOBIS

I am very deeply impressed by the approach and the broader results of this paper. It seems to me that although a very simple model has been used on, if you like, somewhat intuitional grounds, the conclusion seems to be extremely important from the point of view of the underdeveloped countries. The conclusion to which I am referring is that neutrality regarding timing between generations is not possible, the very nature of the process of development or of industrialization discriminates in favour of future generations. Thus I am taking to be a basic point, on page 28, the argument that accumulation of capital, permitting a higher output of consumption goods in later years must discriminate in favour of future generations. The broad conclusions may have important educative effects regarding programmes in underdeveloped countries.

I am not going into details of technical arguments, but suppose instead of the one commodity model we have three types of capital

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(1) Note added after the conference: In the corrected version of the paper printed in the volume, disposal of capital use has been excluded.

goods producing three types of consumer goods; the basket changes but there need not be any change in the total aggregation of capital goods which goes on increasing. The one-commodity model seems to me to be illustrative, and the conclusions may remain valid even if we have many commodities. This question, of course, cannot be settled without going through the full exercise of rigorous analysis.

There are three points which have been summarised in sections 5 and 6. The point about open end I think has been clearly brought out and is logically valid.

I am repeating the question again: whether one commodity or many commodities would affect that result. Theoretically some preference functions may change. The order with which Prof. KOOPMANS started, that pattern of ordering, may also change. But I think Prof. KOOPMANS took care to point out that some kind of good ordering is all that he was keeping in mind.

A further point which is discussed on page 32, that it is likely that technological advance would continue; even if it slows down, even then, from the point of view of the present generation, the discrimination would be in favour of future generations. The question of basic decision is not whether the hundredth generation from now would be in a somewhat less favourable position; the decreasing return in terms of generations may be there even if the technological advance continued at the same rate. I do not see this as a very important consideration for present decisions.

This particular paper seems to me to be of value because of the wider implications of this Study Week. It is not a Study Week arranged by a specialist econometric society but by the Pontifical Academy of Sciences. My interest, as I have continually stressed, is in the broader implications. And from that point of view I welcome this paper as likely to have a very valuable educative effect.

It is more difficult to speak on the question of population size. It may differ from country to country. Prof. KOOPMANS has pointed out that where the density of population is extremely small, there may be some advantage in increasing their number, but where the density of population is high the position may be different

KOOPMANS

I share the beliefs expressed by both speakers, Prof. MAHALANOBIS and Prof. MALINVAUD, that the one-commodity study, one-commodity model does a service here in isolating just those problems that come from the open-endedness of the future. Considering those apart from other complications which arise in two- or more-commodity models, I used the term « belief » because I do not feel that a stronger term can be used at this point. One would want to go on to two- and more-commodity models to examine whether that belief is valid but this is my present state of espectation.

On the question that Prof. MAHALANOBIS raised about flexibility in the ordering, that idea is merely mentioned at the end of my study. I have given some thought to the possibility of working from an ordering which has built into it the possibility of its own revision at a later time, and more particularly an ordering in which the decision maker is willing to make a present sacrifice of immediate satisfaction in order to leave open more doors in the future. He may, for instance, prefer to build a capital stock of such a nature that it can be applied to a wider range of different kinds of production, even though that kind of capital stock would not be the most economical one if the composition of production in the future were already fully specified. For somewhat longer-range planning this seems to me a matter of practical importance. My speculations on this are in a reference at the end of my paper, identified by the year 1962, and they did not carry very far. I believe that from the formal point of view it is a difficult matter to formalize this idea. I do find that the need for such an idea is strong enough to justify further formalizing effort in that direction.

FISHER

Prof. KOOPMANS has as always presented us with an illuminating and beautiful piece of work. I am, however, disturbed a bit at the conclusions that he wants to draw from it. He has shown that there may be severe conflicts between our ethical notions and the analysis of certain kinds of growth models. His conclusion is that we must change our ethical notions. My preference would be to say that there is something wrong with the model. In either case a good deal of further thought seems called for. More particularly, one frequently in the theory of optimization over time encounters peculiar difficulties when one uses an infinite time horizon. Now, in fact, the device of the infinite horizon was probably originally introduced because the choice of a finite horizon is an arbitrary one and because with a finite horizon one has difficulty in deciding what to do about terminal capital stock. Infinite horizons were, however, introduced primarily as a convenience. They have in several contexts now been shown to lead to difficulties all associated with divergence of the improper integral obtained in the problem. Now this suggests to me that infinite horizons are not in fact the convenience they appear. The obvious conclusion from KOOPMANS' paper, therefore, seems to me to be that one ought to abandon the use of infinite horizons — not that one ought to abandon certain ethical notions.

Now, of course, this may be wrong. It may turn out — and Prof. KOOPMANS assures me that it does — that even with a finite horizon one has similar problems which are not so severe. In that case, it may not be worth dropping infinite horizons. Still, the role of this sort of analysis is surely to tell us how one can best achieve one's ethical and social ends. In the course of analyzing that, it may turn out that such ends are unachievable. In such a case, one has to moderate one's ends. The usual circumstance, however, is that one's ends are not achievable in the sense of being contradictory whereas Prof. KOOPMANS has shown that our ends may be unachievable in the sense that no solution to the problem exists — that the whole analysis breaks down if one insists on certain kinds of ethical goals. This sort of circumstance does not persuade me to give up my ethical goals, but rather to refine the mode of analysis. I can understand that the end result may be that I will have to give up certain goals as unachievable, but the demonstration of that ought to be that they are unachievable and not that the problem becomes ill-defined.

Despite the fact that I am unable to agree completely with Prof. KOOPMANS' conclusions, however, I should like to state that his paper like that of Prof. MALINVAUD is a pleasure to read and to listen to. The two papers set a standard that one wishes all the other papers and discussion in this Study Week had met.

CROISSANCES OPTIMALES DANS UN MODELE MACROECONOMIQUE

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ORAL PRESENTATION (*)

I may be brief on the motivations for this paper after what was said by Professor KOOPMANS. Like him, I wanted to understand better the logical problems raised by choices between intertemporal program and, in particular, to deal, as rigorously as I could, with the case when the utility is defined over an indefinite future. Moreover, I wanted to explore the relations between various particular models, proposed by RAMSEY, TINBERGEN, RADNER and SRINIVASAN, all dealing with the choice of optimal programs, but starting from different hypotheses.

In this oral presentation, I shall briefly survey the various sections of my paper.

After a short introduction in section 1, section 2 defines the model. Like Professor KOOPMANS, I am dealing with a world

(*) Le texte de la présentation orale est reproduit à titre de résumé en langue anglaise du mémoire presenté a partir de la page 9.

in which there is just one good that can be used either for consumption or as capital in production. Notwithstanding differences in notation, my model is very similar to his. I am, however, considering a discrete time represented by the index $t = 0, 1, 2 \ldots$ ad infinitum.

C_t is aggregate consumption during period t, i.e. from time t to time $t+1$. Similarly N_t measures the labor services provided in period t. Equation (1), page 11, defines per capita consumption c_t and per capita input of labor n_t. Capital at time t is denoted as K_t, and the stock of good available at time t before consumption as $S_t = K_t + C_t$. One constraint of the model specifies that the initial stock is equal to a given number S_0. Another constraint results from the production function, namely equation (6).

Among all programs which are feasible, I am using the same kind of utility function as Professor KOOPMANS does, except that, in some parts but not everywhere in my paper, I am taking the per capita labor input as an argument of the utility function, thus allowing the amount of labor services provided to be determined simultaneously with consumption by the choice of the optimal program.

In order to make possible a choice among infinite programs I am not using the technique presented by Professor KOOPMANS, but relying on the criterion given by my definition 1, page 17, namely:

A feasible program $\mathscr{P}^1$ is optimal if there is no value of T and no other feasible program $\mathscr{P}$ such that the inequalities (14) and (15) be simultaneously fulfiled.

Taking this as a definition, I am avoiding considering infinite sums that might not converge.

Section 3 is devoted to the determination of sufficient conditions for a program to be optimal. At this stage the model remains general except for some assumptions on the production and utility functions, notably that they be concave and possess partial derivatives.

In the derivation of these conditions, inequality (21), page 20, plays an essential role. It permits a comparison between a feasible program $\mathscr{P}$ and another feasible program $\mathscr{P} + \delta \mathscr{P}$; it might be written as:

$$\delta \mathcal{U}_T \leqq \sum_{t=0}^{T-t} (\alpha_t \delta K_t + \beta_t \delta N_t) - \lambda_T \delta S_T$$

α_t, β_t and λ_T depending on the values of the variables in $\mathscr{P}$. The coefficient λ_T being non-negative, $\mathscr{P}$ is optimal if the α_t's and β_t's are equal to zero and if $\delta S_T \geqq 0$ for any T and any feasible program $\mathscr{P} + \delta \mathscr{P}$ such that $t \geqq T$ would imply $\delta U_t \geqq 0$.

This is essentially what proposition 1 amounts 2. I first define as « *regular* » a feasible program for which the α_t's and β_t's are all zero; this is equivalent to requiring that the equations (22) be fulfiled. I then specify a condition 1 that automatically implies the condition quoted at the end of the preceding paragraph. Condition 1 requires that, in $\mathscr{P}$, the marginal utility of consumption be positive at all times and that there exist a number h larger than 1 such that, at least for large t:

$$(1 + f'_{tK}) \frac{S_t}{S_{t+1}} \geqq h \qquad (23)$$

f'_{tK} being the marginal productivity of capital at time t. Proposition 1 states that a regular program that satisfies condition 1 is optimal.

Let me point out here that any regular program would appear as optimal if time were restricted by a finite horizon T and if the values of both S_0 and S_T were taken as boundary constraints. Condition 1 is specific to the consideration of unlimited programs. We shall have occasions to see in the following examples that it is not vacuous.

We may also observe that the equations $\alpha_t = 0$, $\beta_t = 0$ imply together with the equilibrium condition at time t a set of difference equations which may usually be solved recursively for all C_t, K_t, N_t starting from any given allocation of S_0 between C_0 and K_0. Thus, the regular programs generate a one parameter family of programs, the parameter being C_0 (or equivalently K_0, S_0 being given). Typically, one and only one program of this family will meet condition 1 and therefore be optimal. A possible computational procedure would be first to determine the regular program corresponding to any fixed initial allocation, and then to find by trials and errors the initial allocation that leads to fulfilment of condition 1.

Before turning to specific examples, we may look at the economic interpretations of equations (22), stating that the α_t's and β_t's are all zero, and of condition 1.

The first equation (22) may be written as:

$$(34) \qquad 1 + f'_{tK} = (1 + \varepsilon)\,(1 + \pi_t)\,(1 + u_t)$$

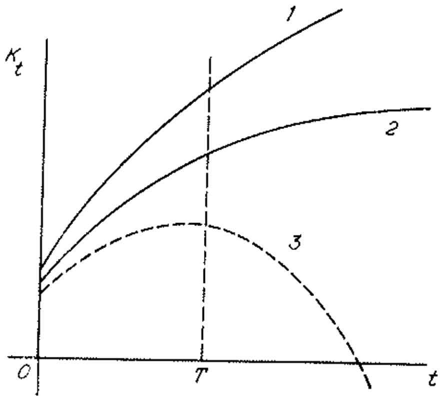
f'_{tK} being the marginal productivity of capital, or the equilibrium rate of interest, ε the social rate of interest used for the discount factor of the utility function, π_t the rate of increase of the population, and u_t the rate of *decrease* of the marginal utility of consumption. This equation exhibits in a suggestive way the three components which explain the equilibrium rate of interest. It is similar to relations recently put forwards by Sir Roy Harrod and Professor Ragnar Frisch.

Condition 1 implies that the equilibrium rate of interest be larger than the rate of increase in the stock of good S_t, and therefore that the present value of S_t decreases to zero with t.

In section 4, I present an application of the above analysis to an aggregate version of the model recently built by ROY RADNER.

Population and labor input increase at a constant rate. The production function is such that S_t appears as a COBB-DOUGLAS function of N_t and K_t with neutral technical progress expanding at a constant rate. The period utility function is proportional to the logarithm of c_t.

In this model, the recurrence equations (22) can be solved explicitly to give equation (43). The regular programs are thus determined. For instance, the diagram here shows the evolution of K_t for three programs satisfying equation (43) but starting from different initial allocations, the consumption C_0 increasing when we pass from program 1 to programs 2 and 3.



Programs 1 and 2 are feasible and therefore regular.

Program 3 is not feasible since K_t becomes negative after some time. Program 2 is definitely prefered to program 1 because it gives a higher value for C_t *at all times*. However, if the total period considered were restricted to a horizon T as shown iu the diagram, programs 1, 2 and 3 would appear as optimal, each one with respect to an appropriate terminal condition on K_T.

If the social interest rate ε, used in the definition of the utility function, is positive, then condition 1 is satisfied by the regular program $\mathcal{P}^2$ that gives the highest value to C_0. This program $\mathcal{P}^2$ is therefore optimal. Its optimality may still be proved directly for the case $\varepsilon = 0$. But, when the social rate of interest ε is negative, no program is optimal.

For instance, program $\mathcal{P}^2$ is preferred to the program $\mathcal{P}^1$ exhibited on the diagram. However a better program can be found as follows when $\varepsilon < 0$: up to time T, select a regular program close to $\mathcal{P}^2$ but allowing a little higher value of K_t, and therefore a little smaller value of C_t; at time T take an extra consumption by reducing K_T to its value in $\mathcal{P}^2$, thereafter continue with program $\mathcal{P}^2$. The program thus defined is not optimal either, because one would prefer to postpone ever farther in the future the time T at which one switches back to $\mathcal{P}^2$.

In section 5, I consider the case in which the production function would simply imply a fixed capital-output ratio. The determination of regular programs then boils down to the solution of a recurrence equation on c_t.

Choosing a type of utility function proposed by R. FRISCH and more recently used by J. TINBERGEN, I find that an optimal program exists only when the social rate of interest ε exceeds a positive minimum that may be of some 10% per annum. This suggests that, in the programming of future development, one can hardly avoid taking into account the decrease in the marginal productivity of capital, unless one discount heavily against the future.

Section 6 is devoted to the case of a linear utility function, a case that is often considered, for instance when one chooses to maximize a discounted sum of the future consumption stream.

The formulas found in section 3 are no longer applicable as such, because the solution of the recurrence equations leads to non-feasible programs. However, the general approach can be maintained if one introduces upper and lower bounds on consumption per head c_t and labor input per head n_t.

Depending on the values of the parameters, the shape of the optimal program varies a great deal. I consider precisely a few cases which may be of some interest. For instance, if the initial endowment of capital is small and if leisure has some value even when consumption is at its minimum, the optimal program may imply that the labor input be not pushed at its maximum in the first periods, but be increased progressively as capital accumulates, consumption being nevertheless kept at its minimum until a sufficiently high capital stock has been reached (see figure 5 in the text).

Notwithstanding the fact that it permits interesting insights, the assumption of a linear utility function is not quite satisfactory. In all cases, the optimal program exhibits some discontinuities in its time shape, discontinuities that go against common sense. For instance, consumption may switch in one period from its minimum to its maximum value.

This unsatisfactory feature is partly due to the simplifications made in the model, notably to the assumption of a one-commodity, one-sector world, and to the assumption of independence among the utilities for different periods. However, optimal programs in more elaborate models would present similar, even though less extreme, discountinuities as long as linear utilities would be assumed. The unescapable conclusion seems to be that, for the problems considered here, one must take into account the decrease in the marginal utility of consumption.

I end up, in section 7, with the model first studied by RAMSEY. I there show that my treatment with discrete time is really quite similar to the one proposed by RAMSEY for a continuous time model, and to the one exposed by Professor KOOPMANS a moment ago.

1. Introduction [1]

La programmation économique nous amène souvent à considérer des problèmes d'optimisation portant sur plusieurs périodes. En particulier, tout choix d'investissements suppose la prise en compte des conséquences futures de nos décisions présentes. Les modèles économétriques qui servent à éclairer ce choix font donc intervenir un nombre plus ou moins élevé de périodes.

Or il subsiste d'importantes lacunes dans l'étude théorique de ces problèmes d'optimisation intertemporelle. Par manque de principes bien établis, le praticien qui doit choisir et résoudre un modèle hésite souvent sur les formalisations et sur les hypothèses simplificatrices à retenir. Il connaît mal, notamment, le caractère plus ou moins limitatif des conditions terminales qu'il est amené à poser.

Le présent mémoire a pour objectif d'aider à l'édification d'une théorie qui puisse servir de guide aux travaux de programmation. Afin d'éliminer à ce stade des complications qui ne sont pas inhérentes à l'optimisation intertemporelle, je me contenterai d'une formalisation purement globale dans laquelle existera un seul bien produit. En revanche, j'introduirai une suite illimitée de périodes de manière à n'imposer a priori aucune condition terminale et à éviter ainsi un certain arbitraire.

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(1) Les recherches qui ont abouti à ce texte ont fait l'objet de conférences à l'École Nationale de la Statistique et de l'Administration Economique ainsi qu'à l'Ecole Pratique des Hautes Etudes en mai et juin 1963.

Je rechercherai d'abord une définition et une caractérisation des croissances optimales. Puis je présenterai une méthode qui, au moins en principe, peut suffire à la détermination de la croissance optimale. J'examinerai ensuite quelques cas particuliers qui ont été envisagés dans la littérature économétrique. Je montrerai alors qu'on ne saurait se contenter de formulations dans lesquelles soit la fonction d'utilité, soit la fonction de production seraient linéaires. Je traiterai enfin de deux modèles proposés respectivement [1] par R. RADNER (1962) et F.P. RAMSEY (1928).

2. LE MODÈLE

Soit une collectivité n'effectuant aucun échange avec l'extérieur. Son développement futur est décrit pour une suite de périodes séparées par les instants $t=0, 1, 2 \ldots$ ad infinitum. L'instant $t=0$ correspond à l'époque actuelle, ou plus précisément à l'époque initiale d'un plan de développement. Par convention, la période t est celle qui s'écoule entre les instants t et $t+1$.

L'évolution future de la population est considérée comme une donnée exogène, résultant de perspectives démographiques indépendantes de la croissance économique. Soit P_t la population prévue pour l'époque t.

Désignons par N_t la quantité de travail à fournir pendant la période t, et par C_t la consommation qui sera effectuée du-

(1) Les références sont rassemblées à la fin du mémoire p. 78.

rant cette période. Le travail d'une part, le bien produit et consommable d'autre part constituent les deux seuls biens introduits dans le modèle. Ils sont l'un et l'autre parfaitement homogènes. Dénotons par n_t et c_t le travail et la consommation par personne, soit:

$$(1) \qquad n_t = \frac{N_t}{P_t} \qquad c_t = \frac{C_t}{P_t}$$

Nous admettrons l'existence d'un maximum n_M à la quantité moyenne de travail par personne, et celle d'un minimum c_m à la consommation par personne. En d'autres termes, nous admettrons que la croissance future doit satisfaire aux inégalités suivantes:

$$(2) \qquad 0 \leqq n_t \leqq n_M$$

$$(3) \qquad c_t \geqq c_m$$

n_M et c_m étant deux nombres non négatifs donnés.

Soit K_t le capital à l'instant t, c'est-à-dire la quantité du bien produit qui est maintenue dans le système productif pour participer à la production durant la période t. Soit Q_t la production nette de cette période; Q_t est disponible à l'instant $t+1$. A cet instant, l'équilibre entre les ressources et les emplois pour le bien produit et consommable s'écrit:

$$(4) \qquad K_t + Q_t = K_{t+1} + C_{t+1} \; .$$

A l'instant o, le stock disponible du bien consommable est donné. Désignons le par S_0 et écrivons la condition d'équilibre:

(5) $$S_0 = K_0 + C_0 \ .$$

Pour représenter les contraintes techniques sur la production de la période t, nous admettrons l'existence d'une fonction de production [1]:

(6) $$Q_t = f_t\ (N_t\ ,\ K_t)$$

f_t étant une fonction donnée qui a priori peut varier d'une période à une autre. Nous supposerons que f_t est définie pour toutes valeurs non négatives de N_t et de K_t. Dans la suite, nous n'aurons pas besoin d'introduire Q_t explicitement. Nous remplacerons les égalités (4) et (6) par la suivante:

(7) $$C_{t+1} = K_t - K_{t+1} + f_t\ (N_t\ ,\ K_t)\ .$$

Nous appellerons « *programme* » $\mathcal{P}$ une spécification des valeurs données aux grandeurs N_t, C_t, n_t, c_t et K_t pour les instants successifs ($t = 0,\ 1,\ 2\ \ldots.$). Nous repèrerons différents programmes par des indices supérieurs. Ainsi $\mathcal{P}^1$ sera le programme correspondant aux valeurs N_t^1, C_t^1, n_t^1, c_t^1 et K_t^1.

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[1] Cette représentation des contraintes techniques est évidemment quelque peu restrictive. Avec un modèle dans lequel le nombre de biens serait quelconque, on pourrait représenter les produits intermédiaires et les produits en cours de fabrication par des biens particuliers. Définir les contraintes techniques à l'intérieur de chaque période n'introduirait alors aucune restriction réelle. Il en va différemment avec un modèle purement global. La formulation retenue ici suppose que la production de la période t est disponible en totalité à la fin de la période et pourrait éventuellement être consommé.

Nous dirons qu'un programme est « *possible* » s'il satisfait aux contraintes définies par les conditions (1), (2), (3), (5), (7) et par la suivante:

$$K_t \geqq 0 \,. \tag{8}$$

Le choix d'un plan de développement se traduit dans ce modèle par la détermination du programme qui peut être considéré, en un certain sens, comme « le meilleur de tous les programmes possibles ».

Pour donner une signification à cette expression, définissons tout d'abord « l'utilité » du programme pour la période t. Nous admettrons que cette utilité dépend seulement de la consommation et du travail par personne durant la période en question [1]; ce sera une fonction $U_t(c_t, n_t)$ qui, en principe, pourra varier d'une période à une autre.

Sous une réserve qui sera examinée ci-dessous, l'utilité du programme entier sera définie par l'expression:

$$\mathcal{U} = \sum_{t=0}^{\infty} \gamma^t U_t(c_t, n_t) \tag{9}$$

dans laquelle γ est un nombre positif, constituant un facteur d'escompte grâce auquel sont combinées les utilités U_t relatives aux différentes époques. Nous l'appellerons « facteur d'escompte normatif » et nous lui ferons correspondre un « taux d'intérêt normatif » ε défini par la relation:

$$\gamma = \frac{1}{1+\varepsilon} \tag{10}$$

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[1] Les préférences relatives à la période t seront ainsi indépendantes des valeurs prises par la consommation et la quantité de travail pendant les périodes autres que t. La nature de cette hypothèse a été précisée par G. DEBREU.

Avec la formulation générale présentée ici, l'introduction du facteur d'escompte γ est évidemment inutile puisqu'une modification dans la définition des U_t ramènerait l'expression (9) à une autre semblable dans laquelle γ^t aurait disparu. Mais la forme (9) facilitera l'application des résultats généraux au cas dans lequel la même fonction $U(c_t, n_t)$ sera retenue pour représenter les utilités relatives aux diverses périodes. Seul ce cas sera examiné dans les applications qui seront faites du modèle général [1].

L'introduction d'un facteur d'escompte différent de 1 peut d'ailleurs sembler contestable pour des raisons plus fondamentales qui prennent tout leur sens quand la même fonction $U(c_t, n_t)$ s'applique à toutes les périodes. Sans doute convient-il d'ailleurs de distinguer deux situations différentes.

S'il s'agit de décrire la croissance d'une société libérale, la fonction d'utilité doit représenter à l'échelle macroéconomique le résultat des choix individuels [2]. Depuis BÖHM-BAWERK il est admis, comme une loi psychologique, que les individus ont une préférence naturelle pour le présent et qu'ils attribuent dans leur choix une pondération d'autant plus faible aux époques futures qu'elles sont plus éloignées dans l'avenir. Si nous admettons cette thèse, qui n'a d'ailleurs jamais été établie de façon parfaitement convaincante, nous devons naturellement introduire dans la fonction d'utilité un facteur d'escompte γ plus petit que un.

Mais il s'agit ici de décrire les choix collectifs d'une économie planifiée et on peut trouver moins de raison à la présence d'un facteur d'escompte. On a fait valoir que l'utilité intervient

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(1) Une utilité de la forme

$$\sum_{t=0}^{\infty} \gamma^t U(c_t, n_t)$$

a été dite « stationnaire » par T. KOOPMANS (1960) qui a discuté la nature des préférences représentables par des fonctions de ce type.

(2) On connaît la gravité des problèmes d'agrégation qu'une telle représentation pose.

seulement pour les décisions de l'époque initiale; d'autres décisions seront prises dans l'avenir qui se traduiront par une révision continuelle du programme choisi à l'origine. Il peut dès lors sembler naturel que l'on attribue dans les choix présents la pondération la plus élevée aux conséquences immédiates, qui ne pourront guère être évitées. Cette argumentation ne semble cependant pas très forte. On peut préférer une éthique collective selon laquelle toutes les époques et toutes les générations recevraient la même attention dans les choix actuels. Notre formulation ne l'interdit évidemment pas. Il suffit de prendre $\gamma = 1$ dans l'expression de $\mathcal{U}$.

Certains économistes estiment que, pour combiner les utilités relatives aux diverses périodes, on doit tenir compte des effectifs des populations qui en profiteront. Ces économistes préfèrent à l'expression (9) la suivante:

$$(11) \qquad \sum_{t=0}^{\infty} \gamma^t \, P_t \, U_t \, (c_t \, , n_t)$$

(Comme ci-dessus, la distinction n'est réelle que si la même fonction $U(c_t, n_t)$ est retenue pour les diverses périodes). Le choix entre (9) et (11) pose une question d'éthique sociale sur laquelle il est difficile de se prononcer a priori. Le lecteur qui a une préférence pour l'expression (11) pourra adapter sans peine les résultats présentés ci-dessous. Nous nous en tiendrons dans la suite à des fonctions d'utilité définies conformément à la formule (9).

Toutefois cette formule ne serait satisfaisante que si était établie la convergence de la somme infinie qu'elle fait intervenir. La convergence est assurée dans certains cas présentant de l'intérêt: par exemple si γ est plus petit que 1 et si les fonctions U_t sont uniformément bornées, inférieurement par un nombre U_m et supérieurement par un nombre U_M. En revanche, cette convergence n'est pas réalisée pour divers cas considérés dans la littérature.

Aussi devons-nous éviter d'introduire l'expression (9) telle quelle dans la définition des programmes optimaux. Pour tourner la difficulté, la seule solution consiste sans doute à limiter la comparaison à une période finie, celle s'écoulant de l'instant ò à un instant horizon T. On considèrera alors l'indicateur d'utilité:

$$\mathcal{U}_T = \sum_{t=0}^{T-1} \gamma^t U_t(c_t, n_t) \tag{12}$$

et on s'imposera de ne comparer un programme donné qu'à certains programmes qui lui sont au moins équivalents pour chacune des périodes à partir de l'instant T.

La méthode la plus simple, et la plus courante, consiste à ne comparer que des programmes absolument identiques à partir de l'instant T. C'est ce que l'on fait en particulier quand on fixe a priori la valeur terminale du stock de bien consommable, soit:

$$S_T = K_T + C_T \,. \tag{13}$$

Mais le caractère peu satisfaisant de cette méthode est bien connu. Elle ne donne aucune garantie que ce stock S_T soit approprié pour le développement économique après l'instant T.

Aussi retiendrons-nous ici un principe plus souple. Nous comparerons un programme donné à tous les programmes qui donnent les mêmes valeurs aux utilités $U_t(c_t, n_t)$ des périodes postérieures à T. D'ailleurs il n'y a aucune raison a priori pour se fixer un horizon T particulier. C'est pourquoi nous adopterons la définition suivante pour les programmes optimaux.

Définition 1. Le programme $\mathcal{P}^1$ est *optimal* s'il est possible, et s'il n'existe aucune valeur T et aucun programme possible $\mathcal{P}$ tels que d'une part:

$$\mathcal{U}_T - \mathcal{U}_T^1 = \sum_{t=0}^{T-1} \gamma^t [U_t(c_t, n_t) - U_t(c_t^1, n_t^1)] > 0 \tag{14}$$

et d'autre part:

$$U_t \ (c_t, \ n_t) \geqq U_t \ (c_t^1, \ n_t^1) \quad \text{pour tout } t \geqq \ T. \tag{15}$$

En somme, un programme possible est dit optimal si on ne peut l'améliorer sur toute période finie sans en réduire l'utilité à un moment au moins après cette période.

Il est clair que, si la somme infinie de l'expression (9) converge pour tous les programmes possibles, et si un programme possible particulier maxime $\mathcal{U}$, alors ce programme est optimal au sens de la définition qui vient d'être donnée. A priori, nous n'avons pas la certitude qu'un programme optimal maxime $\mathcal{U}$ quand la somme infinie converge. Si cette propriété paraît souhaitable, il faudra l'établir dans les cas particuliers considérés.

Pour déterminer les programmes optimaux, nous allons encore poser une hypothèse générale sur les fonctions de production et d'utilité.

Hypothèse 1. Les fonctions f_t (N_t, K_t) *et* U_t (c_t, n_t) sont des fonctions concaves [1]. Elles ont des dérivées premières

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[1] Une fonction $g(x, y)$ est dite concave si quels que soient les nombres x^1, x^2, y^1 et y^2 et quel que soit le nombre α compris entre zéro et un, l'inégalité suivante est satisfaite:

$$g[\alpha \ x^1 + (1-\alpha) \ x^2, \ \alpha \ y^1 + (1-\alpha) \ y^2] \geq \alpha \ g(x^1, \ y^1) + (1-\alpha) \ g(x^2, \ y^2) \ .$$

Si elle existe, la matrice des dérivées secondes d'une fonction concave est négative semi-définie.

que nous désignerons respectivement f'_{tN}, f'_{tK}, U'_{tc} et U'_{tn}. La dérivée U'_{tc} n'est jamais négative.

Supposer l'existence de dérivées dans le modèle macroéconomique considéré ici n'introduit aucune restriction sévère. La concavité des fonctions de production et d'utilité est une hypothèse habituelle. On sait qu'elle exclut les cas dans lesquels les fonctions de production impliqueraient des rendements croissants.

Le modèle qui vient d'être défini est extrêmement simple. Il comporte cependant deux biens: le travail et le bien consommable, biens dont les quantités ne sont pas données a priori mais doivent être déterminées quand le programme optimal est recherché. En un certain sens, ce modèle n'est donc pas purement global. Son étude peut déjà faire apparaître les problèmes que soulèverait la solution de modèles moins agrégés.

Par ailleurs, il peut sembler assez adéquat pour la première phase des études dans la recherche d'un plan de développement. Le problème consiste bien alors à déterminer, dans leurs grandes lignes, les évolutions futures du capital, de la production, de la consommation et de la durée du travail.

Néanmoins, de nombreux modèles considérés dans la littérature sont encore plus simples en ce sens qu'ils considèrent comme une donnée exogène l'évolution de la quantité de travail N_t, ou même qu'ils ne la font pas intervenir du tout. La solution de notre problème s'en trouve grandement facilitée.

Pour le montrer, nous examinerons aussi un modèle général simplifié dans lequel le taux d'activité n_t sera une donnée exogène. La fonction de production pourra être écrite simplement f_t (K_t) et la fonction d'utilité U_t (c_t). Nous remplacerons alors l'hypothèse 1 par la suivante:

Hypothèse 2. Les fonctions f_t (K_t) et U_t (c_t) ont des dérivées premières f'_t et U'_t jamais croissantes. La dérivée U'_t n'est jamais négative.

3. Caractérisation et détermination des croissances optimales

En vue de dégager une méthode pour la détermination des croissances optimales, nous allons rechercher certaines conditions suffisantes pour qu'un programme soit optimal. Nous dégagerons de la sorte des propriétés de certains programmes optimaux, propriétés auxquelles nous pourrons donner une interprétation économique.

Etudions tout d'abord quelles relations existent entre deux programmes possibles. Désignons par N_t, C_t ... etc. ... les valeurs des différentes grandeurs dans le premier programme et par $N_t + \delta N_t$, $C_t + \delta C_t$ ... etc. ... leurs valeurs dans le second programme. Tenons compte de l'hypothèse 1.

En vertu de la concavité [1] des fonctions U_t et f_t, nous pouvons écrire les inégalités suivantes:

$$\delta U_t \leqq U'_{tc}\, \delta c_t + U'_{tn}\, \delta n_t \tag{16}$$

et

$$\delta f_t \leqq f'_{tN}\, \delta N_t + f'_{tK}\, \delta K_t\,. \tag{17}$$

---

(1) Pour toute fonction $g(x, y)$ concave et dérivable, on a

$$\delta g \leqq g'_x\, \delta x + g'_y\, \delta y\,.$$

Dans l'inégalité qui a servi à définir la concavité de $g(x, y)$ prenons en effet $x^1 = x + \delta x$, $y^1 = y + \delta y$, $x^2 = x$, $y^2 = y$. L'inégalité s'écrit alors:

$$g(x + \alpha\, \delta x,\ y + \alpha\, \delta y) \geqq \alpha(g + \delta g) + (1 - \alpha) g$$

ou encore:

$$\delta g \leqq \frac{1}{\alpha} \left[g\,(x + \alpha \delta x,\ y + \alpha \delta y) - g \right]$$

La relation annoncée s'obtient comme limite de la précédente quand le nombre α tend vers zéro.

Reportées dans l'équation (12) définissant $\mathcal{U}_T$ et dans la condition d'équilibre (7), ces inégalités impliquent:

$$\delta\,\mathcal{U}_T \leqq \sum_{t=0}^{T-1} \frac{\gamma^t}{P_t}\left[U'_{tc}\,\delta\,C_t + U'_{tn}\,\delta\,N_t\right] \tag{18}$$

et

$$\delta\,C_{t+1} \leqq f'_{tN}\,\delta\,N_t + (1+f'_{tK})\,\delta\,K_t - \delta\,K_{t+1}\,. \tag{19}$$

De plus, la donnée du stock initial S_0 implique:

$$\delta\,C_0 = -\,\delta\,K_0\,. \tag{20}$$

En reportant les relations (19) et (20) dans l'inégalité (18) et en tenant compte de ce que U'_{tc} ne peut pas être négatif, nous obtenons:

$$\delta\,\mathcal{U}_T \leqq \sum_{t=0}^{T-1} \left\{ \left[\frac{-\gamma^t}{P_t} U'_{tc} + \frac{\gamma^{t+1}}{P_{t+1}} U'_{t+1,c}\,(1+f'_{tK}) \right] \delta\,K_t + \left[\frac{\gamma^t}{P_t} U'_{tn} + \frac{\gamma^{t+1}}{P_{t+1}} U'_{t+1,c}\,.\,f'_{tN} \right] \delta\,N_t \right\} - \frac{\gamma^T}{P_T} U'_{Tc}\,\delta\,S_T \tag{21}$$

formule dans laquelle S_T est égal à $K_T + C_T$ et représente le stock de bien consommable à l'horizon T.

L'inégalité (21) s'applique quelles que soient les valeurs des $\delta\,K_t$, $\delta\,N_t$ et de $\delta\,S_T$. Pour que le programme $\mathcal{P}^o$ soit optimal, il suffit que le second membre de (21) ne puisse pas être positif; c'est-à-dire que, pour tout T:

1) les coefficients des δK_t et des δN_t dans (21) soient nuls;

2) et qu'il n'existe aucun programme possible donnant, à partir de T, des valeurs aux utilités U_t au moins égales à celles que donne $\mathcal{P}$, mais comportant un stock S_T plus faible.

Notons ici que la condition (1) seule suffirait si nous avions fixé le stock terminal S_T. La condition (2) est propre à l'étude des programmes à l'horizon illimité. Négligeons la pour le moment; et étudions ce qu'implique la nullité des coefficients des δK_t et des δN_t.

La condition (1) s'écrit:

$$(22) \qquad \begin{cases} U'_{t+1,c}(1 + f'_{tK}) = (1 + \varepsilon)\dfrac{P_{t+1}}{P_t} U'_{tc} \\ U'_{t+1,c} \cdot f'_{tN} = -(1 + \varepsilon)\dfrac{P_{t+1}}{P_t} U'_{tn} \end{cases}$$

égalités dans lesquelles intervient le taux d'intérêt normatif ε défini par la formule (10).

La condition d'équilibre (7) et les deux égalités (22) peuvent être considérées comme constituant un système d'équations de récurrence sur les grandeurs C_t, N_t et K_t. Plus précisément, les trois égalités en question peuvent généralement être résolues pour donner N_t, C_{t+1} et K_{t+1} en fonction de C_t et de K_t. Pour chaque valeur donnée C_0, et pour la valeur correspondante $S_0 - C_0$ de K_0, il existe donc en général des suites de valeurs C_t, N_t, K_t satisfaisant les équations de récurrence.

Nous pouvons conjecturer qu'un programme optimal est défini par celui qui, de tous les programmes possibles satisfaisant les équations de récurrence, comporte la plus grande valeur de la consommation initiale C_0. Nous allons en effet obtenir une propriété assez analogue. Pour en préciser le sens, posons d'abord la définition suivante:

Définition 2. Un programme est dit « *régulier* » s'il est possible et s'il satisfait les équations de récurrence définies par la condition d'équilibre (7) et les égalités (22). Un programme est dit « *régulier maximal* » s'il est régulier et si aucun programme régulier ne donne une valeur plus forte à C_0.

Posons également les deux conditions suivantes:

Condition 1. Le programme $\mathscr{P}$ satisfait la condition 1 si $U'_{tc}>0$ pour tout t et s'il existe un nombre h plus grand que 1 tel que, au moins à partir d'une certaine valeur de t:

$$(1 + f'_{tK}) \frac{S_t}{S_{t+1}} \geqq h > 1 \tag{23}$$

Condition 2. Etant donné deux programmes réguliers quelconques $\mathscr{P}^1$ et $\mathscr{P}^2$, l'inégalité $C_0^1 \geqq C_0^2$ implique $S_t^1 \leqq S_t^2$ pour tout t.

Etablissons le résultat suivant:

Proposition 1. Un programme régulier $\mathscr{P}$ qui satisfait la condition 1 est optimal.

Supposons en effet qu'il n'en soit pas ainsi. Il doit exister une valeur T et un programme possible $\mathscr{P} + \delta\,\mathscr{P}$ tel que $\delta\,\mathcal{U}_T$ soit positif et que $\delta\,U_t$ ne soit négatif pour aucun $t \geqq T$. En vertu des égalités (22), de l'inégalité (21) et du fait que U'_{Tc} ne peut être négatif, $\delta\,\mathcal{U}_T$ ne peut être positif que si $\delta\,S_T$ est négatif.

Par ailleurs, l'inégalité (16) implique:

$$U'_{tc}\,\delta\,C_t + U'_{tn}\,\delta\,N_t \geqq 0 \text{ pour tout } t \geqq T \tag{24}$$

et la condition d'équilibre (19) peut être écrite:

$$(25) \qquad \delta S_{t+1} \leqq f'_{tN} \, \delta N_t + (1 + f'_{tK}) \, \delta K_t \, .$$

Comme les U'_{tc} sont positifs, les équations (22) impliquent:

$$(26) \qquad f'_{tN} = -(1 + f'_{tK}) \frac{U'_{tn}}{U'_{tc}}$$

La première équation du système (22) implique aussi que $1 + f'_{tK}$ soit positif, de sorte que l'inégalité (24) peut aussi s'écrire:

$$f'_{tN} \, \delta N_t \leqq (1 + f_{tK}) \, \delta C_t \qquad \text{pour tout } t \geqq T \, .$$

En combinant cette inégalité avec l'inégalité (25), nous obtenons:

$$(27) \qquad \delta S_{t+1} \leqq (1 + f'_{tK}) \, \delta S_t \qquad \text{pour tout } t \geqq T \, .$$

Comme δS_T est négatif, il en résulte que tous les δS_t sont négatif pour $t \geqq T$. Soit θ la valeur de t à partir de laquelle l'inégalité (23) s'applique, si cette valeur excède T, ou $\theta = T$ dans le cas contraire. Les inégalités (23) et (27) impliquent:

$$\frac{\delta S_{t+1}}{S_{t+1}} \leqq h \frac{\delta S_t}{S_t} \leqq h^{t-\theta+1} \frac{\delta S_0}{S_0} \qquad \text{pout tout } t \geqq \theta.$$

Comme h est plus grand que 1, $\frac{\delta S_t}{S_t}$ tend vers $-\infty$ quand t croît indéfiniment ce qui contredit le fait que $S_t + \delta S_t$ ne soit jamais négatif. (Les conditions (3) et (8) impliquent bien $S_t \geqq 0$ pour tout programme possible). Cette contradiction complète la démonstration de la proposition 1.

Pour déterminer un programme optimal, il suffirait en principe de trouver un programme régulier qui satisfasse la condition 1. Mais cette recherche serait assez laborieuse si on n'avait aucun fil directeur. En pratique, il peut être plus simple de rechercher d'abord un programme régulier maximal, et de vérifier ensuite qu'il satisfait la condition 1. La proposition suivante suggère que cette manière de faire sera généralement efficace quand la condition 2 est satisfaite.

Proposition 2. Si la condition 2 est satisfaite, un programme régulier maximal est optimal dans l'ensemble de tous les programmes réguliers [1].

La démonstration est immédiate. Supposons en effet que $\mathscr{P}$ soit un programme régulier maximal et $\mathscr{P} + \delta \mathscr{P}$ un programme régulier qui lui soit préférable. Par hypothèse $\delta C_0 \leqq 0$. De plus on établit, comme au début de la démonstration de la proposition 1, que $\delta S_T < 0$. Mais ceci est contradictoire avec la condition 2.

La condition 2 peut sembler un peu difficile à vérifier. Il est donc intéressant de connaître des hypothèses sous lesquelles elle est bien satisfaite. Posons:

Hypothèse 3. La production s'effectue à rendements constants, c'est-à-dire qu'il existe des fonctions $\varphi_t(x)$ possédant des dérivées φ'_t décroissantes, et telles que:

$$f_t(N_t, K_t) = N_t \varphi_t\left(\frac{K_t}{N_t}\right) \tag{28}$$

[1] La définition 1 introduit, entre les programmes possibles, un ordre partiel. Il se pourrait donc a priori qu'un programme régulier non maximal soit optimal. Mais cela semble peu vraisemblable.

Les utilités de la consommation et du travail sont additives, c'est-à-dire qu'il existe des fonctions $V_t\ (c_t)$ et $W_t\ (n_t)$ telles que:

$$U_t\ (c_t,\ n_t) = V_t\ (c_t) - W_t\ (n_t)\ . \tag{29}$$

Ces fonctions sont dérivables; V'_t est positive et décroissante; W'_t est non négative et non décroissante.

Nous pouvons démontrer le résultat suivant:

Proposition 3. Si l'hypothèse 3 est satisfaite, la condition 2 l'est aussi.

Considérons en effet deux programmes réguliers $\mathscr{P}$ et $\mathscr{P} + \delta \mathscr{P}$, et démontrons que $\delta\ C_t \geqq 0$ et $\delta\ K_t \leqq 0$ impliquent $\delta\ N_t \leqq 0$; $\delta\ C_{t+1} \geqq 0$; $\delta\ K_{t+1} \leqq 0$ et $\delta\ S_{t+1} \leqq 0$. La condition 2 sera bien alors établie puisque $\delta\ C_0 \geqq 0$ implique $\delta\ K_0 \leqq 0$ en vertu de l'égalité (5).

Nous avons ici:

$$f'_{tN} = \varphi_t - \frac{K_t}{N_t}\ \varphi'_t \qquad\qquad f'_{tK} = \varphi'_t$$

Les égalités (22) peuvent alors être écrites:

$$1 + \varphi'_t = (1 + \varepsilon)\frac{P_{t+1}}{P_t} \cdot \frac{V'_t}{V'_{t+1}} \tag{30}$$

$$\frac{f'_{tN}}{1 + f'_{tK}} = \frac{\varphi_t - \dfrac{K_t}{N_t}\varphi'_t}{1 + \varphi'_t} = \frac{W'_t}{V'_t} \tag{31}$$

1) Pour obtenir $\delta N_t \leqq 0$ à partir de $\delta C_t \geqq 0$ et $\delta K_t \leqq 0$, supposons $\delta N_t > 0$. Alors $\delta \left(\frac{K_t}{N_t}\right) < 0$. Dans l'égalité (31), le membre de gauche aura avec $\wp + \delta \wp$ une valeur plus faible [1] qu'avec $\wp$. Mais dans le membre de droite de (31), $\delta W'_t \geqq 0$ et $\delta V'_t \leqq 0$. Cette contradiction implique bien $\delta N_t \leqq 0$.

2) Comme $\delta N_t \leqq 0$ et $\delta K_t \leqq 0$, alors $\delta S_{t+1} \leqq 0$ en vertu de l'inégalité (25). (Les égalités (30) et (31) impliquent $1 + f'_{tK} > 0$ et $f'_{tN} \geqq 0$).

3) Pour établir $\delta C_{t+1} \geqq 0$ et $\delta K_{t+1} \leqq 0$, il suffit de montrer que δC_{t+1} ne peut pas être négatif, car $\delta K_{t+1} \leqq 0$ résulte alors directement de $\delta S_{t+1} \leqq 0$. Supposons donc $\delta C_{t+1} < 0$ et considérons deux cas:

a) *Si* $\delta \left[\frac{K_t}{N_t}\right] \leqq 0$, alors le membre de gauche de l'égalité (30) ne peut avoir avec $\wp + \delta \wp$ une valeur plus faible qu'avec $\wp$. Mais dans le membre de droite $\delta V'_t \leqq 0$ et $\delta V'_{t+1} > 0$. Nous obtenons bien une contradiction.

b) Si $\delta \left[\frac{K_t}{N_t}\right] > 0$, considérons l'égalité:

$$(32) \qquad f'_{tN} = (1 + \varepsilon) \frac{P_{t+1}}{P_t} \cdot \frac{W'_t}{V'_{t+1}}$$

---

(1) La productivité marginale du travail f'_{tN} est en effet une fonction croissante de $\frac{K_t}{N_t}$.

Si nous désignons par x le rapport $\frac{K_t}{N_t}$, et si $\delta x < 0$, nous pouvons vérifier les inégalités suivantes:

$$\delta \varphi - \varphi' \, \delta x \leqq 0 \qquad \text{en vertu de la concavité de la fonction } \varphi.$$

Donc: $\quad \varphi - x\varphi' \geqq \varphi + \delta \varphi - \varphi'(x + \delta x)$.

Mais aussi:

$\varphi + \delta \varphi - \varphi' \, (x + \delta x) > \varphi + \delta \varphi - (x + \delta x) \, (\varphi' + \delta \varphi')$

puisque: $\delta \varphi' > 0$ et $x + \delta x > 0$.

Elle est contradictoire avec les inégalités $\delta f'_{tN} > 0$, $\delta V'_{t+1} > 0$ et $\delta W'_t \leqq 0$.

Ainsi, nous avons démontré la proposition 3.

Terminons cette étude générale par quelques remarques.

1. Nous avons prêté attention à des conditions suffisantes pour qu'un programme soit optimal. Nous pourrions chercher à généraliser quelque peu ces conditions. Ainsi, supposons que dans le programme possible $\mathcal{P}$ considéré N_t soit égal à $n_M P_t$ pour une certaine période. On ne peut alors envisager que des variations δN_t négatives. Dans les conditions suffisantes pour que $\mathcal{P}$ soit optimal, l'inégalité

$$U'_{t+1,c} \cdot f'_{tN} \geqq -(1+\varepsilon)\frac{P_{t+1}}{P_t} U'_{tn}$$

doit remplacer l'une des égalités (22) pour cette valeur de t. D'une manière générale, on peut toujours se reporter à l'inégalité (21) pour appliquer le type d'analyse présenté ci-dessus à des cas qui ne respectent pas exactement les conditions que nous avons retenues.

De même, si les dérivées des fonctions U_t et f_t sont continues, la réalisation des conditions marginales (22) est nécessaire pour l'optimalité de tout programme possible $\mathcal{P}$ qui soit tel que l'on puisse encore définir un programme possible en modifiant isolément chaque N_t et chaque K_t, et ceci à la fois dans le sense de la baisse ou dans celui de la hausse. En effet, la différence entre les deux membres de l'inégalité (21) est un infiniment petit du second ordre par rapport aux δN_t et aux δK_t.

2. Nous pouvons donner une interprétation économique des conditions marginales introduites ci-dessus.

Supposons $U'_{t+1,c}$ positif et considérons le taux π_t de croissance de la population et le taux u_t de décroissance de l'utilité marginale de la consommation:

$$(33) \qquad \pi_t = \frac{P_{t+1} - P_t}{P_t} \qquad u_t = \frac{U'_{tc} - U'_{t+1,c}}{U'_{t+1,c}}$$

La première des égalités (22) s'écrit:

$$(34) \qquad 1 + f'_{tK} = (1+\varepsilon)\ (1+\pi_t)\ (1+u_t)\ .$$

La productivité marginale du capital, pour la période t, c'est-à-dire le taux d'intérêt durant cette période, doit satisfaire une relation particulièrement simple contenant trois termes dépendant, le premier du taux d'intérêt normatif, le second du taux de croissance de la population, le troisième du taux de décroissance de l'utilité marginale ([1]).

Les deux relations (22) impliquent aussi:

$$(35) \qquad \frac{f'_{tN}}{1 + f'_{tK}} = - \frac{U'_{tn}}{U'_{tc}}$$

c'est-à-dire l'égalité de deux taux marginaux de substitution entre le travail et le bien consommable, substitutions envisagées

([1]) Des relations analogues ont été obtenues récemment par divers auteurs. Voir par exemple R. FRISCH (1962).

du point de vue de la production d'une part, du point de vue de la consommation d'autre part.

Enfin l'inégalité (23) de la condition 1 implique que la valeur actualisée à l'instant 0 du stock de bien consommable disponible à l'instant t tende vers zéro quant t augmente indéfiniment. Soit en effet s_t cette valeur actualisée. Par définition:

$$\frac{s_{t+1}}{s_t} = \frac{1}{1+f'_{tK}} \cdot \frac{S_{t+1}}{S_t}$$

La condition 1 énonce que, au moins à partir d'une certaine valeur θ de t, le membre de droite soit inférieur ou au plus égal à un nombre $\frac{1}{h}$ plus petit que 1. Il en résulte que

$$0 \leqq s_t \leqq \frac{1}{h^{t-\theta}} s_\theta \tag{36}$$

Par suite s_t tend bien vers zéro avec t.

3. Nous avons considéré ici le modèle dans lequel les quantités de travail sont déterminées de manière endogène. Nous pouvons aisément adapter les résultats au cas dans lequel ces quantités de travail seraient données de manière exogène.

Dans l'inégalité fondamentale (21), il n'y a plus lieu de faire intervenir le terme en δN_t. La seconde des égalités (22) disparaît donc. Les équations de récurrence qui définissent les programmes réguliers comprennent l'égalité (7) et la première des égalités (22). Elles peuvent généralement être résolues pour donner C_{t+1} et K_{t+1} en fonction de C_t et de K_t. Avec ces modifications, les propositions 1 et 2 restent valables.

L'hypothèse 3 peut être notablement réduite. Il suffit de supposer que les fonctions f_t (K_t) et U_t (c_t) ont des dérivées décroissantes et que U'_t est positive. La proportion 3 reste alors valable elle aussi.

4. Un modèle de R. Radner

La méthode proposée ci-dessus est en général trop laborieuse pour être appliquée telle quelle sur des modèles abstraits; car on ne peut pas obtenir explicitement la solution des équations de récurrence. Toutefois, R. Radner (1962) a proposé pour les fonctions de production et d'utilité des formes analytiques qui facilitent beaucoup les calculs. Nous allons examiner brièvement la version la plus agrégée de son modèle. Ceci nous permettra de vérifier comment s'applique la méthode en question.

Radner considère le cas dans lequel la quantité de travail serait donnée de manière exogène. La population croîtrait à un rythme constant, de même que la quantité de travail. La même fonction d'utilité s'appliquerait à toutes les périodes; elle serait linéaire par rapport au logarithme de la consommation. Le minimum vital c_m serait pris égal à zéro. Enfin, *l'output total* $K_t + Q_t$ obéirait à une fonction de Cobb-Douglas à progrès technique neutre et de rythme constant. Ces hypothèses s'expriment par les égalités suivantes:

$$P_t = (1+\pi)^t \ P_0 \qquad N_t = (1+\nu)^t \ N_0$$

$$U_t \ (c_t) = a \log c_t + b \tag{37}$$

$$f_t \ (N_t, \ K_t) + K_t = q_0 \ N^\alpha_t \ K^\beta_t \ (1+\theta)^t \tag{38}$$

π, ν, a, b, q_0, α, β et θ étant des nombres fixes.

[5] *Malinvaud* - pag. 30

(Le modèle n'est réaliste que si $\nu \leqq \pi$, sans quoi la quantité de travail par personne croîtrait indéfiniment).

Si β est plus petit que 1, ce que nous supposerons, ce modèle satisfait l'hypothèse 2, et même l'hypothèse 3 modifiée comme il a été dit à la fin de la section précédente.

Les expressions analytiques retenues sont évidemment restrictives. Avec la fonction (37), l'utilité croît indéfiniment avec la consommation. Toutefois, cette forme peut sans doute convenir pour des collectivités qui auraient un niveau de vie bien supérieur au minimum vital et encore éloigné d'un niveau de satiété.

La fonction de production (38) a une forme inhabituelle. Elle implique pratiquement que la production nette ait à tout moment un maximum qui serait atteint pour une valeur du capital assez peu supérieure à celle observée effectivement. Pour fixer les idées on pourrait retenir par exemple les valeurs suivantes des constantes $\alpha = \frac{3}{10}$ et $\beta = \frac{13}{16}$. La productivité marginale du capital serait égale alors à $\frac{1}{12}$ pour un coefficient de capital $\frac{K_0}{Q_0}$ égal à 3. Le maximum de la production nette serait atteint avec un coefficient de capital $\frac{K_0}{Q_0}$ égal à $\frac{13}{3}$ $\simeq 4{,}3$. Ces ordres de grandeur ne semblent pas invraisemblables a priori.

L'expression (38) peut encore être écrite:

$$f_t\ (N_t,\ K_t) + K_t = h_0\ \mu^t\ K_t^\beta$$

avec les constantes h_0 et μ définies par:

$$h_0 = q_0\ N_0^\alpha \qquad \mu = (1+\nu)^\alpha\ (1+\theta)$$

On en déduit:

$$(39) \qquad 1 + f'_{tk} = \beta h_0 \; \mu^t \; K_t^{\beta - 1} \, .$$

Par ailleurs, la forme retenue pour la fonction U implique:

$$\frac{U'_t}{U'_{t+1}} = \frac{P_t}{P_{t+1}} \cdot \frac{C_{t+1}}{C_t}$$

Ainsi, les conditions marginales (22) deviennent:

$$(40) \qquad 1 + f'_{tK} = (1 + \varepsilon) \frac{C_{t+1}}{C_t}$$

ou encore:

$$(41) \qquad \frac{C_{t+1}}{C_t} = \gamma \beta h_0 \, \mu^t \, K_t^{\beta - 1}$$

De même, la condition d'équilibre (7) devient:

$$(42) \qquad C_{t+1} + K_{t+1} = h_0 \; \mu^t \; K_t^{\beta} \, .$$

La récurrence définie par les égalités (41) et (42) prend une forme particulièrement simple, du fait que ces deux égalités impliquent:

$$\frac{C_{t+1}}{C_t} = \gamma\beta \frac{C_{t+1} + K_{t+1}}{K_t}$$

ou encore:

$$\frac{K_t}{C_t} = \gamma\beta \frac{K_{t+1}}{C_{t+1}} + \gamma\beta$$

Posons

$$\eta = \frac{1}{\gamma\beta}$$

et écrivons

$$\frac{K_{t+1}}{C_{t+1}} = \eta \frac{K_t}{C_t} - 1$$

dont la solution est immédiate:

$$\frac{K_t}{C_t} = \eta^t \frac{K_o}{C_o} - \frac{1 - \eta^t}{1 - \eta} \tag{43}$$

Les programmes réguliers sont les programmes possibles qui satisfont les égalités (43). Pour qu'une valeur de C_0 corresponde à un programme régulier, il faut donc que le membre de droite de ces égalités ne soit négatif pour aucune valeur de t, c'est-à-dire que:

$$\frac{K_0}{C_0} \geqq \frac{1 - \eta^{-t}}{\eta - 1} \qquad \text{pour tout } t\,.$$

Or η est supérieur à 1, du moins si β est plus petit que $1 + \varepsilon$, ce que nous supposerons. La solution des équations de récurrence (41) et (42) définira donc un programme régulier si:

$$\frac{K_0}{C_0} \geqq \frac{1}{\eta - 1}$$

La valeur C_0 qui correspond au programme régulier maximal est alors définie par l'égalité

$$\frac{S_0 - C_0}{C_0} = \frac{1}{\eta - 1}$$

En reportant cette valeur dans l'égalité (43), nous trouvons que, dans le programme régulier maximal le rapport K_t/C_t est constant:

$$\frac{K_t}{C_t} = \frac{1}{\eta - 1} \tag{44}$$

Il en résulte que S_t/C_t est aussi constant. En vertu de l'égalité (40), nous pouvons écrire:

$$(1 + f'_{tK}) \frac{S_t}{S_{t+1}} = 1 + \varepsilon$$

La condition 1 est donc satisfaite, et le programme régulier maximal est optimal, si le taux d'intérêt normatif ε est positif (1).

Pour déterminer complètement le programme régulier maximal, reportons (44) dans (41). Nous obtenons une équation de récurrence sur K_t seul, soit:

$$K_{t+1} = \gamma\, \beta\, h_0\, \mu^t\, K_t^\beta\,. \tag{45}$$

Définissons k_t et $\bar{k}$ par les égalités:

$$K_t = \mu^{\frac{1}{1-\beta}t}\, k_t \tag{46}$$

1) Dans un programme régulier non maximal, on peut poser $\frac{K_0}{C_0} = \frac{1+\lambda}{\eta - 1}$ avec un nombre λ positif. L'égalité (43) s'écrit alors:

$$\frac{K_t}{C_t} = \frac{1 + \lambda\eta^t}{\eta - 1}$$

et l'égalité (40) implique:

$$\frac{S_t}{S_{t+1}}(1 + f'_{tK}) = \beta \frac{\lambda + \eta^{1-t}}{\lambda + \eta^{-t}}$$

Comme η est plus grand que 1, cette quantité tend vers β qui est plus petit que 1 par hypothèse. La condition 1 ne peut donc être satisfaite par aucun programme régulier non maximal.

et

$$(47) \qquad \bar{k} = (\gamma \beta h_o)^{\frac{1}{1-\beta}} \mu^{\frac{-1}{(1-\beta)^2}}$$

Nous vérifions tout de suite que l'équation (43) implique:

$$(48) \qquad k_{t+1} = [\bar{k}]^{1-\beta} k_t^{\beta}$$

la grandeur k_t obéit donc à une loi de récurrence très simple. Sa valeur à l'époque t est une moyenne géométrique de sa valeur à l'époque $t-1$ et d'une valeur fixe $\bar{k}$. Elle tend progressivement vers $\bar{k}$. La rapidité de cette tendance dépend du coefficient β puisque l'on déduit de (48):

$$(49) \qquad \log \frac{k_t}{\bar{k}} = \beta^t \log \frac{k_o}{\bar{k}}$$

A titre d'exemple [1], retenons $\alpha = \frac{3}{16}$, $\beta = \frac{13}{16}$, $\nu = 1\%$ et $\theta = 0{,}5\%$. Le coefficient $\mu^{\frac{1}{1-\beta}}$ est alors approximativement égal à 1,037. La croissance limite du capital et de la consommation s'effectue au rythme de 3,7% par période.

Si le taux d'intérêt normatif est nul et si $\frac{K_o}{Q_o}$ est égal à 3,

[1] Comme le montre la formule (38), le coefficient de progrès technique s'applique à l'output total et non à la production nette. Il convient donc de retenir pour lui une valeur plus faible que celles adoptées habituellement pour caractériser le développement de la production nette.

le rapport $\frac{\bar{k}}{k_0}$ est approximativement égal à 1,26. Il aurait suffi que le capital initial fut supérieur de 26% pour que la croissance s'effectue de façon strictement proportionnelle au taux de 3,7% par période. Même avec un coefficient de capital initial de 3, l'écart entre k_t et $\bar{k}$ n'est plus que de 3% après 10 périodes, comme le montre l'application de la formule (49). Enfin, le taux d'épargne nette limite est de 13,4%.

Si le taux d'intérêt normatif est de 10% et si $\frac{K_0}{Q_0}$ est égal à 3, le rapport $\frac{\bar{k}}{k_0}$ est approximativement égal à 0,76. Le capital initial excède celui qui conduirait à une croissance optimale strictement proportionnelle. Le taux d'épargne nette limite n'est plus que 9,2%.

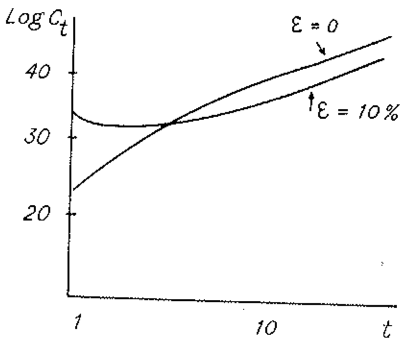

FIG. 1

La figure 1 représente l'évolution de la consommation dans les deux programmes réguliers maximaux correspondant aux valeur 0 et 10% du taux d'intérêt normatif. La seconde permet une consommation supérieure de 40% dans la première période; mais elle a pour effet une consommation inférieure de 8% dans le régime asymptotique.

Retournant aux formules générales, nous observons qu'à la limite le capital et la consommation globale croissent à un taux constant égal à

$$(1+\nu)^{\frac{\alpha}{1-\beta}}(1+\theta)^{\frac{1}{1-\beta}}-1 .$$

La consommation par unité de travail tend de même à augmenter au taux

$$(1+\nu)^{\frac{\alpha+\beta-1}{1-\beta}}(1+\theta)^{\frac{1}{1-\beta}}-1 .$$

Si la fonction de production satisfait l'hypothèse des rendements constants, ce dernier taux est simplement de $(1+\theta)^{\frac{1}{1-\beta}}-1$; il diffère peu en pratique de $\frac{\theta}{1-\beta}$ ·

Soit ρ_t la productivité marginale du capital, on le taux d'intérêt, de la période t.

La formule (40) montre que $1+\rho_t$ est égal au produit de $1+\varepsilon$ par le facteur de croissance de la consommation globale. A la limite $1+\rho_t$ tend vers

$$1+\bar{\rho}=(1+\varepsilon)\,\mu^{\frac{1}{1-\beta}}$$

Enfin, nous pouvons vérifier que la croissance asymptotique pour laquelle la consommation se situe au niveau le plus élevé est justement celle dans laquelle le taux d'intérêt normatif est nul. (Le taux d'intérêt ρ_t est alors égal au taux de croissance de la consommation globale). En effet, comme μ ne dépend pas de γ et que k_t tend vers $\overline{k}$, le niveau de la croissance asymptotique de la consommation varie comme

$$(\eta - 1)(\gamma \beta h_0)^{\frac{1}{1-\beta}}$$

(voir formules (44), (46) et (47) ci-dessus). Il varie donc aussi comme

$$(1 - \gamma\beta)\, \gamma^{\frac{\beta}{1-\beta}}$$

La dérivée par rapport à γ du logarithme de cette expression est égale à:

$$\frac{-\beta}{1 - \gamma\beta} + \frac{\beta}{(1 - \beta)\gamma}$$

Elle s'annule justement quand $\gamma = 1$.

L'étude qui précède a permis de caractériser les programmes réguliers maximaux. Dans quels cas ceux-ci constituent-ils des programmes optimaux? Nous avons déjà observé que la condition 1 était satisfaite quand le taux d'intérêt normatif était positif. Nous avons donc déterminé un programme optimal pour toute situation dans laquelle il en est ainsi. Afin de mieux comprendre les complications qui apparaissent quand le taux d'intérêt normatif est négatif ou nul, essayons d'examiner directement l'optimalité des programmes réguliers maximaux dans le cadre du modèle de RADNER.

Nous devons donc comparer le programme régulier maximal, que nous désignerons par $\mathcal{P}^*$, avec un programme possible qui, à partir d'un certain horizon T, donne aux U_t, c'est-à-dire aux consommations C_t, des valeurs au moins égales à celles que leur donne $\mathcal{P}^*$. Nous avons déjà vu que, pour que cet autre programme donne une valeur supérieure à $\mathcal{U}_T$, il fallait qu'il donne une valeur plus faible à S_T. Le problème consiste donc à tronver la réponse à la question suivante: Est-il possible d'obtenir, au moins à partir d'une certaine époque T, la suite des consommations C^*_T, C^*_{T+1} ... ad infinitum, à partir d'un stock de bien S_T plus faible que S^*_T? Si la réponse est négative, nous savons que $\mathcal{P}^*$ est optimal. Si elle est affirmative, nous savons que $\mathcal{P}^*$ n'est pas optimal, car on améliorerait ce programme en augmentant la consommation de $S^*_T - S_T$ pendant la période T, sans la modifier dans aucune autre période.

Pour répondre à cette question, considérons la relation qui existe entre les variations δK_t et δK_{t+1} du capital lorsque l'on passe du programme $\mathcal{P}^*$ à un autre programme possible $\mathcal{P}^* + \delta \mathcal{P}$ qui assure la même consommation C^*_{t+1} dans la période $t+1$. Plaçons-nous dans le cas où δK_t est négatif.

Partons de l'égalité suivante qui exprime la condition d'équilibre à l'instant $t+1$:

$$S_{t+1} = h_0 \mu^t K_t^\beta \quad . \tag{50}$$

Comme la dérivée seconde de K_t^β est croissante et que δK_t est négatif, nous pouvons écrire:

$$\delta K_{t+1} = \delta S_{t+1} \leqq \beta h_o \mu^t K_t^{\beta-1} \delta K_t + \frac{\beta(\beta-1)}{2} h_o \mu^t K_t^{\beta-2} (\delta K_t)^2 \tag{51}$$

Nous savons aussi que la différence entre les deux membres de l'inégalité peut être rendue aussi petite que l'on veut si δK_t est pris suffisamment petit.

Par ailleurs, les équations (50) et (44) impliquent que, dans le programme $\mathscr{P}^*$:

$$(1+\varepsilon)\, K_{t+1} = \beta\, h_0\, \mu^t\, K_t^\beta$$

Nous pouvons donc réécrire (51) sous la forme:

$$\frac{\delta K_{t+1}}{K^*_{t+1}} \leqq (1+\varepsilon)\,\frac{\delta K_t}{K^*_t} - \frac{(1+\varepsilon)(1-\beta)}{2}\cdot\left(\frac{\delta K_t}{K^*_t}\right)^2$$

Cette inégalité implique que δK_{t+1} soit négatif, car β est plus petit que 1. Elle implique aussi:

$$(52) \qquad \left|\frac{\delta K_{t+1}}{K^*_{+1}}\right| \geqq (1+\varepsilon)\left|\frac{\delta K_t}{K^*_t}\right| + \frac{(1+\varepsilon)(1-\beta)}{2}\left|\frac{\delta K_t}{K^*_t}\right|^2$$

Considérons maintenant la suite des $\left|\frac{\delta K_t}{K^*_t}\right|$ à partir de l'instant T. Si cette suite peut être choisie de manière à ne jamais dépasser la valeur 1, la réponse à la question posée est affirmative, et $\mathscr{P}^*$ n'est pas optimal. Si non, la réponse est négative, et $\mathscr{P}^*$ est bien optimal.

1) Si le taux d'intérêt normatif n'est pas négatif ($\varepsilon \geqq 0$), alors quel que soit $\delta K_T < 0$, la suite des $\left|\frac{\delta K_t}{K^*_t}\right|$ augmente au delà

de toute limite en vertu de l'inégalité (52). Cette suite finit donc toujours par dépasser la valeur 1. Le programme régulier maximal étudié ici est optimal.

2) Si le taux d'intérêt normatif est négatif ($\varepsilon < 0$), alors on peut prendre δK_T suffisamment petit pour que

$$\left| \frac{\delta K_{T+1}}{K^*_{T+1}} \right| < \left| \frac{\delta K_T}{K^*_T} \right|$$

et, a fortiori, que

$$\left| \frac{\delta K_{T+2}}{K^*_{T+2}} \right| < \left| \frac{\delta K_{T+1}}{K^*_{T+1}} \right|$$

et ainsi de suite. Le programme $\mathscr{P}^*$ n'est donc pas optimal.

Que se passe-t-il quand le taux d'intérêt normatif est négatif? Nous pourrions alors vérifier sans trop de peine qu'aucun programme possible n'est optimal.

Les raisons de ce fait peuvent paraître plus claires si nous considérons le programme possible $\mathscr{P}^* + \delta \mathscr{P}$ qui est apparu préférable au programme $\mathscr{P}^*$ dans la démonstration précédente. Ce programme permet les consommations C_t^* pendant toutes les périodes sauf la T-ème, et permet $C_T^* + |\delta K_T|$ pendant celle-ci. Mais, supposons qu'au lieu de consommer $|\delta K_T|$ dans la période T, on envisage de l'utiliser dans la production et de consommer dans la période $T+1$ le produit supplémentaire qu'il permettra d'obtenir. L'inégalité (52), jointe au fait qu'il existe une proportion constante entre C_t^* et K_t^*, montrent que:

$$\frac{|\delta K_{T+1}|}{C^*_{T+1}} > (1 + \varepsilon) \frac{|\delta K_T|}{C^*_T}$$

Ceci implique que l'opération envisagée est avantageuse du point de vue de l'utilité $\mathcal{U}_{T+1}$.

Ainsi, bien que à chaque moment le stock de capital existant dans le programme régulier maximal soit surabondant, à aucun moment on a intérêt à en consommer une partie, car il est toujours préférable de reporter à plus tard cette consommation.

En somme, l'étude du modèle de Radner comporte deux conclusions qui pourraient avoir une portée générale.

1. Si on n'y prend pas garde, la formulation du modèle peut conduire à des situations dans lesquelles aucun programme optimal n'existe. Il semble que cette particularité se présente surtout quand la fonction d'utilité attribue une grande importance au futur, c'est-à-dire quand le taux d'intérêt normatif est faible (négatif dans le cas présent).

2. En revanche, on peut parfois définir des programmes optimaux dans des cas dans lesquels la fonction d'utilité $\mathcal{U}_T$ ne converge vers aucune valeur finie quand T croît indéfiniment. Nous l'avons vérifié ici puisque nous avons trouvé un programme optimal pour le modèle comportant un taux d'intérêt normatif ε nul.

5. Croissances optimales à coefficient de capital fixe

Les équations de récurrence qui interviennent dans la définition des programmes réguliers prennent une forme plus simple dans deux cas que nous allons considérer successivement: celui dans lequel la fonction de production f_t est linéaire, et celui dans lequel l'utilité U_t est linéaire.

Si la fonction de production s'écrit:

$$f_t\ (N_t,\ K_t) = a_t\ N_t + b_t\ K_t \tag{53}$$

avec des coefficients numériques fixes a_t et b_t, les équations de récurrence (22) peuvent être écrites sous la forme:

$$\left\{ \begin{aligned} & U'_{t+1,c}(1 + b_t) = (1 + \varepsilon)\frac{P_{t+1}}{P_t} U'_{tc} \\ & U'_{tn} = -\frac{a_t}{1 + b_t} U'_{tc} \end{aligned} \right. \tag{54}$$

Elles ne font plus alors intervenir la grandeur K_t. On peut donc en général les résoudre par rapport aux C_t et N_t à partir d'une valeur donnée de C_0. En principe, la deuxième équation de (54) détermine N_t en fonction de C_t. Si une solution explicite de cette équation peut être obtenue et reportée dans la première équation, alors celle-ci définit une récurrence sur la seule grandeur C_t. La détermination des programmes réguliers est donc grandement facilitée.

Malheuresement, une fonction de production du type (53) ne peut offrir qu'une représentation très médiocre des contraintes techniques. Elle suppose la constance des productivités marginales, constance contraire aux résultats de nombreuses études économétriques.

Toutefois, on admet assez souvent que la main-d'oeuvre est surabondante dans les pays peu développés, et que la production y est proportionnelle à la quantité de capital disponible. Nous allons nous placer ici dans cette perspective et retenir pour la fonction de production l'expression (53) avec une valeur nulle de a_t. Nous allons donc rechercher des croissances optimales pour une économie peu développée dans laquelle le coefficient de capital peut être considéré comme fixe.

La deuxième équation du système (54) montre qu'alors l'utilité marginale du travail doit être nulle dans tout programme régulier. On ne perd dès lors guère en réalisme à éliminer complètement du modèle la quantité de travail et à retenir une fonction d'utilité U_t qui ne dépende que de c_t.

Pour déterminer les programmes réguliers, il suffit de considérer la condition d'équilibre

$$C_{t+1} = (1 + b_t) K_t - K_{t+1} \tag{55}$$

et l'équation de récurrence:

$$(1 + b_t) U'_{t+1} = (1 + \varepsilon) \frac{P_{t+1}}{P_t} U'_t \tag{56}$$

La résolution de (56) donne la suite des C_t en fonction de C_0. La suite de K_t est ensuite facile à obtenir grâce à l'équation (55). Pour que la solution ainsi trouvée convienne, il faut encore vérifier qu'elle satisfait les contraintes, et notamment qu'elle ne donne pas à certains K_t des valeurs négatives.

Nous allons appliquer cette méthode à une spécification particulière du modèle: celle dans laquelle la population croîtrait de manière exponentielle, le coefficient de capital serait le même pour toutes les périodes, et l'utilité marginale U'_t serait de la forme:

$$U'_t\ (c_t) = (c_t - c_m)^{-u}$$

avec un coefficient u positif et au plus égal à 1 (la grandeur c_m définit comme ci-dessus le niveau minimum de la consommation par personne). Nous remplacerons donc b_t par la constante b et écrirons:

$$P_t = P_0\ (1 + \pi)^t$$

avec un nombre π fixe.

Le modèle ainsi défini a été déjà considéré dans la littérature économique (¹). La forme retenue pour la fonction d'utilité est due à R. FRISCH. Elle semble devoir bien convenir, au moins en première approximation, pour les économies qui sont encore très éloignées d'un quelconque niveau de saturation.

Avec cette spécification, l'équation (56) s'écrit:

$$(c_{t+1} - c_m) = \alpha\ (c_t - c_m)$$

(¹) Voir notamment J. TINBERGEN (1960), ainsi qu'une note de M. BOITEUX, *Taux d'épargne optimal dans une économie en développement* (3 novembre 1961).

α étant la constante définie par:

$$(57) \qquad \alpha^u = \frac{(1 + b)}{(1 + \pi)(1 + \varepsilon)}$$

En conséquence l'évolution de la consommation obéit à:

$$(58) \qquad (c_t - c_m) = \alpha^t \, (c_0 - c_m) \, .$$

Désignons par k_t le capital par personne (K_t/P_t). L'équation (55) implique:

$$\frac{1 + b}{1 + \pi} \, k_t = k_{t+1} + c_{t+1}$$

qui s'écrit encore:

$$(k_{t+1} - k_m) = \beta(k_t - k_m) - (c_{t+1} - c_m)$$

β et k_m étant les deux constantes définies par:

$$\beta = \frac{1 + b}{1 + \pi} \qquad\qquad (\beta - 1)\, k_m = c_m$$

En tenant compte de l'égalité (58), on trouve aisément la solution de la récurrence sur $k_t - k_m$, soit:

$$(59) \qquad (k_t - k_m) = \beta^t \left[(k_o - k_m) - \mu \cdot \frac{1 - \mu^t}{1 - \mu} (c_o - c_m) \right]$$

μ désignant le rapport α/β. La formule (59) s'applique pour toute valeur positive de t si μ diffère de 1. Nous étudierons ci-dessous le cas dans lequel μ serait égal à 1. Pour $t=0$, la formule (59) doit être remplacée par:

$$(60) \qquad k_o = \frac{S_o}{P_o} - c_o$$

Il nous reste à vérifier que ces formules ne conduisent jamais à une valeur négative pour k_t.

Etant donné les ordres de grandeurs que peuvent prendre en pratique b, π et ε, nous admettrons que α et β sont plus grands que 1.

Comme c_0 ne peut pas être inférieur à c_m, il faut que k_0 soit au moins égal à k_m; sans quoi $k_t - k_m$ tendrait vers $-\infty$ et k_t deviendrait tôt ou tard négatif. Nous supposerons donc que le stock initial permet au moins une consommation par personne égale à c_m et un capital par personne égal à k_m, soit:

$$(61) \qquad S_0 \geqq P_0 \; (c_m + k_m)$$

ou encore:

$$S_o \geqq \frac{\beta}{\beta - 1} P_o c_m$$

On peut facilement voir qu'il n'existerait aucun programme possible si cette condition n'était pas satisfaite.

Nous considèrerons encore trois cas suivant que μ est plus grand, plus petit que 1, ou égal à 1.

Premier cas: $\mu > 1$.

La formule (59) montre alors que α^{-t} $(k_t - k_m)$ tend vers $-(c_0 - c_m)$ quand t augmente indéfiniment. La grandeur α^{-t} k_t tend vers la même limite. Pour que k_t ne devienne jamais négatif, il faut que c_0 soit égal à c_m.

Le seul programme régulier est donc celui dans lequel:

$$c_t = c_m \qquad k_t = k_m + \beta^t \ (k_0 - k_m)$$

La consommation est maintenue continuellement à son niveau minimum. L'économie accumule du capital indéfiniment au rythme le plus élevé possible.

Ce programme régulier est le seul programme possible si $k_0 = k_m$; c'est-à-dire si S_0 est juste égal à P_0 $(c_m + k_m)$. C'est bien alors aussi le programme optimal.

En revanche, si S_0 excède P_0 $(c_m + k_m)$, k_0 excède k_m. Le programme régulier n'est évidemment pas optimal. De plus, aucun programme n'est optimal. La situation est tout à fait comparable à celle que nous avons rencontrée dans l'étude du modèle de RADNER pour le cas dans lequel le taux d'intérêt normatif ε est négatif.

Second cas: $\mu = 1$.

Si α est égal à β (c'est-à-dire si $\mu = 1$), la formule (59) doit être remplacée par:

$$(k_t - k_m) = \alpha^t [(k_o - k_m) - t(c_o - c_m)]$$

Il ne résulte que $(k_t - k_m)/t\, \alpha^t$ tend vers $-(c_o - c_m)$ quand t augmente indéfiniment. Pour que k_t ne devienne jamais négatif, il faut que c_0 soit égal à c_m. Il existe donc un seul programme régulier. Si $S_0 = P_0\ (c_m + k_m)$ c'est le programme optimal. Sinon, il n'existe aucun programme optimal.

Troisième cas: $\mu < 1$.

Dans la formule (59), le second terme de l'expression entre crochets décroît à partir de la valeur $-\mu\ (c_0 - c_m)$ pour $t = 1$, et tend vers $-\frac{\mu}{1-\mu}(c_o - c_m)$. Pour que k_t ne devienne jamais négatif, il faut et il suffit que l'expression entre crochets ne soit jamais négative, donc que:

$$(62) \qquad (k_o - k_m) - \frac{\mu}{1-\mu}(c_o - c_m) \geqq 0$$

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En remplaçant k_0 et k_m par leurs expressions et en tenant compte de la définition de μ, nous pouvons transformer cette inégalité en la suivante:

$$(63) \qquad c_o - c_m \leqq \left(1 - \frac{\alpha}{\beta}\right) \left[\frac{S_o}{P_o} - \frac{\beta}{\beta - 1} c_m\right]$$

Le programme régulier maximal donne évidemment à $c_0 - c_m$ la valeur du second membre de cette inégalité, et à $k_0 - k_m$ la valeur $\frac{\mu}{1-\mu}$ $(c_0 - c_m)$. La formule (59) montre qu'alors $(k_t - k_m)$ croît en progression géométrique suivant:

$$(64) \qquad (k_t - k_m) = \alpha^t \; (k_0 - k_m)$$

La consommation et le capital suivent deux évolutions analogues comme le montrent les égalités (58) et (64).

Pour ce programme régulier maximal, on a:

$$(1 + f'_{tK}) \; \frac{S_t}{S_{t+1}} = (1 + b) \frac{P_t}{P_{t+1}} \cdot \frac{c_t + k_t}{c_{t+1} + k_{t+1}}$$

Quand t augmente indéfiniment, cette expression tend vers

$$\frac{1 + b}{\alpha \; (1 + \pi)} = \frac{1}{\mu} > 1$$

La condition 1 est satisfaite, de sorte que le programme en question est optimal.

A titre d'exemple, retenons les valeurs suivantes des diverses constantes: $b = \frac{1}{4}$ correspondant à un coefficient de capital de 4 et à une productivité marginale du capital de 25%, $\pi = \frac{1}{50}$ correspondant à un rythme de croissance démographique de 2% par an et $\gamma = \frac{10}{11}$ correspondant à un taux d'intérêt normatif de 10%. Admettons encore deux valeurs pour le coefficient u: la valeur 0,6 qui a parfois été proposée, et la valeur 1 qui correspond à une fonction d'utilité logarithmique. L'application de la formule (57) conduit à des valeurs de α égales respectivement à 1,19 et à 1,11. Le rythme de croissance de $c_t - c_m$ serait de 19% par an si u valait 0,6, et de 11% par an si u était égal à 1.

Il nous faut examiner maintenant quelles valeurs des paramètres conduisent à une valeur de μ plus petite que 1. D'après la formule (57), α est plus petit que β si [1]

$$1 + \varepsilon > \left(\frac{1 + b}{1 + \pi}\right)^{1-u} \tag{65}$$

Le taux d'intérêt normatif doit être positif si l'utilité $U(c_t)$ est proportionnelle au logarithme de c_t. Il doit être supérieur à une valeur voisine de 8,5% si $b = \frac{1}{4}$, $\pi = \frac{1}{50}$ et $u = 0,6$.

Quand la formulation du modèle suppose que l'accumulation du capital n'entraîne aucune décroissance de sa productivité marginale, le taux d'intérêt normatif doit être suffisamment

(1) On peut observer que c'est aussi la condition pour la convergence de l'utilité $\mathcal{U}_T$ d'un programme dans lequel $(c_t - c_m)$ croît au taux α.

élevé pour que l'on soit en mesure de trouver un programme optimal. Ceci n'est évidemment pas une raison pour rejeter une éthique sociale qui impliquerait un taux d'intérêt normatif faible, et par exemple un taux nul. Les résultats obtenus suggèrent au contraire que l'hypothèse de constance de la productivité marginale du capital ne peut être maintenue que si le taux d'intérêt normatif est notablement supérieur à la borne inférieure de l'inégalité (65). Quand les principes éthiques qui président aux choix collectifs ne satisfont pas cette condition, une détermination valable du programme de croissance suppose la prise en compte du fait qu'une accumulation accélérée du capital se traduit nécessairement par une certaine diminution de sa productivité marginale.

Cette conclusion pourrait être évitée grâce à une modification de la fonction d'utilité. Il nous suffirait d'introduire un niveau de satiété pour retrouver un programme optimal ([1]). Mais les résultats obtenus alors n'auraient sans doute pas grande signification, car ce programme dépendrait dès les premières périodes du niveau auquel serait fixé la satiété. Il semble bien irréaliste de faire dépendre la croissance de pays peu développés d'hypothèses sur l'importance de la consommation qui leur apporterait la satiété.

([1]) J. TINBERGEN et H.C. BOS (1962) ont étudié le cas dans lequel il existerait une consommation de satiété c_M et l'utilité marginale serait de la forme:

$$U'(c_t) = \left[\frac{c_M - c_t}{c_t - c_m}\right]^{\varepsilon}$$

Ils ont montré que, dans le programme optimal, $c_t - c_m$ croissait suivant une courbe logistique. Ils ont noté toutefois que, si ε était nul, le taux d'épargne devait prendre des valeurs très élevées.

6. Fonctions d'utilité linéaires

Les équations de récurrence (22) prennent une forme particulièrement simple dans le cas où l'utilité U_t (c_t , n_t) est une fonction linéaire de la consommation et du travail par personne. Nous allons examiner ce cas qui peut être intéressant en lui-même et qui présente un avantage du point de vue méthodologique. Il nous conduira en effet à étudier comment adapter les procédés décrits ci-dessus au cas dans lequel les contraintes sur la consommation et sur le travail deviennent effectives. Nous poserons donc:

$$U_t(c_t , n_t) = \gamma_t c_t - \nu_t n_t \tag{66}$$

les γ_t et ν_t étant des constantes données non négatives.

A vrai dire une fonction d'utilité de ce type est assez particulière. Elle suppose que le taux marginal de substitution entre consommation et travail est indépendant de la consommation obtenue et du travail fourni. Elle suppose de même que le taux marginal de substitution entre les consommations de deux périodes différentes est indépendant des niveaux auxquels s'établissent ces consommations. On peut douter a priori qu'un critère de choix présentant ces caractéristiques soit vraiment adéquat. Nous allons voir en effet qu'il conduit à des programmes optimaux peu satisfaisants.

Cependant, dans la littérature sur le développement économique, on prend souvent comme critère une valeur actualisée de la suite des consommations futures par personne, ou des consommations futures globales. On se réfère bien alors à une fonction du type (66) dans laquelle les ν_t sont nuls et les γ_t sont tous égaux ou proportionnels aux populations P_t.

Avec la fonction d'utilité retenue ici, les équations de récurrence (22) deviennent:

$$(67) \qquad \begin{cases} 1 + f'_{tK}(N_t, K_t) = (1 + \varepsilon)\dfrac{\gamma_t \; P_{t+1}}{\gamma_{t+1} \; P_t} \\ f'_{tN}(N_t, K_t) \quad = (1 + \varepsilon)\dfrac{\nu_t \; P_{t+1}}{\gamma_{t+1} \; P_t} \end{cases}$$

Elles ne font plus intervenir que les variables N_t et K_t relatives à une même période et n'établissent plus une récurrence véritable.

Pour déterminer un programme régulier, il nous suffit en principe de résoudre, dans chaque période, le système (67) par rapport à N_t et K_t et de déduire C_t de la condition d'équilibre relative à l'instant t. A partir de la seconde période, le programme régulier ne dépend donc plus de la valeur du stock initial S_0.

Ce paradoxe tient au fait que, le plus souvent, les opérations décrites ci-dessus ne définissent pas un véritable programme régulier. Les valeurs trouvées pour certains des C_t sont négatives, ce qui est contraire à la contrainte $c_t \geqq c_m$.

Dans ces conditions, on ne peut espérer obtenir un programme optimal en utilisant seulement les équations de récurrence qui supposent la réalisation d'égalités marginales. Il faut tenir compte plus directement des contraintes sur c_t et n_t.

Afin d'aboutir à des résultats suffisamment précis, nous allons restreindre quelque peu la généralité du modèle. Nous supposerons que la population croît à un rythme constant.

$$P_t = P_0 \;\; (1 + \pi)^t$$

qu'il n'y a pas de progrès technique et que la production s'effectue à rendements constants. Nous poserons:

$$x_t = \frac{K_t}{N_t}$$

et nous écrirons:

$$f_t\ (N_t,\ K_t) = N_t\ \varphi\ (x_t)$$

φ étant une fonction donnée. L'hypothèse 1 implique que φ ait une dérivée φ' qui soit une fonction jamais croissante de x_t. Afin d'éviter des complications supplémentaires, nous supposerons que φ' est une fonction décroissante de x_t. Nous admettrons que l'utilité U_t est la même fonction pour toutes les périodes. Comme nous pouvons la multiplier par une constante positive quelconque, nous l'écrirons:

(68) $$U_t\ (c_t,\ n_t) = c_t - \nu\ n_t$$

ν étant un nombre donné non négatif. Enfin, nous préciserons les contraintes sur c_t et n_t en fixant un niveau de satiété c_M à la consommation et un minimum à la quantité de travail:

(69) $$c_m \leqq c_t \leqq c_M$$

(70) $$n_m \leqq n_t \leqq n_M$$

c_m, c_M, n_m et n_M étant quatre nombres donnés.

Dans ces conditions, nous pouvons écrire:

$$f'_{tK} = \varphi' \qquad\qquad f'_{tN} = \varphi - x\,\varphi'$$
$$U'_{tc} = 1 \qquad\qquad U'_{tn} = -\nu$$

Nous poserons encore:

(71) $$\lambda = (1+\varepsilon)\,(1+\pi)\ .$$

L'inégalité de base (21), qui nous a servi pour comparer les utilités $\mathcal{U}_T$ de deux programmes possibles, devient maintenant:

(72) $$P_o\,\delta\mathcal{U}_T \leqq \sum_{T=0}^{T-1} \lambda^{-t-1}\,(\alpha_t\,\delta K_t + \beta_t\,\delta N_t) - \lambda^{-T}\,\delta S_T$$

α_t et β_t étant définis par:

(73) $$\alpha_t = \varphi'\ (x_t) - \lambda + 1$$

(74) $$\beta_t = \varphi\ (x_t) - x_t\,\varphi'(x_t) - \lambda\,\nu\ .$$

Les égalités (67) impliqueraient que α_t et β_t soient nuls. Mais on observe que ces égalités sont incompatibles en général, car il n'y a aucune raison pour que les deux équations en x

$$\varphi'\ (x) = \lambda - 1$$

et

$$\varphi\ (x) - x\,\varphi'\ (x) = \lambda\,\nu$$

aient une solution commune. Chacune d'elles a une solution au plus; en effet $\varphi'(x)$ est continuellement décroissante par hypothèse et $\varphi(x) - x\,\varphi'(x)$ est continuellement croissante puisque sa variation dans tout intervalle infiniment petit $(x, x + dx)$ est égale à la quantité positive $-x\,d\varphi'(x)$. Désignons par x_K la solution de la première équation, si elle existe. C'est la valeur du rapport $\frac{K}{N}$ pour laquelle la productivité marginale du capital est égale à $\lambda - 1$. Désignons de même par x_N la solution de la deuxième équation, si elle existe. C'est la valeur du rapport $\frac{K}{N}$ pour laquelle la productivité marginale du travail est égale à $\lambda\nu$.

Notons immédiatement que α_t est positif quand x_t est inférieur à x_K, négatif dans le cas contraire. De même β_t est négatif quand x_t est inférieur à x_N, positif dans le cas contraire.

La condition d'équilibre à l'instant t s'écrit ici:

$$C_{t+1} = K_t - K_{t+1} + N_t\ \varphi(x_t) \tag{75}$$

ou encore:

$$(1 + \pi)\frac{C_{t+1}}{n_t} = \varphi(x_t) + x_t - (1 + \pi)\frac{n_{t+1}}{n_t}x_{t+1} \tag{76}$$

Nous devons repérer la valeur $\underline{x}$ de x qui correspond à un programme dans lequel l'utilité serait continuellement égale à son minimum ($c_t = c_m$ et $n_t = n_M$). Nous pouvons de même repérer la valeur $\bar{x}$ correspondant à un programme dans lequel l'utilité serait continuellement égale à son maximum ($c_t = c_M$ et $n_t = n_m$);

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la valeur x_M correspondant à $c_t = c_M$ et $n_t = n_M$; et la valeur x_m correspondant à $c_t = c_m$ et $n_t = n_m$. *Il* résulte de (76) que ces valeurs, si elles existent, sont solutions respectivement de:

$$(77)\qquad \begin{cases} \varphi(\underline{x}) - \pi\,\underline{x} = (1+\pi)\,\dfrac{c_m}{n_M} \\ \varphi(\bar{x}) - \pi\,\bar{x} = (1+\pi)\,\dfrac{c_M}{n_m} \end{cases} \qquad \begin{cases} \varphi(x_m) - \pi\,x_m = (1+\pi)\,\dfrac{c_m}{n_m} \\ \varphi(x_M) - \pi\,x_M = (1+\pi)\,\dfrac{c_M}{n_M} \end{cases}$$

De même, nous pouvons définir $\underline{x}_0$ et $\bar{x}_0$ comme les valeurs que prend x dans la première période si c_0 et n_0 sont fixés d'une part à c_m et n_M, d'autre part à c_M et n_m.

$$(78)\qquad \underline{x}_0 = \frac{S_0}{P_0\,n_M} - \frac{c_m}{n_M} \qquad\qquad \bar{x}_0 = \frac{S_0}{P_0\,n_m} - \frac{c_M}{n_m}$$

$\underline{x}_0$ est évidemment plus grand que $\bar{x}_0$.

Pour une étude complète du problème qui nous intéresse maintenant nous devrions examiner de nombreux cas suivant les positions respectives des valeurs: x_K, x_N, $\underline{x}$, x_m, x_M, $\underline{x}_0$ et $\bar{x}_0$. Nous nous en tiendrons à trois qui suffiront sans doute pour illustrer la détermination et les caractéristiques des programmes optimaux.

Si $\underline{x}_0$ était inférieur à $\underline{x}$, il n'y aurait aucun programme possible. Si $\bar{x}_0$ était supérieur à $\bar{x}$, un programme optimal serait aisément défini, programme dans lequel la consommation serait constamment égale à son maximum et le travail à son minimum. Ces deux cas ne sont guère intéressants. Nous supposerons donc $\underline{x}_0 > \underline{x}$ et $\bar{x}_0 < \bar{x}$.

1) *Premier cas*: $x_N < \underline{x}_0 < x_K < x_M$ (ou, plus généralement, si x_N et x_M n'existaient pas: $\underline{x}_0 < x_K$;
$\varphi(\underline{x}_0) - \underline{x}_0\, \varphi'(\underline{x}_0) > \lambda\, \nu$; et $\varphi(x_K) - \pi\, x_K < (1+\pi)\; c_M/n_M$).

Si $\varepsilon > 0$, un programme optimal $\mathcal{P}^*$ est défini comme suit:

— La quantité de travail est continuellement maintenue à son maximum ($n_t = n_M$ pour tout t).

— La consommation est maintenue à son minimum ($c_t = c_m$) tant que

(79) $$\underline{x}_t = \frac{S_t}{P_t\, n_M} - \frac{c_m}{n_M} < x_K$$

simultanément $x_t = \underline{x}_t$ croît.

S_t est défini par:

(80) $$S_t = n_M\; P_{t+1}\left[x_{t-1} + \varphi\,(x_{t-1})\right]$$

— A partir de l'instant t_0 où $\underline{x}_t \geqq x_K$, x_t prend la valeur x_K; la consommation par personne prend dans la période t_0 la valeur $\dfrac{S_t}{P_t} - n_M\, x_K$ et dans les périodes suivantes la valeur c^* définie par [1]:

(81) $$(1+\pi)\; c^* = \left[\varphi(x_K) - \pi\, x_K\right]\; n_M\,.$$

[1] On peut vérifier que la valeur de la consommation par personne dans la période t_0 est inférieure à ce qu'elle est dans les périodes suivantes.

Les évolutions de c_t et x_t sont schématisées sur la figure 2.

Pour démontrer que ce programme est possible, il nous suffit de vérifier que $\underline{x}_t$ croît avec t et finit nécessairement par atteindre x_K. (Les égalités (79), (80) et (81) établissent que la condition d'équilibre est bien satisfaite à chaque instant).

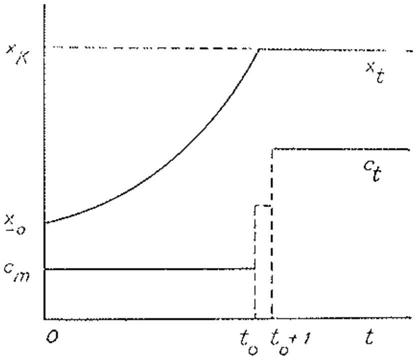

FIG. 2

Des égalités (77), (79) et (80), on déduit aisément:

$$(82) \qquad (1+\pi)\,(\underline{x}_t - \underline{x}_{t-1}) = \left[\varphi(\underline{x}_{t-1}) - \pi\underline{x}_{t-1}\right] - \left[\varphi(\underline{x}) - \pi\underline{x}\right].$$

Or $\varphi(x) - \pi x$ a la dérivée décroissante $\varphi'(x) - \pi$. Pour toute valeur de x inférieure à x_K, cette dérivée est plus grande que $\varphi'(x_K) - \pi = \lambda - 1 - \pi = \varepsilon\,(1+\pi)$. Comme $\underline{x}_{t-1} - \underline{x}$ est positif:

$$(83) \qquad \underline{x}_t - \underline{x}_{t-1} > \varepsilon\,(\underline{x}_{t-1} - \underline{x}) > \varepsilon\,(\underline{x}_0 - \underline{x}).$$

La différence $\underline{x}_t - \underline{x}_{t-1}$ est bornée inférieurement par un nombre positif tant que $\underline{x}_t < x_K$. Ainsi, $\underline{x}_t$ croît et finit certainement par atteindre x_K.

Le programme $\mathcal{P}^*$ est aussi optimal. Soit en effet un autre programme possible $\mathcal{P}^* + \delta \mathcal{P}$. Nécessairement, $\delta N \leqq 0$ pour tout t et $\delta K_t \leqq 0$ pour tout $t < t_0$. Or $\beta_t > 0$ pour tout t; $\alpha_t > 0$ pour tout $t < t_0$ et $\alpha_t = 0$ pour tout $t \geqq t_0$. L'inégalité (72) implique donc $P_0 \, \delta \, \mathcal{U}_T \leqq -\lambda^{-T} \, \delta \, S_T$.

Admettons maintenant que le programme $\mathcal{P}^* + \delta \mathcal{P}$ donne à l'utilité au moins la même valeur que $\mathcal{P}^*$ à partir d'un certain instant T que nous pouvons supposer postérieur à t_0. Pour que $\delta \, \mathcal{U}_T$ soit positif, il faudrait que $\delta \, S_T$ soit négatif. Mais nous allons montrer que ceci n'est pas possible, ce qui établira l'optimalité de $\mathcal{P}^*$.

En effet, en vertu de la concavité de la fonction de production, nous pouvons écrire, pour tout $t \geqq T$:

$$\delta \, S_{t+1} \leqq [1 + \varphi'(x_K)] \, \delta \, K_t + [\varphi(x_K) - x_K \, \varphi'(x_K)] \, \delta \, N_t \, .$$

Par ailleurs, pour tout $t \geqq T$.

$$\delta \, C_t \geqq \nu \, \delta \, N_t \, .$$

Or $\delta \, N_t$ est nécessairement négatif, et $\delta \, K_t = \delta \, S_t - \delta \, C_t$. Par suite de la définition de x_K et du fait que $x_N < x_K$, nous pouvons écrire:

$$\delta \, S_{t+1} \leqq \lambda \, \delta \, S_t - \lambda \, \delta \, C_t + \lambda \, \nu \, \delta \, N_t \leqq \lambda \, \delta \, S_t \, .$$

Cette inégalité montre que δS_t sera effectivement négatif pour tout $t \geqq T$.

Enfin, comme $S^*_{t+1} = (1+\pi) S^*_t$ quand $t \geqq T$.

$$\frac{\delta S_{t+1}}{S^*_{t+1}} \leqq (1+\varepsilon) \frac{\delta S_t}{S^*_t}$$

Comme ε est positif, cette inégalité montre que la valeur absolue de $\frac{\delta S_t}{S^*_t}$ croît au delà de toute limite, et finit nécessairement par dépasser 1. Ainsi $S^*_t + \delta S_t$ devient négatif. Le programme $\mathscr{P}^* + \delta \mathscr{P}$ n'est pas possible.

Nous n'examinerons pas ici ce qui peut se produire quand le taux d'intérêt normatif est nul ou négatif. Ceci nous entraînerait trop loin. Nous retrouverions des situations comparables à celles décrites à propos des deux modèles précédents.

2) *Second cas*: $x_N < \underline{x}_0 < x_M < x_K < \bar{x}$.

La détermination du programme optimal devient plus délicate si le rapport $\frac{K}{N}$ atteint avant x_K la valeur x_M qui permet une consommation maximale avec un travail par personne égal à n_M. Nous ne procéderons pas ici à une vérification précise de l'optimalité du programme que nous allons définir. Quelques indications suffiront.

Il est clair qu'une fois x_M atteint l'accumulation du capital doit permettre une réduction de la quantité de travail par personne. Suivant quelle évolution cette diminution va-t-elle avoir lieu?

Pour découvrir la réponse à cette question, nous pouvons comparer le programme optimal $\mathscr{P}^*$ à un autre programme possible $\mathscr{P}^* + \delta \mathscr{P}$ qui attribue les mêmes valeurs à la consommation ($\delta C_t = 0$). La variation de l'utilité de la période t sera:

(84) $$\delta U_t = \frac{-\nu}{P_t} \delta N_t$$

La condition d'équilibre à l'instant t et la concavité de la fonction f impliquent:

$$\delta K_{t+1} \leqq (1 + f'_{tK}) \delta K_t + f'_{tN} \delta N_t$$

D'où:

$$\delta U_t \leqq \frac{-\nu}{P_t f'_{tN}} \left[\delta K_{t+1} - (1 + f'_{tK}) \delta K_t \right]$$

et pour l'utilité $\mathscr{U}_T$:

(85) $$P_0 \delta \mathscr{U}_T \leqq - \nu \sum_{t=1}^{T-1} \lambda^{-t} \mu_t \delta K_t - \frac{\nu \lambda^{1-T}}{f'_{T-1,N}} \delta K_T$$

les coefficients μ_t étant définis par:

(86) $$\mu_t = \frac{\lambda}{f'_{t-1,N}} - \frac{1 + f'_{tK}}{f'_{tN}}$$

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Le programme $\mathscr{P}^*$ que nous cherchons sera vraisemblablement tel que tous les μ_t soient nuls à partir du moment où c_t atteint son niveau maximal c_M. Examinons donc ce qu'implique la nullité de ces coefficients.

Les productivités marginales f'_{tN} et f'_{tK} dépendent seulement de x_t; de sorte que les égalités $\mu_t = 0$ établissent une récurrence sur les x_t. Une méthode graphique peut servir à la résolution de cette récurrence. Sur une figure portant les x en abscisses, soit Γ_1 et Γ_2 les courbes représentatives des fonctions $g = \log (1 + f'_K) - \log \lambda$ et $h = \log f'_N$. Pour que μ_t soit nul, il faut que $h(x_t) - h(x_{t-1}) = g(x_t)$; c'est-à-dire que l'accroissement de h entre $t-1$ et t soit égal à la valeur de g en t. La figure 3 ci-dessous illustre comment les valeurs successives des x_t peuvent être déterminées.

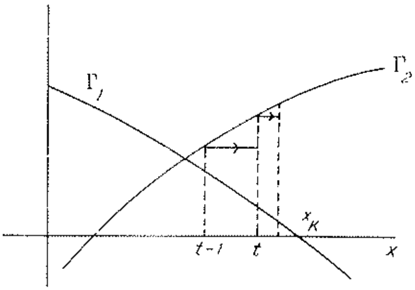

FIG. 3

Comme la courbe Γ_2 coupe l'axe des x au point d'abscisse x_K et que Γ_1 est continuellement croissante, la grandeur x_t croît continuellement et tend vers x_K.

Les x_t une fois déterminés de la sorte, on peut utiliser la condition d'équilibre (76) comme une récurrence sur les grandeurs n_t. Le travail par personne n_t tend vers la quantité n^* définie par:

$$\varphi(x_K) - \pi x_K = (1 + \pi) \frac{c_M}{n^*}$$

La grandeur n_t doit décroître de n_M vers n^*.

Enfin, l'évolution de K_t peut être aisément déterminée puisque $K_t = P_t\ x_t\ n_t$. Le capital par personne $x_t\ n_t$ doit croître à partir de la quantité $x_M\ n_M$ pour tendre vers $x_K\ n^*$. (On peut effectivement vérifier que $x_M\ n_M$ est plus petit que $x_K\ n^*$; mais je n'ai réussi à démontrer ni que n_t décroît continuellement de n_M à n^*, ni que $x_t\ n_t$ croît continuellement de $x_t\ n_t$ à $x_M\ n_M$).

La figure 4 ci-dessous représente l'évolution de la consom-

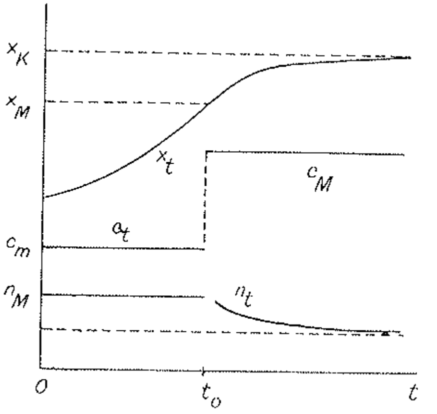

Fig. 4

mation et du travail par personne, ainsi que celle de x_t, dans le programme qui vient d'être déterminé.

Il resterait à vérifier que ce programme est bien optimal quand le taux d'intérêt normatif est positif. Nous ne le ferons pas ici; car ce serait un peu fastidieux.

3) *Troisième cas*: $x_0 < x_N < x_K < x_M$.

Revenons-en à une situation analogue à celle du premier cas, x_K étant inférieur à x_M. Mais supposons que la productivité marginale du travail serait inférieure à $\lambda\nu$, si, dans la première période, la consommation était maintenue à son minimum et le travail à son maximum. Le programme qui a fait l'objet de la figure 2 n'est plus nécessairement optimal. Il se peut que l'utilité soit accrue si l'on réduit le travail fourni dans la première période et que l'on retarde corrélativement le moment auquel la consommation par personne passera de c_m à c^*.

Si une telle éventualité se produit, il semble clair que, dans le programme optimal, le travail fourni sera inférieur à $n_M P_t$ depuis l'instant $t = 0$ jusqu'à une certaine époque θ, que la consommation par personne restera fixée à son minimum jusqu'à une époque t_0 postérieure à θ et qu'elle passera à la valeur c^* à partir de la période $t_0 + 2$.

Afin de faire apparaître des conditions suffisantes pour l'optimalité d'un programme p^*, nous allons comparer ce programme à un programme possible $p^* + \delta p$ choisi de telle manière que:

$$\begin{cases} \delta C_t = 0 & \text{pour} \quad t \leqq \theta \\ \delta C_t = \delta N_t = 0 & \text{pour} \quad \theta < t \leqq t_0 \\ \delta N_t = 0 & \text{pour} \quad t > t_0 \end{cases}$$

En opérant comme pour la dérivation des formules (21) et (85), on obtient:

$$(87) \qquad P_0\, \delta\, \mathcal{U}_T \leqq -\nu \sum_{t=0}^{\theta} \lambda^{-t} \mu_t\, \delta K_t - \frac{\nu\lambda^{-\theta}}{f'_{\theta N}}\, \delta K_{\theta+1} + \\ + \lambda^{-t_0-1}(1 + f'_{t_0 K})\, \delta K_{t_0} + \sum_{t=t_0+1}^{T-1} \lambda^{-t-1} \alpha_t\, \delta K_t - \lambda^{-T}\, \delta S_T$$

les coefficients α_t et μ_t étant définis par les formules (73) et (86). De plus, on sait que:

$$\delta K_{t_0} \leqq \prod_{t=\theta+1}^{t_0-1} (1 + f'_{tK}) \, \delta K_{\theta+1}$$

Des conditions suffisantes pour l'optimalité de $\mathscr{P}^*$ semblent donc être définies comme suit:

1) les μ_t sont nuls pour $t \leqq 0$

2) les α_t sont nuls pour $t > t_0$

3) l'égalité suivante est satisfaite:

$$(88) \qquad f'_{\theta N} \prod_{t=\theta+1}^{t_0} (1 + f'_{tK}) = \nu \lambda^{t_0-\theta+1}$$

Il serait fastidieux de vérifier que, si le taux d'intérêt normatif est positif, un programme possible qui satisfait les conditions 1), 2) et 3) est effectivement optimal. Contentons-nous de quelques remarques.

La condition 1) est une égalité marginale traduisant le fait qu'il n'est pas avantageux de réduire le travail dans la période $t-1$ pour l'augmenter dans la période t de telle façon que le capital K_{t+1} reste inchangé. L'égalité $\alpha_t = 0$ implique qu'il n'est pas avantageux de réduire la consommation de la période t pour augmenter celle de la période $t+1$ sans modifier K_{t+1}. Enfin, l'égalité (88) implique qu'il n'est pas avantageux d'augmenter le travail durant la période θ pour augmenter la consommation dans la période t_0+1 sans diminuer le capital à l'instant t_0+1.

[5] *Malinvaud* - pag. 68

Il n'est pas très aisé de déterminer un programme possible qui satisfasse les conditions 1), 2) et 3). Néamnoins, on peut observer que ces conditions pertant uniquement sur les variables x_t et que l'égalité (88) peut s'écrire:

$$\sum_{t=\theta+1}^{t} g(x_t) + h(x_\theta) = \log \lambda\nu \tag{89}$$

g et h étant les deux fonctions introduites ci-dessus et représentées sur la figure 3. De plus, la condition d'équilibre implique:

$$(1 + \pi)\, x_{t+1} = \varphi(x_t) + x_t - (1 + \pi)\, \frac{c_m}{n_M} \quad \text{pour } \theta < t < t_0 \tag{90}$$

On pourra dès lors opérer de la manière suivante:

— Utiliser la récurrence (90) en sens inverse pour calculer les valeurs des x_t à partir de $x_{t_0+1} = x_K$, t_0 étant supposé connu (Faire comme si cette récurrence s'appliquait pour $t = \theta$ et $t = t_0$).

— Déterminer la valeur de $t_0 - \theta$ à partir de laquelle le membre de gauche de l'égalité (89) excède la quantité log λ ν. (Pour ce faire, on peut utiliser les courbes représentatives de la figure 3).

— En retenant provisoirement la valeur obtenue pour x_θ, utiliser en sens inverse la récurrence

$$h(x_t) - h(x_{t-1}) = g(x_t) \tag{91}$$

pour déterminer les valeurs antérieures de x_t.

— A l'aide de la condition d'équilibre (76), déterminer par récurrence en sens inverse les valeurs de n_t à partir de $n_{\theta+1} = n_M$ (en retenant $c_{t+1} = c_m$).

— Utiliser la définition des x_t et les valeurs obtenues des n_t pour en déduire les valeurs correspondantes de $\frac{K_t}{P_t}$.

— Choisir la valeur de t_0 et réviser en hausse par approximations successives les valeurs de x_{t_0} et de n_0 de telle manière que $\frac{K_o}{P_o} = \frac{S_o}{P_o} - c_m$, que l'égalité (89) soit satisfaite exactement, que x_{t_0} soit inférieur à x_K et n_0 inférieur à n_M et que $h(x_{\theta+1}) - h(x_\theta)$ soit inférieur à $g(x_{\theta+1})$. (On profite alors de ce que l'égalité (90) ne s'applique ni pour $t = t_0$, ni pour $t = 0$).

L'évolution du programme optimal est schématisée sur la figure 5. Le cas étudié ici correspond à une situation dans laquelle le capital initial est peu important. Néanmoins l'accu-

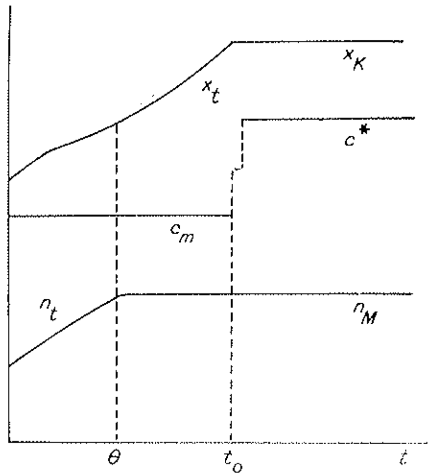

FIG. 5

mulation du capital n'est pas poussée dès le début au rythme le plus élevé possible. Au début la productivité marginale du travail serait trop faible. Bien que pauvre, la collectivité préfère ne pas travailler au maximum possible.

On peut observer que la valeur $x_{\theta+1}$ est inférieure à la valeur x_N pour laquelle la productivité marginale du travail est égale à $\lambda\nu$. En effet, $h(x_N) = \log \lambda\nu$. L'égalité (89) implique alors:

$$g(x_{\theta+1}) + h(x_\theta) \leqq h(x_N) \, .$$

Or, par construction, $g(x_{\theta+1})$ excède $h(x_{\theta+1}) - h(x_\theta)$. Par suite $h(x_N)$ excède $h(x_{\theta+1})$; et x_N excède $x_{\theta+1}$. Il se peut donc très bien que le travail fourni par personne soit égal à n_M dès l'instant o, bien que $\underline{x}_0$ est plus faible que x_N.

Quelqu'incomplète qu'elle soit, cette étude du cas dans lequel la fonction d'utilité est linéaire doit suffire à illustrer une méthode qui convient pour la détermination d'un programme optimal.

Elle montre aussi le caractère peu satisfaisant d'une formulation dans laquelle les utilités marginales ne dépendent ni de la consommation ni du travail fourni par personne. Dans le programme optimal, la consommation subit des sauts brusques qui s'accordent mal avec la notion intuitive d'une croissance harmonieuse.

Il semble que l'on devrait se méfier, même pour les applications, de modèles dynamiques dans lesquels la fonction à maximer est purement linéaire. Les solutions obtenues avec de tels modèles dépendent fortement des contraintes imposées aux variables sur lesquelles porte la maximation (ici c_m, c_M, n_m et n_M). Or il y a toujours un certain arbitraire dans le choix de ses contraintes.

Bien entendu, si les conclusions auxquelles nous sommes parvenus sont aussi tranchées, c'est que nous nous sommes contentés d'une représentation très sommaire des contraintes techniques. Considérant un modèle avec deux secteurs, T.N. SRINIVASAN (1964) a toutefois obtenu des résultats qui présentent, quoique à un degré moindre, le même caractère paradoxal. La difficulté semble bien être générale pour les modèles contenant une fonction d'utilité purement linéaire.

7. Le modèle de Ramsey

Le modèle qui a été employé dans cette étude peut être rapproché de celui introduit par F.P. RAMSEY dans son article célèbre (1928).

RAMSEY utilise une représentation continue du temps. Les symboles C, N, c, n désignent alors le flux de consommation par unité de temps, le flux de travail par unité de temps ... etc. Ce sont évidemment des fonctions de l'instant t considéré. Les symboles K et P désignent le capital et la population à l'instant t.

RAMSEY suppose aussi que la production est instantanée. La production à chaque instant dépend uniquement du flux de travail et du stock de capital au même instant. Le flux de production à l'instant t est une fonction $f(N, K, t)$.

Cette production est affectée immédiatement soit à la consommation, soit à l'accumulation du capital. Il n'y a aucun délai dans la mise en oeuvre de ce capital. La condition (7) d'équilibre à l'instant t devient ici:

$$C = f(\mathrm{N}, \mathrm{K}, t) - \frac{d\mathrm{K}}{dt} \qquad (7')$$

Les simplifications qu'implique une telle représentation sont assez sévères, peut-être même plus que celles résultant du modèle introduit au début de ce mémoire.

Avec une représentation continue du temps, la fonction d'utilité $\mathcal{U}_T$ doit être écrite sous la forme:

$$(12') \qquad \mathcal{U}_T = \int_0^T e^{-\varepsilon t}\, U(c, n, t)$$

ε désignant le taux d'intérêt normatif instantané. Moyennant ces modifications, nous pouvons maintenir la même définition pour les programmes optimaux. Le programme $\mathscr{P}^*$ sera dit optimal s'il est possible et s'il n'existe aucune valeur de T et aucun programme possible $\mathscr{P}^* + \delta\mathscr{P}$ tel que $\delta\mathcal{U}_T > 0$ et $\delta U \geqq 0$ pour tout $t \geqq T$.

De même, la définition des programmes réguliers peut être aisément adaptée. Les égalités marginales qui remplacent les équations (22) pourraient être obtenues directement par application des règles du calcul des variations pour la maximation de l'intégrale (12'). C'est ainsi que RAMSEY les avait déterminées. Afin de montrer la similitude avec l'approche employée ici, nous allons les déduire des équations (22).

Dans le modèle comportant une représentation discontinue du temps, admettons que la période de production ait la durée *dt*, et non plus la durée 1. Admettons que la quantité de travail utilisée pendant cette période soit égale à $N_t dt$, la production à $f_t(N_t, K_t)dt$ et la consommation en t à $C_t\, dt$. Admettons enfin que l'utilité pour la période soit égale à $U_t(c_t, n_t)dt$ et le taux d'intérêt normatif par période à εdt. Les équations (22) deviennent:

$$\begin{cases} U'_{t+dt,c}\,(1 + f'_{tK}\,dt) = (1 + \varepsilon\, dt)\,\dfrac{P_{t+dt}}{P_t}\,U'_{tc} \\ U'_{t+dt,c}\,f'_{tN}\,dt \quad = -(1 + \varepsilon\, dt)\,\dfrac{P_{t+dt}}{P_t}\,U'_{tn}\,dt \end{cases}$$

Supposons alors que la période dt tende vers zéro, la fonction f_t (N_t, K_t) tendant vers la limite $f(N_t, K_t, t)$ et la fonction U_t (c_t, n_t) vers la limite U (c_t, n_t, t). Les équations ci-dessus impliquent alors:

$$(22') \qquad \begin{cases} \dfrac{d}{dt}(U'_c) + U'_c f'_K = \left(\varepsilon + \dfrac{1}{P}\dfrac{dP}{dt}\right) U'_c \\ U'_c f'_N = -U'_n \end{cases}$$

Un programme régulier sera par définition un programme possible satisfaisant le système des équations (7′) et (22′), équations différentielles sur les fonctions N, K et C de t.

L'inégalité (23), qui figure dans la conditions 1, deviendrait pour une période de production égale à dt:

$$(1 + f'_{tK}\, dt)\, \frac{K_t + C_t\, dt}{K_{t+dt} + C_{t+dt}\, dt} \geqq h > 1$$

Lorsque dt tend vers zéro, cette inégalité implique:

$$(23') \qquad f'_K - \frac{1}{K} \cdot \frac{dK}{dt} \geqq m > 0$$

m étant le nombre $h - 1$.

Moyennant cette modification, on pourrait encore démontrer l'optimalité de tout programme régulier qui satisfait la condition I, dans un modèle où les fonctions f et U satisfont l'hypothèse I.

Ramsey considérait en fait un cas particulier de ce modèle, cas dans lequel la population était constante, le taux d'intérêt normatif était nul et les fonctions f et U ne dépendaient pas de t (ce qui excluait tout progrès technique). Les conditions (22) devenaient alors:

$$(22'') \qquad \begin{cases} \dfrac{d}{dt}(U'_c) + U'_c f'_K = 0 \\ U'_c f'_N + U'_n = 0 \end{cases}$$

Ramsey avait observé que l'on pouvait procéder aisément à une première intégration du système défini par (7') et (22'') et le remplacer par le suivant:

$$(92) \qquad \begin{cases} C = f(N, K) - \dfrac{dK}{dt} \\ U + U'_c \dfrac{dK}{dt} = u_o \\ U'_c f'_N + U'_n = 0 \end{cases}$$

u_0 étant une constante d'intégration. En effet, toute solution de (92) est bien solution de (7') et de (22''). Il suffit de vérifier que la première équation de (22'') est satisfaite. En dérivant par rapport à t la seconde équation du système (92), on obtient:

$$U'_c \frac{dC}{dt} + U'_n \frac{dN}{dt} + \frac{d}{dt}(U'_c) \frac{dK}{dt} + U'_c \frac{d^2 K}{dt^2} = 0$$

Après dérivation, la première équation devient:

$$\frac{dC}{dt} = f'_N \frac{dN}{dt} + f'_K \frac{dK}{dt} - \frac{d^2K}{dt^2}$$

qui, reporté dans l'équation précédente, implique:

$$\left[U'_c f'_K + \frac{d}{dt}(U'_c)\right]\frac{dK}{dt} + (U_c f'_N + U'_n)\frac{dN}{dt} = 0$$

En tenant compte de la dernière équation de (92), on retrouve bien les équations (22″).

La détermination et l'étude des programmes réguliers sont évidemment facilitées par la substitution de (92) au système défini par (7′) et (22″). Malheureusement la première intégration à laquelle RAMSEY a pu procéder ne semble pas se généraliser aux cas dans lesquels l'une quelconque de ses hypothèses particulières n'est pas vérifiée.

RAMSEY se limite encore à l'étude de deux types particuliers de programmes réguliers:

1) Dans le programme régulier, C_t, N_t et K_t tendent quand t croît indéfiniment, vers trois valeurs C_M, N_m et K_∞ telles que, pour ces valeurs, $U'_c = U'_n = 0$ et $f'_K > 0$. La condition 1 est alors satisfaite puisque, au moins à partir d'une certaine valeur de t le membre de gauche de (23′) excèdera tout nombre positif choisi à l'avance et plus petit que la valeur limite de f'_K. Quand il existe, un tel programme régulier est bien optimal.

2) Dans le programme régulier, C_t, N_t et K_t tendent, quand t croît indéfiniment, vers des valeurs $\bar{c}$, $\bar{N}$ et $\bar{K}$ telles que, pour ces valeurs, $f'_K = 0$, $U'_c > 0$ et $U'_n < 0$. Alors, la condition I n'est pas satisfaite. Il semble que l'optimalité du programme considéré devra être étudiée dans chaque cas particulier que l'on pourra rencontrer.

* * *

Bien qu'elle ait été limitée à l'examen d'un modèle très simple, cette étude laisse encore sans réponse un certain nombre de questions. Elle montre sans doute l'ampleur des problèmes que pose la détermination des croissances optimales.

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DISCUSSION

KOOPMANS

About the very interesting and remarkable condition 1, it was indicated that it was a sufficient condition — is it also a necessary condition for the one member family of paths that satisfies the three recursive conditions to be an optimal path? Or if it isn't — is there any example of an optimal path that does not meet that condition?

MALINVAUD

Yes, there is an example of an optimal path that would not meet that condition: in the case of the linear logarithmic model with epsilon equal to zero, condition 1 is not satisfied because the left-hand member of (23) is just equal to 1.

KOOPMANS

I have the impression, though, that in most cases this condition is just picking out that one path for which the recursive equations can hold for all times. Is it so that any path that does not meet condition 1 but satisfies the recursive requirements necessarily violates at some finite time, the sign restrictions on capital or consumption? Or is that not correct?

MALINVAUD

No, I do not think that is correct. For instance in the linear logarithmic case with epsilon greater than 1, there is a whole family of paths which meets the recursive requirements and the sign restrictions. Just one path satisfies condition 1 and is therefore optimal.

(*)

HAAVELMO

I have a somewhat strange question which may or may not be relevant to what Professor KOOPMANS said. It is this: it seems that political groups of powers often see it as their task to try to impose upon people, or to convince people that they should accept a smaller discount rate for evaluating the future gains from development projects than would appear to be the individual discount rate, and as time passes, people often say that the politicians were right. Now I have a feeling that this may have some connection with KOOPMAN's thesis, but I am not quite sure.

ALLAIS

I will begin with a few remarks relating to both papers. First point: both papers use a single preference function. Perhaps this may be useful, but it can be quite dangerous for a very drastic and very strong hypothesis is introduced into the models and some of the conclusions derived using this hypothesis can be questioned. Even if a single preference function could be assumed, the form of this preference function would be open to discussion. In the

(*) From here on the discussion concerns both the paper presented by MALINVAUD and the earlier one presented by KOOPMANS.

KOOPMANS paper the preference function is a function of one variable x_t only; in the MALINVAUD paper it is a function of two variables n_t and c_t, work and consumption. The way in which the utility functions are discounted is also open to discussion. From the point of view of the theory of optimum allocation of resources I do not see any economic reason for such discounting. It is a pure hypothesis, and in the general theory of optimum allocation of resources no justification of this procedure is to be found.

In my second point I join Prof. FRISCH's position, but only from a theoretical point of view. I think that from the standpoint of theory, it is very interesting to separate the problem of optimal economic growth into two problems. The first is the study of what happens if we limit ourselves to the production function; the second is the introduction of preference functions. I think that in economic programming, this procedure is not valid, but from the point of view of theory, it is very useful indeed, since the difficulties resulting from the consideration of the utility function can be avoided. Namely, is it possible to consider only one utility function? and if so what utility function must we consider? etc. In fact, we can obtain very general results even if some strong hypothesis such as convexity in the ordinary sense is not taken into account.

Third point: for this reason, I think my point is bound up with the second one. Professor KOOPMANS has said that there is no optimal path with a negative value of the rate ρ of his paper. Certainly this conclusion is absolutely true for the model considered by Professor KOOPMANS. All these models are logically consistent. But in my opinion it is very interesting to study independently the path which can be considered as optimum if no attempt is made to take the psychological point of view into account. And in this case very precise conclusions can be derived even if the rate ρ has a negative value.

Fourth point: so far as MALINVAUD's paper is concerned, I apologize for repeating what I said in Cambridge last July. Under the hypothesis considered by MALINVAUD, the rate of interest must be greater than the rate of growth of primary income. This result is not open to doubt if, but only if, we are limiting ourselves to the physical point of view. But if human psychology is taken into account, this result does not remain valid. There are two reasons for this; I can give two examples. The first case is where the shape of the time preference curve implies a high preference for the future. In my book « Economie et intérêt » in 1947 I studied a model in which the difference i-ρ between the rate of interest and the rate of growth of the primary income is negative, but nevertheless there is optimality in the paretian sense with an infinite horizon. My second example relates to the case where the utility functions are functions not only of consumption, but also of capital goods. If people want very much to possess capital goods, then there can be an optimal path with a negative difference i-ρ.

My fifth point is that my Econometrica paper is only one study carrying forward things described in many preceding papers and I believe that I gave very precise consideration to the problem of the optimal path as long ago as 1947 in my book « Economie et intérêt », that is fifteen years before the DESROUSSEAUX, PHELPS, JOAN ROBINSON, SWAN and VON WEIZSÄCKER studies which Professor KOOPMANS mentioned.

MALINVAUD

In order to avoid the conclusion that no optimal program would exist, one has suggested that we drop the assumption of an infinite horizon. I cannot accept this point of view. Considering an infinite horizon often leads to interesting results concerning the non-optimality of programs which would appear as optimal if time were limited to some specific date, however far in the future this date may be. In such non-optimal programs, the economy is accumulating too much capital all the time and never take for consumption the full benefit of its high capital endowment. I see no way of discarding these programs if a finite horizon is adopted and if the terminal capital stock is taken as a constraint.

With regard to one remark made by Professor ALLAIS, I should make clear I did not prove, in the paper presented here, that the interest rate had to be larger than the growth rate. The inequality between the two rates was introduced as a *sufficient condition* for a general result concerning optimal programs. However, in most particular cases I considered here, the rate of interest is larger than the rate of growth all the time.

ALLAIS

The point is, if this proposition cannot be proved in a general way, there cannot be an optimal path with the condition i smaller than ρ. I therefore cannot see the meaning of the preceding proposition.

MALINVAUD

In this paper, I introduced the condition only because I was unable to find a result without it. But I may remark incidentally that a finite horizon was present in the cases where optimal programs were found with an interest rate smaller than their growth rate.

KOOPMANS

Supplementing Professor MALINVAUD's remarks I do not think that the response to the difficulties I have pointed out should be to drop the infinite horizon. I think if you make a very large horizon, the same difficulty that shows itself starkly with an infinite horizon will also show itself somewhat less starkly but in an equally disturbing manner with a very large finite horizon. Thus the infinite horizon is a mathematically explicit way of bringing the presence of a mathematical limitation to ethical thought to our attention.

Professor HAAVELMO pointed out that sacrifices enforced at one time may later be endorsed by public opinion, perhaps when the benefits from that earlier sacrifice become apparent. This would re-enforce an idea I have expressed in my paper but not in my presentation. Perhaps the discount rate ρ itself should be a function of the level of consumption reached. I would expect that if we get close to affluence ρ diminishes. It is conceivable that one could find ways of making ρ depend on the consumption level in such a way as to avoid the difficulties that I have encountered.

Several speakers have asked whether these strange results are due to the assumption of a double commodity that can both be eaten and used as capital. So far I do not know of more detailed or elaborate studies directed to the same question; so I can only state my hunches. I would think that essentially the same results would be found with other forms of indefinitely continuing population growth as long as the percentage rate of growth stays above some positive percentage. I think one would find the same difficulty even more strongly if one introduces technological progress in addition. It is possible, however, that resource limitations not ultimately compensated by technological progress could work in the opposite direction and would do away with the conclusion. If the single social preference function is replaced by individual preference functions and a market mechanism is introduced of the type that Professor ALLAIS has stressed several times in the discussion, I would not want to venture a guess as to whether the difficulty I have encountered would remain or disappear.

As to Professor ALLAIS' statement that there is no reason for discounting, I started out with that idea myself. But I found that for there to exist an optimum path, I had to either discount or discriminate against people on the basis of how many there are in a given generation.

DYNAMIC STRUCTURE AND ESTIMATION IN ECONOMY-WIDE ECONOMETRIC MODELS

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1. INTRODUCTION AND CLASSIFICATION OF ESTIMATORS

1.1. *General Introduction*

This paper is concerned with the techniques of and the problems in the structural estimation of economy-wide econometric models. Briefly stated, the essential general features of such models which raise special problems for estimation are as follows. They tend to involve a large number of equations and variables; they are nearly closed in the sense that most of the variables of the model are endogenously determined; they are dynamic and essentially interconnected in the sense that, considered as dynamic systems, they are indecomposable; finally, the disturbances from different equations tend to be correlated with each other and with their own past values. All of these features will be discussed at greater length below, and all

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of them raise problems of varying magnitude for structural estimation.

As is well known, there are now a fairly large number of alternative estimation techniques available for such estimation. Such methods fall into classes which differ in the assumptions made or amount of information taken into account. They also have different properties. In general, a great deal is known concerning the asymptotic properties under ideal conditions of most of these estimators; rather less is known of small sample properties; and a very few results are available on their relative robustness — the relative degree to which they stand up to such things as multicollinearity, specification error, and serial correlation in the disturbances of the model.

This paper begins by reviewing the known properties of the principal estimators in the context of economy-wide models. We observe that the features of such models mentioned above make the use of even the best of such estimators rather suspect in its original form, while the size of such models makes them literally unavailable when time series of lengths usually encountered are the data involved. This leads naturally to estimation using instrumental variables in some form, and the second half of the paper is devoted in one way or another to exploring the question of how appropriate instrumental variables should be chosen. It is argued that this is best done through continual application of the *a priori* structural information which governs the formulation of the entire model in the first place, rather than through relatively arbitrary statistical devices.

1.2. *Classification of Estimators*

For our purposes, the estimators which have been proposed for structural estimation may be divided into three classes. The first of these consists of ordinary least squares and its generalizations. The second includes: two-stage least squares; limited-information maximum likelihood; the other members of Theil's k-class; Theil's h-class; and Nagar's double k-class (2). All the estimators in this group have the common property that whereas (unlike ordinary least squares) they take account of the simultaneous nature (if any) of the equations in the model to be estimated, they use only *a priori* restrictions on one equation at a time. Accordingly, we shall call such estimators « limited information » methods. The last class of estimators consists of those methods which do use information on all equations at once, what we shall term « full information » methods. Among these, of course, is full-information maximum likelihood, but the class also contains A. ZELLNER and H. THEIL's three-stage least squares, an estimator recently proposed by T.J. ROTHENBERG and C.T. LEENDERS called « linearized maximum likelihood », and the simultaneous least squares estimator of T.M. BROWN (3).

In principle, all of the above estimators make use of all exogenous and lagged endogenous variables in the model as predetermined instruments. As indicated above, for reasons to be discussed below, this cannot always be done or is not always desirable, and in such cases other methods which so employ only some of the exogenous or lagged endogenous variables must be used. We shall discuss the problems raised in such situations below, observing here only that, given the choice of variables to be treated as predetermined, most of the estimators just classified have exact counterparts in such circumstances.

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(2) See THEIL [32, pp. 353-354] and NAGAR [23].

(3) See ZELLNER and THEIL [37], ROTHENBERG and LEENDERS [26], and T.M. BROWN [6a].

2. Ordinary least squares

2.1. *Assumptions and Properties*

Ordinary least squares has a number of desirable properties when appropriate assumptions are satisfied. Briefly, if the explanatory variables in the equation to be estimated are either non-stochastic or distributed independently of all past, present, and future values of the disturbance term in that equation, if the disturbance term is serially uncorrelated and homoscedastic, and if there are no *a priori* restrictions on the parameters to be estimated, then ordinary least squares is the best linear unbiased estimator. In addition, if the disturbances are normally distributed, then ordinary least squares is the maximum likelihood estimator.

These assumptions can be weakened in several ways. First, if the explanatory variables are not independent of the disturbance term but are uncorrelated with it in the probability limit, then ordinary least squares ceases to be unbiased but is consistent. If the disturbances are serially correlated, ordinary least squares loses efficiency but retains consistency provided that such serial correlation does not affect the validity of assumptions concerning the correlation of the current disturbance term and the explanatory variables (a matter to which we shall return) (4). Finally, ordinary least squares presents no particular difficulties of computation.

As is well known, however, the minimum assumption for the consistency of ordinary least squares — that the explanatory variables are uncorrelated with the disturbance term — cannot be maintained if the equation to be estimated is one of a system of simultaneous structural equations. In this case, ordinary least squares loses even consistency when used as an estimator

(4) See Theil [32, pp. 219-225] or Johnston [15, pp. 192-195] for a discussion of this case.

of a structural equation, although not when used to estimate the equations of the reduced form.

This argument is not sufficient, however, to dismiss ordinary least squares from consideration as an appropriate estimator in large econometric models. In the first place, there is the question emphasized by H. WOLD (5) as to whether such models really should be simnltaneous given the nature of causation. Second, the issue is not of the yes-or-no variety as it is often made to appear; rather, if the model is such that correlation between the disturbance term and the explanatory variables in the given equation can be appropriately assumed to be *small* (rather than zero) or if the variance of the disturbance is known to be small, then least squares will be *almost* consistent (6). One may then be willing to accept the small inconsistencies involved for the sake of the other properties of the estimator, principally its relatively small variance around its probability limit. We must therefore go on to ask when this is likely to happen and when the assumptions of WOLD's recursive model are likely to be approximately satisfied.

2.2. *Recursive Systems and Necessary Assumptions*

Suppose that the model to be estimated is:

$$y_t = Ay_t + By_{t-1} + Cz_t + u_t \tag{2.1}$$

where u_t is an m-component column vector of disturbances; y_t is an m-component column vector of current endogenous variables; z_t is an n-component column vector of exogenous variables (known at least to be nncorrelated in the probability limit with all current and past disturbances); A, B, and C are constant matrices to be estimated; and (I – A) is nonsingular,

(5) WOLD and JURÉEN [34, 50-51] and other writings.
(6) WOLD and FAXÈR [35].

while A has zeros everywhere on its principal diagonal. The assumption that there are no terms in $y_{t-\theta}$ for $\theta > 1$ involves no loss of generality in the present discussion, since it can always be accomplished by redefinition of y_t and expansion of the equation system and will be used only for convenience in dealing with the solution of (2.1) regarded as a system of stochastic difference equations.

If:

(R.1) A is triangular;

(R.2) The variance-covariance matrix of the current disturbances is diagonal;

(R.3) No current disturbance is correlated with any past disturbance;

then the model is recursive and does not violate the assumption that in each equation the disturbance term is uncorrelated with the variables which appear therein other than the one to be explained by that equation. Ordinary least squares is then a consistent estimator and is the maximum likelihood estimator if each element of u_t is normally distributed and homoscedastic.

To see that the no-correlation assumption is not violated, we solve the system for y_t, obtaining:

$$(2.2) \qquad y_t = (I - A)^{-1} B y_{t-1} + (I - A)^{-1} C z_t + (I - A)^{-1} u_t.$$

Denote $(I - A)^{-1}$ by D and note that it is triangular by (R.1). We may take the zero elements to lie above the principal diagonal. Assuming that DB is stable, we have: (7)

$$(2.3) \qquad y_t = \sum_{\theta=0}^{\infty} (DB)^{\theta} (DCz_{t-\theta} + Du_{t-\theta})$$

(7) We shall not discuss the assumption of the stability of DB in any detail at this point. If it is not stable, then it suffices to assume that the model begins with non-stochastic initial conditions. Obviously, if stability fails the assumption of no serial correlation becomes of even greater importance than if stability holds. We shall return to this and shall discuss the question of stability in general in a later section.

Denoting the covariance matrix of u_t and $y_{t-\theta}$ by $W(\theta)$ with columns corresponding to elements of u_t and rows corresponding to elements of $y_{t-\theta}$, and that of u_t and $u_{t-\theta}$ by $V(\theta)$ (which is assumed to be independent of t), with columns corresponding to u_t and rows to $u_{t-\theta}$:

$$W(0)=\sum_{\theta=0}^{\infty}(DB)^{\theta}(DV(\theta)) \tag{2.4}$$

Since, by (R.3), $V(\theta)=0$ for $\theta>0$, this becomes:

$$W(0)=DV(0). \tag{2.5}$$

By (R.2), $V(0)$ is diagonal, hence $W(0)$ is triangular with zero elements above the principal diagonal. Thus any element of y_t is uncorrelated with all higher-numbered elements of u_t. Similarly,

$$W(1)=\sum_{\theta=1}^{\infty}(DB)^{\theta-1}(DV(\theta))=0 \quad . \tag{2.6}$$

Hence all variables which appear in any given equation in (2.1) save that variable which is to be explained by that equation are uncorrelated with the disturbance from that equation, as stated.

We have gone through this demonstration in detail partly for later purposes and partly to exhibit the way in which each of the assumptions (R.1)-(R.3) enter. We must now ask whether those assumptions can be weakened.

In the first place, it is clear that the triangularity of A is crucial. From (2.5), if A and therefore D is not triangular, then $W(0)$ will not be triangular either in general, and the elements of y_t cannot be taken as uncorrelated with higher-numbered disturbances. This is well known, as in this case the system (2.1) is truly simultaneous. In such a case, ordinary least squares will be inconsistent for at least one equation in the model.

What is less often realized in practice is the role played by the assumptions on the disturbances. Because of the simplicity and other advantages of ordinary least squares, there is a natural tendency to settle for a triangular A and to overlook the fact that such triangularity does not suffice to make ordinary least squares consistent ([8]).

To see that such assumptions are generally required, consider first the assumption that V(o) is diagonal. If this fails, then (2.5) shows that W(o) cannot generally be taken to be triangular, whence ordinary least squares will be inconsistent. This corresponds to the intuitive idea that if a high-numbered and a low-numbered disturbance are correlated, the endogenous variable corresponding to the low-numbered disturbance cannot be taken to be uncorrelated with the high-numbered disturbance even if thre is no direct influence through the explicit equations of the model. Indeed, not only is the diagonality of V(o) required for the consistency of ordinary least squares, but also, if nothing more is known of the coefficients of the model save that A is triangular, such an assumption is necessary for the very identifiability of the equations ([9]).

It is possible, however, to alter the assumption of no serial correlation. Clearly, this enters in both (2.5) and (2.6) because y_{t-1} appears in the model. If this were not the case, the assumption in question would not be needed for consistency. In most econometric models, however, and certainly in economy-wide ones, lagged values of the endogenous variables do in fact appear. We are nevertheless able to weaken the no-serial-correlation assumption (R.3) to:

(R.3*) B (as well as A) is triangular with zeros above the diagonal, and for all $\theta > 0$, $V(\theta)$ is triangular with the same arrangement of zeros so that high-numbered disturbances are

([8]) In fairness, it should be pointed out that WOLD's theoretical writings are entirely clear on this point. See WOLD [36, pp. 358-359], for example.
([9]) See FISHER [10].

uncorrelated with lagged values of low-numbered disturbances. Further, either B or all V(θ) (θ>o) have zeros everywhere on the principal diagonal.

If (R.1), (R.2), and (R.3*) hold, every term in (2.4) will be triangular, so that W(o) will likewise be triangular as required. Further, W(1) will also be triangular rather than zero and will have zeros on its principal diagonal, but this will be all that is needed, since if B is triangular no lagged endogenous variable appears in an equation of (2.1) explaining a lower-numbered current endogenous variable.

Intuitively, the general necessity of no serial correlation for the consistency of least squares is that an element of y_{t-1} is influenced by an element of u_{t-1}. If that element of u_{t-1} is itself correlated with a lower-numbered element of u_t, then the corresponding element of y_{t-1} cannot be assumed to be uncorrelated with that element of u_t. Even if V(θ) is triangular for θ>o (or even diagonal) and B is not triangular, the dynamics of the system will carry serial correlation into relations between any current disturbance and any current endogenous variable. If both V(θ) and B are triangular, however, such effects are only carried toward higher-numbered equations.

That triangularity of both V(θ) for all θ>o and B are generally necessary in the presence of serial correlation may be seen from the fact that since D is triangular, the terms in (2.4) will generally not otherwise be triangular and the fact that if B is not triangular, even triangularity of W(1) will not suffice. (The condition as to the principal diagonals can be easily seen to be required by considering a single-equation model).

Of course, as is also the case for the assumption of the diagonality of V(o), even if such assumptions fail generally, similar weaker assumptions concerning certain off-diagonal elements may hold and yield the consistency of ordinary least squares for certain equations. The indicated assumptions *are* necessary for such consistency in *all* equations, however. (Such weaker conditions are fairly readily obtained from the generalization of the current discussion given in a later section).

2.3. *Recursive Systems in Economy-wide Models*

Are the assumptions of the recursive model just discussed likely to be valid for an economy-wide econometric model? In general, the answer appears to be in the negative.

In the first place, the argument for the triangularity of A that causation takes place sequentially in time (which, incidentally, would imply diagonality) is not conclusive if the data are collected as averages over a much longer period than the causal interval involved. It may be true that simultaneous structures are but approximations to underlying recursive ones with very short time lags; this does not make the matrices involved triangular, however, whatever it implies about appropriate estimators (10).

Even if triangularity of the A matrix is satisfied in an economy-wide model, however, the other conditions discussed are unlikely to be fulfilled. Even the best specified econometric models inevitably omit variables the effects of which then enter the disturbance terms. If the model is well specified, these effects will not be large and systematic, rather they will be small and random. Even so, the omitted variables appearing in the disturbances cannot all generally be expected to be different ones for different equations. Indeed, one expects there to be some events which act as shocks on many or all the equations in an economy-wide model. Such action may indeed be of different magnitudes for different equations, but it is surely extremely restrictive to assume zero correlation among the

(10) STROTZ [30] considers a model in which the variables are observed at discrete intervals longer than a short causal period which is allowed to approach zero — a problem not quite the same as that considered in the text. He argues that the usual estimators are not approached in the limit by the maximum likelihood estimator of his model. The status of the argument is presently in some doubt as GORMAN [11] has suggested that « natural » assumptions on the continuity of the stochastic process generating the disturbances do lead to the usual estimators in the limiting case of simultaneity.

different disturbance terms. Thus it is very unlikely that V(o) will be diagonal.

Similarly, it is rather unrealistic to assume no serial correlation in the disturbances. Disturbances from econometric models do in fact tend to be serially correlated and while we shall later argue that correlation between a given element of u_t and a different element of $u_{t-\theta}$ may be small, even a diagonal $V(\theta)$ for $\theta > o$ will not help. This is especially the case if the time lag involved in the model is small (the very situation in which triangularity of A is relatively likely), as in such a case the effects of a random shock due to an omitted variable are likely to persist for more than one time period. To put it another way, it is natural to suppose that as the time period involved goes to zero, V(1) approaches V(o) which is certainly not zero ([11]).

Moreover, there seems little direct comfort in the points made above that it is sufficient to have B = o or to have both B and all $V(\theta)$ $\theta > o$ triangular and either B or all such $V(\theta)$ with zero principal diagonals. Economy-wide models are generally dynamic ones so that lagged endogenous variables do appear. Further, while we shall argue below that a diagonal $V(\theta)$ for $\theta > o$ is not quite so unreasonable as it may seem, a triangular B matrix is wholly unlikely, since this would be a case in which there were *no* feedbacks (simultaneous or lagged) from one variable to another and economy-wide models simply do not have such a hierarchic structure in view of the interconnectedness of economic activity.

It is thus evident that even if one is willing to assume a triangular A matrix, the assumptions of the recursive model cannot generally be taken as valid in an economy-wide econometric model. This is especially true if triangularity has been achieved by the introduction of relatively short time lags. At the risk of over-emphasis, we repeat. Ordinary least squares

([11]) See GORMAN [11].

does not become consistent when one changes a current endogenous variable to a recent past value of the same variable even if triangularity of the A matrix is achieved in this way. The assumptions which lead to the consistency of least squares require more than this and all the same difficulties will still be encountered even if they go unrecognized.

2.4. *The Proximity Theorem and Near-Consistency*

As already remarked, however, the issue of the use of ordinary least squares (or indeed of any particular estimator) is not whether the assumptions thereof are precisely satisfied. Rather the crucial question is that of how closely they are satisfied, of how those assumptions stand up as approximations rather than as exact statements. The problem is not a discrete one; rather it is continuous. Moreover, the question of goodness of approximation is itself dependent on the sensitivity of the properties of the estimator to variation in the assumptions thereof. In general, the less sensitive is an estimator, the greater the tolerable deviation from the strict conditions under which it has desirable properties.

In the present instance, our discussion has largely run in terms of consistency. Consistency, however, is a rather weak, although desirable property. Since ordinary least squares has several other attractive features, we might plausibly be willing to tolerate small inconsistencies to gain, for example, computational ease, small variance around probability limits, and so forth. It is thus not sufficient to ask whether the assumptions under which ordinary least squares is consistent are satisfied; we must ask whether the fact that they are not generally satisfied in economy-wide econometric models is likely to be of much importance.

This question is formally answered by the Proximity Theorem of Wold [12]. That theorem states that the inconsistency of least squares will be small the smaller are the correlations between the explanatory variables in the equation to be estimated and the disturbance from that equation and also the smaller is the variance of that disturbance. A perhaps more illuminating way of looking at the same thing is to consider the disturbance as made up of a linear combination of omitted variables. The inconsistencies in the parameter estimates can then be shown to be equal to the coefficients of the multiple regression of the disturbance term on the explanatory variables [13].

For our purposes, the Proximity Theorem shows that if (R.1), (R.2), and (R.3) or (R.3*) hold approximately, the inconsistency of ordinary least squares will be small. Indeed, that inconsistency will be small in a given equation if the appropriate columns of

$$(2.7) \qquad W = \begin{bmatrix} W(0) \\ W(1) \\ 0 \end{bmatrix}$$

premultiplied by the inverse of the variance-covariance matrix of the variables appearing on the right of that equation is small. Since that inverse enters the ordinary least squares parameter estimates in precisely the same way, we may say that (roughly) *relative* inconsistencies will be small provided that W(0) and W(1) are small. Thus, if all terms above the diagonal in A are *nearly* zero; if cross-equation covariance between contemporary disturbances is small; and if there is little serial correlation, ordinary least squares will not do too badly.

Unfortunately, there is reason to believe that this will not generally be the case. The arguments given above for the

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([12]) Wold and Juréen [34, p. 189 and pp. 37-38]. The Proximity Theorem as stated by Wold is one concerning bias; we discuss inconsistency since unbiasedness is not in any case a property of least squares in models with lagged endogenous variables. See Hurwicz [14].

([13]) See Fisher [8] and Theil [31].

failure of (R.2) and (R.3), in particular, are arguments that the excluded effects are likely to be substantial in practice. While one may be willing to assume that they are not so in particular cases, depending on the structure of the model to be estimated, this seems a dangerous procedure in most economy-wide models given the high degree of approximation which such models inevitably involve. The Proximity Theorem in more general form will be of considerable help to us below and is of substantial value in other contexts; for structural estimation in economy-wide models, it seems a weak reed on which to rest estimation by ordinary least squares.

2.5. *Reduced Form Estimation*

Our discussion thus far has run in terms of the estimation of the parameters of structural equations. The simultaneous model context in which ordinary least squares is most often thought to be appropriate, however, is not this at all, but rather in the estimation of the equations of the reduced form. Here the difficulties in the use of ordinary least squares which arise from simultaneity apparently disappear as all variables on the right-hand side of reduced form equations are either exogenous or lagged.

In this connection, the argument against the use of ordinary least squares has generally run in terms of lack of asymptotic efficiency when compared with estimates of the reduced form which are derived from structural estimates using overidentifying *a priori* information. Such lack of asymptotic efficiency may be particularly important in the event of a structural break or in the prediction of turning points ([14]). The argument in favor of ordinary least squares estimates of reduced form equations has been the desirability of having forecasts of the endo-

([14]) Lesnoy [18].

genous variables which are unbiased conditional on the values of the predetermined variables ([15]). It has also been suggested that the added asymptotic efficiency in the use of other estimators stemming from the employment of *a priori* information may in fact frequently be quite illusory as such information may be incorrect ([16]).

There is substantial merit in all of these arguments in various contexts. Fortunately, the issue is rather easy to decide in the context of estimation of the reduced form of a dynamic economy-wide econometric model. In the first place, such a model generally involves lagged endogenous variables. To estimate even the reduced form by ordinary least squares when such variables appear on the right-hand side does not yield consistent estimates in the presence of serial correlation, substantially as seen above. Moreover, even if the assumption of no serial correlation is made, ordinary least squares still does not give an unbiased estimate of the parameters nor a conditionally unbiased forecast of the dependent variable. Nevertheless, one might plausibly be willing to accept such defects in ordinary least squares for the sake of greater efficiency. Such efficiency fails, asymptotically, however, if the overidentifying information on which structural estimation by other means is based is approximately correct as the issue is again one of good approximation rather than of correctness ([17]). Since restrictions on coefficients are more likely to be good approximations than are restrictions on disturbances concerning which economic theory provides relatively little information, ordinary least squares is unlikely to be asymptotically efficient.

On the other hand, such information as is available on the small sample properties of the limited-information estimators (discussed below) does suggest that asymptotic efficiency may

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([15]) WAUGH [33], FISHER [9].
([16]) LIU [19].
([17]) See FISHER [8].

not be remarkably important as the small sample variances of such estimators are infinite in some cases. Ordinary least squares certainly does have the property of finite small sample variances under ordinary conditions, however defective it may be for other reasons. Ordinary least squares estimates of the reduced form equations may therefore be appropriate ones to consider if one is willing to assume that serial correlation is unimportant.

Note that this is not quite the same as the situation as regards structural estimation already discussed. In that context several strong assumptions have to be nearly satisfied in order to justify the use of ordinary least squares. In the present context, only the assumption of no serial correlation must be approximately satisfied; if it is, the remaining argument against ordinary least squares is the one of lack of asymptotic efficiency and this may be by no means decisive in a world of relatively small samples (18).

In practice, however, ordinary least squares estimation of the reduced form of a large economy-wide model is simply incapable of accomplishment. If all lagged endogenous variables are treated as predetermined, the number of exogenous and predetermined variables in any but the most aggregative economy-wide model is simply too large to permit this type of estimation in the presence of the relatively low number of observations ordinarily available.

3. Full-information estimators

We now discuss the class of full-information estimators out of what is perhaps the natural order, because it is relatively

(18) All of our discussion of the effects of serial correlation has overlooked the existence of estimation techniques designed precisely to deal with that problem. See for example Johnston [15, pp. 192-195] and Theil [32, pp. 219-225]. All of these techniques, however, assume that there are no lagged endogenous variables in the model, and we have principally been concerned with the problems raised by serial correlation when there are such lagged variables.

easy to dispose of it. We shall then be free to turn our attention to the class of estimators ordinarily used in these problems — the limited-information class.

It is customary in these discussions to pay lip-service to full-information maximum likelihood as the optimal estimator using all information available and then to dismiss it in practice as too difficult of computation. While it is still true that such computational difficulties are still prohibitive in practice for even moderately large systems ([18a]), such dismissal no longer suffices. This is the case because there are now two full-information estimators which are known to have the same asymptotic distribution as full-information maximum likelihood and which are not particularly difficult to compute. These are the three-stage least squares estimator proposed by ZELLNER and THEIL and the linearized maximum likelihood method of ROTHENBERG and LEENDERS ([19]). Since the known virtues of full-information maximum likelihood are all asymptotic, computational difficulty can no longer be considered a valid reason for not using some such method.

As it happens, however, there are more cogent reasons than computational difficulty for the abandonment of full-information methods in practice. However desirable the properties of full-information methods may be in principle when all assumptions are met, such estimators suffer relatively heavily from a lack of robustness in the presence of common practical difficulties. Thus KLEIN and NAKAMURA have suggested that full-information maximum likelihood is more sensitive to multicollinearity than are limited-information estimators ([20]).

([18a]) The difficulties are being overcome, however. See EISENPRESS [7a].

([19]) ZELLNER and THEIL [37]; ROTHENBERG and LEENDERS [26]. ROTHENBERG and LEENDERS give the proof that these estimators have the same asymptotic distribution as full-information maximum likelihood. See also SARGAN [27] and MADANSKY [20]. BROWN's simultaneous least squares [6a] [which is a member of the full-information class] is known to be consistent but is not known to have the same asymptotic distribution as the other members.

([20]) KLEIN and NAKAMURA [16].

Further, it is evident that all full-information methods are rather sensitive to specification errors of the types that are unavoidable in the foreseeable state of econometric models which are only approximate. In particular, such estimators have the defects of their merits in that by using information on the entire system to estimate any single equation, they carry the effects of specification error in any part of the system to the estimate of any other part. Since it is clear that some equations may be thought to be better specified than others as the quality of economic information is by no means constant in an economy-wide model, this is a highly undesirable feature. It seems clear that specification error should be quarantined and hence that limited-information estimators which are known to accomplish this are preferable to full-information ones in large models ([21]). While it may be desirable to use intermediate estimators which apply full-information type methods to sectors rather than to the system as a whole, the theory of how this should be done remains to be worked out.

4. Limited-information estimators

4.1. *Availability in Practice*

While a great many limited-information estimators have been suggested, in practice, none of them are available for use in their original form in estimating an economy-wide econometric model. This is the case because all such estimators begin in one way or another with the ordinary least squares estimates of the reduced form equations. As we have already seen, such estimates are likely to be difficult or impossible to secure in all but the most aggregate models because of the low number of

([21]) Fisher [8, p. 155].

observations generally available relative to the number of predetermined variables, if all lagged endogenous variables are treated as predetermined.

In addition, if lagged endogenous variables are so treated, then we have already seen that such treatment raises considerable difficulties in the likely presence of serial correlation of the disturbances. On the other hand, economy-wide models are generally sufficiently closed to make their equations unidentifiable if only truly exogenous variables are treated as instruments.

We shall discuss the problems raised by serial correlation in the next section which will be concerned with the question of how instrumental variables should be chosen in practice to avoid inconsistency. In the present section, we shall discuss the properties of the limited-information estimators ignoring these problems. Such a discussion is not rendered irrelevant by the practical difficulty of using all variables that are not current endogenous ones as instruments when the number of observations is relatively limited. This is so because given the variables which are to be treated as predetermined, treatment of all remaining variables as endogenous results in a situation in which every known limited-information estimator has its precise counterpart ([22]). Thus, for example, if only certain lagged endogenous and exogenous variables are to be used, replacing every other (save the normalized one) in a given equation by its value as computed from a multiple regression on the instruments and then regressing the normalized variable on the resulting variables provides the exact analogue of two-stage least squares.

Of course, in such a situation, and especially where serial

([22]) It must be admitted, however, that if different predetermined variables are used in replacing each included endogenous variable (as suggested below), then it is not clear how limited-information maximum likelihood carries over to such cases. Fortunately, this will not matter for our purposes.

correlation in the disturbances cannot be presumed absent, the choice of instrumental variables is likely to be of considerably greater importance than the choice of the particular limited-information method in which such instruments are to be applied. The latter choice is clearly worth discussing, however, although the major portion of our discussion will be reserved for the former one which will be taken up in the two following sections.

4.2. *Classification and Large Sample Properties under Ideal Conditions*

The limited-information estimators in common use are those of THEIL's k-class. Chief among these are two-stage least squares, limited-information maximum likelihood, and an estimator due to NAGAR (23). Another class of estimators, the h-class, has also been suggested by THEIL, and NAGAR has recently proposed still a third class, the double k-class (24). For our purposes such subdivisions will not be particularly important. What will be important is the fundamental distinction between limited-information maximum likelihood and all other proposed limited-information estimators. Alone among suggested members of the k-, h-, and double k-classes, the fundamental distinguishing parameter (k in this case) is stochastic in limited-information maximum likelihood, being determined as a root of a stochastic determinantal equation. As we shall see below, this distinction aside from making limited-information maximum likelihood somewhat cumbersome to compute leads to a lack of robustness in that estimator in the presence of multicollinearity. Such a lack is not shared by the other estimators of the limited-information class.

(23) THEIL [32, pp. 231-232]; NAGAR [22].
(24) THEIL [32, pp. 353-354]; NAGAR [23].

Indeed, when one looks only at the properties of limited-information estimators under ideal conditions, there are relatively few grounds for choice among them. In the next subsection we shall consider the little which is known of their small sample properties, here we merely observe that they all have essentially the same large sample properties. It can be argued that limited-information maximum likelihood has the desirable property of treating all included endogenous variables in an equation symmetrically; indeed, CHOW has shown that it is a natural generalization of ordinary least squares in the absence of a theoretically given normalization rule ([25]).

On the other hand, such an argument seems rather weak since normalization rules are in fact generally present in practice, each equation of the model being naturally associated with that particular endogenous variable which is determined by the decision-makers whose behavior is represented by the equation. The normalization rules are in a real sense part of the specification of the model, and the model is not completely specified unless every endogenous variable appears (at least implicitly) in exactly one equation in normalized form. For example, it is not enough to have price equating supply and demand, equations should also be present which explain price quotations by sellers and buyers and which describe the equilibrating process. (For most purposes, of course, such additional equation can remain in the back of the model builders' mind, although the rules for choosing instrumental variables given below may sometimes require that they be made explicit.)

Thus, symmetry may be positively undesirable in a well-specified model where one feels relatively certain as to appropriate normalization, although it may be desirable if one wishes to remain agnostic as to appropriate normalization. So far as arguments of this type or from large sample properties under

([25]) CHOW [7].

ideal conditions are concerned, then, there seems little or no reason for preferring one limited-information estimator to another.

4.3. *Small Sample Properties*

The situation is not very different at the present time when one considers small sample properties. To date, relatively little is known about these and work has proceeded largely by means of Monte Carlo experiments. Moreover, all such experiments and such analytic work as is available have been exclusively concerned with the case in which lagged endogenous variables do not appear (or at least are not used as predetermined instruments) and the analytic work has dealt only with those members of the k-class with non-stochastic k. In the present context, the former limitation is a severe one. Nevertheless, it seems worth briefly discussing what is known about small sample properties in such cases as the situation when lagged endogenous variables are present is probably no more hopeful.

The principal point that has emerged on small sample properties of limited-information estimators is that the sampling variances involved are infinite in some cases. Such a conclusion is borne out both from the analytic work that has been accomplished to date and by the results of the Monte Carlo experiments that have been performed ([26]). It seems idle to hope that this circumstance does not occur when lagged endogenous variables are present in the model.

In practice, this unhappy circumstance has a number of consequences. First, it is clearly the case that relatively little reliance can be placed on judgments of goodness of fit derived from consideration of asymptotic standard errors. Such asymp-

([26]) See BASMANN [4], [5], BERGSTROM [6], NAGAR [22], and SARGAN [28] for the analysis. JOHNSTON [15, pp. 275-295] summarizes most of the Monte Carlo experiments; see also QUANDT [25], [26].

totic standard errors are obviously inappropriate when directly taken as approximations to an infinite sample variance and may or may not be reliable when used to derive approximations to the probability that an estimate diverges from the true parameter by more than a given amount. In general, the latter approximation is probably better for small divergences than for large ones as the normal approximation to the small sample distribution is almost certainly worst in the tails ([27]).

Second, as already indicated, the absence of this property in ordinary least squares makes the latter estimator rather more attractive than would be the case if limited-information estimators always had finite variance. Certainly, there is a certain amount of justification for using ordinary least squares as an approximation while building the model provided that assumptions (R.1)-(R.3) are not too badly violated (which we have argued cannot be assumed in economy-wide models). Further, QUANDT has recently suggested combining ordinary least squares and limited-information estimators to take advantage of the fact that the latter are consistent while the former has a finite variance ([28]).

Furthermore, the infinite small sample variance of limited-information estimators casts doubt on the convergence in some cases of the expansions used by NAGAR to demonstrate the unbiasedness of his suggested estimator to order $1/T$, where T is the sample size ([29]). When such expansions do converge, such unbiasedness is about the only known sample property in which one limited-information estimator is demonstrably superior to the others. As it happens, however, NAGAR's demonstration assumes that there are no lagged endogenous variables in the model so that, even aside from the convergence problem just mentioned, his results are not applicable in the present case.

([27]) See BASMANN [5]. SARGAN [28] derives approximate expressions for the probabilities just described.

([28]) QUANDT [25].

([29]) NAGAR [22]. See SARGAN [28].

4.4. *Robustness*

Thus neither large nor small sample properties under ideal conditions provide much guide for the choice of an estimator from the limited-information class in the present state of knowledge. This is not entirely the case as regards the robustness of such estimators, however. KLEIN and NAKAMURA have shown that as a consequence of the stochastic nature of k in limited-information maximum likelihood, that estimator is more sensitive to multicollinearity than are the other members of the k-class ([30]). *In the absence of other criteria,* these seem grounds for abandoning limited-information maximum likelihood in practice in favor of some other limited-information estimator.

There seem to be no very strong reasons, however, for choosing among the limited-information estimators other than maximum likelihood. The paper on robustness just mentioned indicates that these do not differ among themselves as regards this property ([31]). Since two-stage least squares is the easiest of these estimators to compute and since it does provide a natural generalization of ordinary least squares in the presence of theoretically given normalization rules ([32]), it seems natural to choose it in the present state of our knowledge.

5. NEAR-CONSISTENCY, BLOCK-RECURSIVE SYSTEMS, AND THE CHOICE OF ELIGIBLE INSTRUMENTAL VARIABLES

5.1. *Introduction*

In this section we begin the discussion of the choices of predetermined instrumental variables which are available and

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([30]) KLEIN and NAKAMURA [16].

([31]) See also FISHER [8] for proof that the same is true as regards sensitivity to specification error.

([32]) See CHOW [7].

the circumstances under which such choices are likely to be appropriate. Most of our discussion will be in terms of the assumptions that must be approximately satisfied if a given variable is to be eligible for inclusion as an instrument. We thus postpone to the next section the important question of how the eligible candidates ought in fact to be used. Until further notice, then, we discuss only whether and under what circumstances a given single variable ought to be treated as predetermined.

In general, we desire two things of a variable which is to be treated as predetermined in the estimation of a given equation. First, it should be uncorrelated in the probability limit with the disturbance from that equation; second, it should closely causally influence the variables which appear in that equation and should do so independently of the other predetermined variables (33). If the first criterion is not satisfied, treating the variable as predetermined results in inconsistency; if the second fails, such treatment does not aid much in estimation — it does not reduce variances. In practice, these requirements may frequently not be consistent and one has to compromise between them. The closer the causal connection the higher may be the forbidden correlation. Thus, in one limit, the use of ordinary least squares which treats all variables on the right-hand side of the equation as predetermined perfectly satisfies the second but not the first criterion. In the other limit, the use of instrumental variables which do not directly or indirectly causally influence any variable in the model perfectly meets the first requirement but not the second (34). In general, one is frequently faced with the necessity of weakening the first requirement to one of low rather than of zero correlation and

(33) It should therefore be relatively uncorrelated with the other variables used as instruments so that lack of collinearity is not really a separate criterion. We shall return to this in the next section.

(34) If only such variables are available for use (or if an insufficient number of more interesting ones are) then the equation in question is underidentified and even asymptotic variances are infinite.

accepting indirect rather than direct causal relations between instruments and included variables. (Such a compromise may result in different instruments for different equations when a limited-information estimator is used; this will be the case below).

In the present section, we discuss the circumstances under which zero or low inconsistencies can be expected, leaving explicit use of the causal criterion to the next section.

Now, two sets of candidates for treatment as instrumental variables are obviously present. The first of these consists of those variables which one is willing to assume truly exogenous to the entire system and the lagged values thereof; the second consists of the lagged endogenous variables. The dynamic and causal structure of the system may well provide a third set, however, and may cast light on the appropriateness of the use of lagged endogenous variables; to a discussion of this we now turn.

5.2. *The Theory of Block-Recursive Systems*

A generalization of the recursive systems already discussed is provided by what I have elsewhere termed « block-recursive systems » ([35]). In general, such systems have similar properties to those of recursive systems when the model is thought of as subdivided into sets of current endogenous variables and corresponding equations (which we shall call sectors) rather than into single endogenous variables and their corresponding equations.

Formally, we ask whether it is possible to partition the vectors of variables and of disturbances and the corresponding matrices (renumbering variables and equations, if necessary) to secure a system with certain properties. In such partition-

([35]) See FISHER [8].

ings, the Ith subvector of a given vector x will be denoted as x^{I}. Similarly, the submatrix of a given matrix M which occurs in the Ith row and Jth column of submatrices of that matrix will be denoted by M^{IJ}. Thus:

$$(5.1) \qquad M = \begin{bmatrix} M^{11} & M^{12} & \dots & M^{1N} \\ M^{21} & M^{22} & \dots & M^{2N} \\ \vdots & \vdots & & \vdots \\ M^{N1} & M^{N2} & \dots & M^{NN} \end{bmatrix} ; x = \begin{bmatrix} x^{1} \\ x^{2} \\ \vdots \\ x^{N} \end{bmatrix}$$

We shall always assume the diagonal blocks, M^{II} to be square.

If when written in this way, the matrix M has the property that $M^{IJ} = 0$ for all $I = 1, \dots, N$ and $J > I$, the matrix will be called *block-triangular*. If $M^{IJ} = 0$ for all $I = 1, \dots, N$ and $J \neq I$, the matrix will be called *block-diagonal* [36].

Now consider the system (2.1). Suppose that there exists a partition of that system (with $N > 1$) such that:

(BR.1) A is block-triangular;
(BR.2) V(0) is block-diagonal;
(BR.3) $V(\theta) = 0$ for all $\theta > 0$.

(Note that these are generalizations of (R.1) - (R.3)). In this case, it is easy to show that the *current* endogenous variables of any given sector are uncorrelated in probability with the current disturbances of any higher-numbered sector. Such variables may thus be consistently treated as predetermined instruments in the estimation of the equations of such higher-numbered sectors.

To establish the proposition in question, observe that (2.2)-(2.4) always hold [37]. By (BR.3), (2.5) holds also, so that:

$$(5.2) \qquad W(0) = DV(0).$$

[36] Block-triangularity and block-diagonality are the respective canonical forms of decomposability and complete decomposability.

[37] Continuing to assume that DB is stable.

By (BR.1), however, $D = (I - A)^{-1}$ is block-triangular, while V(0) is block diagonal by (BR.2). It follows that their product is block triangular with the same partitioning. Thus:

(5.3) $W(0)^{IJ} = 0$ for all I, J = 1, ..., N and J>I,

but this is equivalent to the proposition in question.

As in the special case of recursive systems, assumption (BR.3) can be replaced by a somewhat different assumption:

(BR.3*) B is block-triangular with the same partitioning as A, as is V(θ) for all θ>0. Further, either all B^{II} or all V(θ) (θ>0) are zero (I = 1, ..., N).

To see that this suffices, observe that in this case every term in (2.4) will be block-triangular.

Note, however, that whereas (BR.1)-(BR.3) patently suffice to give W(1) = 0 and thus to show that lagged endogenous variables are uncorrelated with current disturbances, this is not the case when (BR.3) is replaced by (BR.3*). As in the similar case for recursive systems, what is implied by (BR.1), (BR.2), and (BR.3*) in this regard is that W(1) is also block-triangular with zero matrices on the principal diagonal so that lagged endogenous variables are uncorrelated with the current disturbances of the same or *higher*-numbered blocks, but not necessarily with those of *lower*-numbered ones.

If A and B are both block-triangular with the same partitioning, then the matrix DB is also block-triangular and the system of difference equations given by (2.2) is decomposable. In this case, what occurs in higher-numbered sectors *never* influences what occurs in lower-numbered ones, so that there is in any case no point in using current or lagged endogenous variables as instruments in lower-numbered sectors. This is an unlikely circumstance to encounter in an economy-wide model in any essential way, but it may occur for partitionings which split off a small group of equations from the rest of the model. If it does not occur, then (BR.3) is generally necessary for the block-triangularity of W(0). Indeed unless either (BR.3) or the first statement of (BR.3*) holds, *no* $W(0)^{ij}$ can generally be expected to be zero if $B \neq 0$.

To see this, observe that (2.4) implies that W(0) cannot generally be expected to have any zero submatrices unless every term in the sum which is not wholly zero has a zero submatrix in the same place. This cannot happen unless every matrix involved is either block-diagonal or block-triangular. Hence, if $V(\theta) \neq 0$ for all $\theta > 0$, all such $V(\theta)$ must at least be block-triangular as must B ([38]).

5.3. *Block-Recursive Assumptions in Economy-wide Models*

Unfortunately, while block-triangularity of A is not an unreasonable circumstance to expect to encounter in practice ([39]) the assumptions on the disturbances involved in (BR.2) and (BR.3) or (BR.3*) seem rather unrealistic in economy-wide models for much the same reasons as did the parallel assumptions of the recursive model. Thus, it does not seem reasonable to assume that the omitted effects which form the disturbances in two different sectors have no common elements; nor, as already discussed, does it seem plausible to assume either that there is no serial correlation of disturbances or that the dynamic system involved is decomposable.

Note, however, that these assumptions may be better approximations than in the case of recursive systems. Thus one

([38]) Of course, this does not show that (BR.3) or (BR.3*) is necessary, since counter-examples may easily be produced in which different non-zero terms in (2.4) just cancel out. The point is that this cannot be assumed to occur in practice. To put it another way, since such cancellation cannot be known to occur, it clearly occurs only on a set of measure zero in the parameter space. Thus (BR.3) or (BR.3*) is necessary with probability 1.

([39]) It is encountered in preliminary versions of the Brookings-SSRC model. C. Holt and D. Steward have developed a computer program for organizing a model in block-triangular form.

may be more willing to assume no correlation between contemporaneous disturbances in two different aggregate sectors than between disturbances in any two single equations. A similar assumption may be even more attractive when the disturbances in question are from different time periods as will be seen below. Thus also, the dynamic system may be thought *close* to decomposability when broad sectors are in view and feedbacks within sectors explicitly allowed. If such assumptions are approximately satisfied, then the inconsistencies involved in the use of current and lagged endogenous variables as predetermined in higher-numbered sectors will be small ([40]).

Nevertheless, the assumption of no correlation between contemporaneous disturbances from different sectors, the assumption of no serial correlation in the disturbances, and the assumption of decomposability of the dynamic system all seem rather strong ones to make. If none of these assumptions is in fact even approximately made, then the use of current endogenous variables as instruments in higher-numbered sectors leads to non-negligible inconsistencies. We shall show, however, that this need not be true of the use of *lagged* endogenous variables in higher-numbered sectors under fairly plausible assumptions as to the process generating the disturbances. We thus turn to the question of the use of lagged endogenous variables, assuming that A is known to be at least nearly block-triangular.

5.4. *Reasonable Properties of the Disturbances*

The problems which we have been discussing largely turn on the presence of common omitted variables in different equations and on the serial correlation properties of the disturbances. It seems appropriate to proceed by setting up an explicit model

(([40]) See Fisher [8]. The theorems involved are generalizations of the Proximity Theorem for recursive systems.

of the process generating the disturbances in terms of such omitted variables and such serial correlation.

We shall assume that the disturbances to any equation are made up of three sets of effects. The first of these will consist of the effects of elements common to more than one sector — in general, common to all sectors. The second will consist of the effects of elements common to more than one equation in the sector in which the given equation occurs. The third will consist of effects specific to the given equation.

Thus, let the number of equations in the Ith sector be n_I. We write:

$$u_t^I = \varphi^I e_t + \Psi^I v_t^I + w_t^I \qquad (I = 1, \ldots, N) \tag{5.4}$$

where

$$e_t = \begin{bmatrix} e_{1t} \\ \vdots \\ e_{Kt} \end{bmatrix} \tag{5.5}$$

is a vector of implicit disturbances whose effects are common (in principle) to all equations in the model and φ^I is an $n_I \times K$ constant matrix;

$$v_t^I = \begin{bmatrix} v_{1t}^I \\ \vdots \\ v_{H_I t}^I \end{bmatrix} \tag{5.6}$$

is a vector of implicit disturbances whose effects are common (in principle) to all equations in the Ith sector but not to equations in other sectors; Ψ^I is an $n_I \times H_I$ constant matrix; and

$$w_t^I = \begin{bmatrix} w_{1t}^I \\ \vdots \\ w_{n_I t}^I \end{bmatrix} \tag{5.7}$$

is a vector of implicit disturbances the effect of each of which is specific to a given equation in the Ith sector.

Define

(5.8)
$$\varphi = \begin{bmatrix} \varphi \mathrm{I} \\ \vdots \\ \varphi^{N} \end{bmatrix} \quad ;$$

(5.9)
$$v_t = \begin{bmatrix} v_t^1 \\ \vdots \\ v_t^N \end{bmatrix} \quad ;$$

(5.10)
$$\Psi = \begin{bmatrix} \Psi^1 & 0 & \dots & 0 \\ 0 & \Psi^2 & \dots & 0 \\ \vdots & \vdots & & \vdots \\ 0 & 0 & & \Psi^N \end{bmatrix} \quad ;$$

and

(5.11)
$$w_t = \begin{bmatrix} w_t^1 \\ w_t^2 \\ \vdots \\ w_t^N \end{bmatrix} \quad .$$

Then (5.4) may be rewritten more compactly as:

(5.12)
$$u_t = \varphi e_t + \Psi v_t + w_t \; .$$

We shall refer to the elements of e_t, v_t, and w_t as *economy-wide*, *sector*, and *equation* implicit disturbances, respectively, noting that whether an economy-wide or sector implicit disturbance actually affects a given equation depends on the relevant rows of φ and Ψ', respectively. (The unqualified term « disturbance » will be reserved for the elements of u_t.)

All elements of e_t, v_t, and w_t are composites of unobservables; it is hardly restrictive to assume:

(A.1) Every element of e_t, v_t, or w_t is uncorrelated in probability with all present or past values of any *other* element of any of these vectors. The vectors can always be redefined to accomplish this.

We shall assume that each element of each of these implicit disturbance vectors obeys a (different) first-order auto-regressive scheme ([41]). Thus:

$$e_t = \Lambda_e \; e_{t-1} + e_t^*, \tag{5.13}$$

$$v_t = \Lambda_v \; v_{t-1} + v_t^*, \tag{5.14}$$

$$w_t = \Lambda_w \; w_{t-1} + w_t^*, \tag{5.15}$$

where Λ_e, Λ_v, and Λ_w are diagonal matrices of appropriate dimension and e_t^*, v_t^*, and w_t^* are vectors of non-auto-correlated random variables. Assuming that the variance of each element of e_t, v_t, and w_t is constant through time, the diagonal elements of Λ_e, Λ_v, and Λ_w are first-order auto-correlation coefficients and are thus each less than one in absolute value.

Now let Δ_e, Δ_v, and Δ_w be the diagonal variance-covariance matrices of the elements of e_t, v_t, and w_t, respectively. In view of (A.1) and (5.13)-(5.15) it is easy to show that (5.12) implies:

$$V(\theta) = \varphi \Lambda_e^{\theta} \Delta_e \varphi' + \Psi \Lambda_v^{\theta} \Delta_v \Psi' + \Lambda_w^{\theta} \Delta_w \qquad (\theta \geq 0) \quad . \tag{5.16}$$

([41]) Auto-regressive relations of higher orders could be considered in principle, but this would rather complicate the analysis. We shall thus assume that first-order relationships are sufficiently good approximations. If higher-order relationships are involved there is no essential change in the qualitative results.

Evidently, $V(\theta)$ will be non-zero unless a number of other assumptions are imposed. Consider, however, the question of whether $V(\theta)$ will be block-diagonal. Since all the Λ and Δ matrices are diagonal, and since Ψ is itself block-diagonal by (5.10), we have:

$$(5.17)\quad V(\theta)^{IJ} = \varphi^{I} \Lambda_{e}^{\theta} \Delta_{e} \varphi^{J\prime} \qquad (\theta \geq 0;\ I, J = 1, \ldots, N;\ J \neq I)\ .$$

Thus the off-diagonal blocks of $V(\theta)$ depend only on the properties of the economy-wide disturbances.

This result is perhaps worth emphasizing. When applied to $\theta = 0$, it merely states formally what we have said previously, that contemporaneous disturbances from the equations of the model which occur in different sectors cannot be assumed uncorrelated if there are common elements in each of them, that is, implicit disturbance elements affecting both sectors. When applied to $\theta > 0$, however, the result is at least slightly less obvious. Here it states that despite the fact that contemporaneous disturbances from different sectors may be highly correlated, and despite the fact that every disturbance may be highly auto-correlated, a given disturbance will *not* be correlated with a lagged disturbance from another sector unless the economy-wide implicit disturbances are themselves auto-correlated. To put it another way, the presence of economy-wide implicit disturbances and the presence of substantial serial correlation do not prevent us from taking $V(\theta)$ as block-diagonal for $\theta > 0$ provided that the serial correlation is entirely confined to the sector and equation implicit disturbances.

Is it then reasonable to assume that the serial correlation is so confined? I think it is reasonable in the context of a carefully constructed economy-wide model. We argued above that any such model inevitably omits variables the effects of which are not confined within sectors. Effects which are highly auto-correlated, however, are effects which are relatively systematic over time. In an inevitably aggregate and approximate economy-wide model, there are likely to be such systematic effects influencing individual equations and even whole sectors. Systematic effects which spread over more than one sector, however, seem substantially less likely to occur, especially when we recall that the limits of a sector in our sense are likely to be rather wide ([42]). Variables which give rise to such effects are not likely to be omitted variables whose influence lies in the disturbance terms. Rather they are likely to be explicitly included in the model, if at all possible. If not, if they relate to the occurrence of a war, for example, and are thus hard to specify explicitly, the time periods in which they are most important are likely to be omitted from the analysis. In short, systematic behavior of the disturbances is an indication of incomplete specification. Such incompleteness is much less likely to occur as regards effects which are widespread than as regards effects which are relatively narrowly confined, especially since the former are less likely to be made up of many small effects ([43]). (Recall that an economy-wide implicit disturbance is one which affects more than one sector *directly*, not simply one whose effects are transmitted through the dynamic causal structure of the explicit model.) It thus does not seem unreasonable to assume that:

$$(5.18) \qquad \Lambda_e = 0$$

and therefore

$$(5.19) \qquad V(\theta)^{IJ} = 0 \qquad (\theta > 0;\ I, J = 1, \ldots, N;\ J \neq I)$$

as good approximations.

([42]) As they are in the Brookings-SSRC model.

([43]) A similar argument obviously implies that sector implicit disturbances are less likely to be serially correlated than are equation implicit disturbances. The analysis of the effects of this on $V(\theta)$ and the subsequent discussion is left to the reader. The assumption of no serial correlation in the sector implicit disturbances seems considerably more dangerous than that being discussed in the text.

5.5. *Implications for the Use of Lagged Endogenous Variables*

Of course, assuming (5.19) to hold is not sufficient to yield consistency when lagged endogenous variables are treated as predetermined. We have already seen that unless $V(\theta)=0$, the decomposability of the dynamic system must be assumed in addition to (5.19) to secure such consistency. We argued above, however, that such decomposability was rather unlikely in an interconnected economy, although the fact that (5.19) is likely to hold approximately makes it important to look for *near*-decomposability and thus secure *near*-consistency [44].

Even if such near-decomposability of the dynamic system does not occur, however, (5.19) has interesting consequences for the treatment of lagged endogenous variables as predetermined. To these we now turn.

Consider the expression for W(1) given in equation (2.6). Writing out the first few terms of the sum, we obtain:

$$(5.20) \qquad W(1)=DV(1)+DBDV(2)+(DB)^2DV(3)\ \dots\ .$$

Since D is block-triangular and V(1) block-diagonal by (5.19), the first term in this expansion is also block-triangular. Hence even if the dynamic system is not decomposable, endogenous variables lagged one period are approximately uncorrelated in the probability limit with disturbances in *higher*-numbered sectors (but not in the same or lower-numbered sectors), to the extent that the right-hand terms in (5.20) other than the first can be ignored.

In what sense is it legitimate, then, to assume that such terms can in fact be ignored? Assume that the matrix DB is

(44) Near-decomposability of a dynamic system has a number of interesting consequences in addition to this. Se ANDO, FISHER, and SIMON [3], especially ANDO and FISHER [2].

similar to a diagonal matrix, so that there exists a non-singular matrix P such that ([45]):

$$(5.21) \qquad DB = PHP^{-1}$$

where H is diagonal and has for diagonal elements the latent roots of DB. Let:

$$(5.22) \qquad \Lambda = \begin{bmatrix} \Lambda_e & 0 & 0 \\ 0 & \Lambda_v & 0 \\ 0 & 0 & \Lambda_w \end{bmatrix}$$

$$(5.23) \qquad \Delta = \begin{bmatrix} \Delta_e & 0 & 0 \\ 0 & \Delta_v & 0 \\ 0 & 0 & \Delta_w \end{bmatrix}$$

$$(5.24) \qquad \underset{\sim}{Q} = [\, \varphi \,\vdots\, \Psi \,\vdots\, I \,]$$

Then every such term can be written as:

$$(5.25) \qquad (DB)^{\theta-1} DV(\theta) = PH^{\theta-1} P^{-1} D\underset{\sim}{Q}\Lambda^{\theta}\Delta\underset{\sim}{Q}' \qquad (\theta > 1) \; .$$

We know that every diagonal element of the diagonal matrix Λ is less than unity in absolute value (indeed, we are assuming that some of the diagonal elements are zero). Moreover, *if we are prepared to maintain the stability assumption on* DB which was slipped in some time ago, every diagonal element of the diagonal matrix H will also be less than unity in absolute value. It follows that every element of every term in the expansion of W(1) other than the first is composed of a sum of terms each of which involves at least the product of

([45]) The assumption involved is, of course, very weak and is made for ease of exposition.

a factor less than unity and the square of another such factor. There is clearly a reasonable sense in which one may be prepared to take such terms as negligible at least when compared with the non-zero elements of the first term in the expansion for W(1) which involve only the diagonal elements of Λ to the first power. If one is willing to do this, then one is saying that the use of endogenous variables lagged one period as instruments in higher-numbered sectors involves only negligible inconsistency at least as compared with the use of the same variables as instruments in their own or lower-numbered sectors.

There may be considerable difficulties in accepting such a judgment, however. In the first place, it is well to be aware that there are two different statements involved. It is one thing to say that the effects in question are negligible compared to others and quite another to say that they are negligible in a more absolute sense. If one accepts the stability assumption, then there certainly is a value of θ beyond which further terms in the expansion of W(1) are negligible on any given standard. These may not be all terms after the first, however; we shall discuss the case in which there are non-negligible terms after the first below.

Second (a minor point but one worth observing) even our conclusion about *relative* importance *need* not hold although other assumptions are granted. While it is true that as θ becomes large the right-hand side of (5.25) approaches zero, such approach need not be monotonic. To put it another way, every element of the matrix involved is a sum of terms. Each such term involves a diagonal element of Λ to the θ and a diagonal element of H to the $\theta - 1$. If all such diagonal elements are less than unity in absolute value, then the absolute value of each separate term approaches zero monotonically as θ increases; this need not be true of the *sum* of those terms, however, and it is easy to construct counter-examples. Nevertheless, there is a sense in which it seems appropriate to assume the terms in the expansion for W(1) to be negligible for θ greater than some value, perhaps for $\theta > 1$.

All this, however, has leaned a bilt heavily on the stability of DB. If that matrix has a latent root greater than unity in absolute value, then part of the reason for assuming that the right-hand side of (5.25) is negligible even for high values of θ has disappeared. Of course, it is the case that the diagonal elements of Λ are known to be less than unity in absolute value, so that the infinite sum involved in W(1) may still converge. However, such convergence is likely to be slow in an unstable case and may not occur at all, so that the effects of serial correlation are even more serious than in the stable case. Clearly, the stability assumption requires additional discussion at this point.

The usual reason for assuming stability of the dynamic model being estimated is one of convenience or of lack of knowledge of other cases. Since the unstable case tends to lead to unbounded moment matrices, the usual proofs of consistency of the limited-information estimators tend to break down in that circumstance. Indeed, maximum-likelihood estimators are presently known to be consistent only in the stable case and in rather special unstable cases ([46]). It is therefore customary to assume stability in discussions of this sort. For present purposes, even if limited-information estimators are consistent in unstable cases and even if the Generalized Proximity Theorems which guarantee small inconsistencies for sufficiently good approximations also hold ([47]), the approximations which we are now discussing are relatively unlikely to be good ones in such cases. Even if the existence of W(1) is secured by assuming that the dynamic process (2.1) begins with non-stochastic initial conditions at some finite time in the past (and even

([46]) For example, if *all* latent roots are greater than unity in absolute value. See ANDERSON [1]. J. D. SARGAN has privately informed me that he has constructed a proof of consistency for the general case. The classic paper in this area is that of MANN and WALD [21].

([47]) See FISHER [8].

this does not suffice for the existence of the probability limit), the effects of serial correlation will not die out (or will die out only slowly) as we consider longer and longer lags. The conclusion seems inescapable that if the model is thought to be unstable (and the more so, the more unstable it is), the use of lagged endogenous variables as instruments *anywhere* in an indecomposable dynamic system with serially correlated disturbances is likely to lead to large inconsistencies at least for all but very high lags. The lower is serial correlation and the closer the model to stability, the less dangerous is such use.

Is the stability assumption a realistic one for economy-wide models, then? I think it is. Remember that what is at issue is not the ability of the economy to grow, but its ability to grow (or to have explosive cycles) with no help from the exogenous variables and no impulses from the random disturbances. Since the exogenous variables generally include population growth and since technological change is generally either treated as a disturbance or as an effect which is exogenous in some way, this is by no means a hard assumption to accept. While there are growth and cycle models in economic theory which involve explosive systems, such models generally bound the explosive oscillations or growth by ceilings or floors which would be constant if the exogenous sources of growth were constant [48]. The system *as a whole* in such models is not unstable in the presence of constant exogenous variables and the absence of random shocks [49]. We shall thus continue to make the stability assumption.

Even when the stability assumption is made, however, it may not be the case, as we have seen, that one is willing to take the expression in (5.25) as negligible for all $\theta > 1$. (In particular, this will be the case if serial correlation is thought to be very high so that the diagonal elements of Λ are close

(48) See, for example, HICKS [13] and HARROD [12].
(49) Whether a linear model is a good approximation if such models are realistic is another matter.

to unity in absolute value). In such cases, one will not be willing to assume that the use of endogenous variables lagged *one* period as instruments in higher-numbered sectors leads to only negligible inconsistencies. Accordingly, we must generalize our discussion.

Fortunately, this is easy to do. There clearly does exist a smallest $\theta^* > 0$ such that for all $\theta > \theta^*$ even the diagonal blocks of $V(\theta^*)$ are negligible on any given standard. Consider $W(\theta^*)$, the covariance matrix of the elements of u_t and those of $y_{t-\theta^*}$ with the columns corresponding to elements of u_t and the rows to elements of $y_{t-\theta^*}$. Clearly,

$$(5.26) \qquad W(\theta^*) = DV(\theta^*) + \sum_{\theta=\theta^*+1}^{\infty} (DB)^{\theta-\theta^*} DV(\theta)$$

$$= DV(\theta^*) + \sum_{\theta=\theta^*+1}^{\infty} PH^{\theta-\theta^*} P^{-1} DQ\Lambda^{\theta} Q' \ .$$

Since D is block-triangular and $V(\theta^*)$ block-diagonal by (5.19), the product, $DV(\theta^*)$, is also block-triangular. Considering $W(\theta^*)^{IJ}$ for $J > I$, it is apparent that the covariances of endogenous variables lagged θ^* periods and current disturbances from *higher-numbered sectors* are made up of only negligible terms. Not only is $V(\theta)$ negligible by assumption for $\theta > \theta^*$, but also every such term involves at least one power of H, which, by assumption, is diagonal and has diagonal elements less than unity in absolute value.

Note, however, that a similar statement is clearly false as regards the covariances of endogenous variables lagged θ^* periods and current disturbances *from the same or lower-numbered sectors.* Such covariances involve the non-zero diagonal blocks of $V(\theta^*)$ in an essential way. It follows that the order of inconsistency, so to speak, involved in using endogenous variables lagged a given number of periods as instruments is less if such variables are used in higher-numbered sectors than if they are used in the same or lower-numbered sectors. To put it another way, the minimum lag with which it is reasonably safe to use endogenous variables as instruments is at least one less for use in higher-numbered than for use in the same or lower-numbered sectors.

As a matter of fact, our result is a bit stronger than this. It is apparent from (5.26) that the use of endogenous variables lagged θ^* periods as instruments in higher-numbered sectors involves covariances of the order of $\Lambda^{\theta^*+1}H$. Even the use of endogenous variables lagged θ^*+1 periods as instruments in the same or lower-numbered sectors, however, involves covariances of the order of only Λ^{θ^*+1}. No positive power of H is involved in the first term of the expansion for the latter covariances. Since H is diagonal with diagonal elements less than unity in absolute value, the difference between the minimum lag with which it is safe to use endogenous variables as instruments in the same or lower-numbered sectors and the corresponding lag for use in higher-numbered ones may be even greater than one. This point will be stronger the more stable one believes the dynamic system to be. It arises because the effects of serial correlation in sector and equation implicit disturbances are direct in the case of lagged endogenous variables used in the same or lower-numbered sectors and are passed through a damped dynamic system in the case of lagged endogenous variables used in higher-numbered sectors ([50]).

To sum up: so far as inconsistency is concerned, it is likely to be safer to use endogenous variables with a given lag as instruments in higher-numbered sectors than to use them in the same or lower-numbered sectors. For the latter use, the endogenous variables should be lagged by at least one more period to achieve the same level of consistency ([51]).

([50]) All this is subject to the minor reservation discussed above concerning sums each term of which approaches zero monotonically. In practice, one tends to ignore such reservations in the absence of specific information as to which way they point.

([51]) The reader should be aware of the parallel between this result and the similar result for the use of *current* endogenous variables which emerges when (BR.1)-(BR.3) are assumed. Essentially, we have replaced (BR.2) with (5.19) and have dropped (BR.3).

Now, it may be thought that this result is a rather poor return for all the effort we have put into securing it. While one can certainly conceive of stronger results, the usefulness of the present one should not be underestimated. We remarked at the beginning of this section that one important *desideratum* of an instrumental variable was a close causal connection with the variables appearing in the equation to be estimated. In general, economy-wide (and most other) econometric models have the property that variables with low lags are often (but not always) more closely related to variables to be explained than are variables with high ones. There may therefore be a considerable gain in efficiency in the use of recent rather than relatively remote endogenous variables as instruments, and it is important to know that in certain reasonable contexts this may be done without increasing the likely level of resulting inconsistency. To the discussion of the causal criterion for instrumental variables we now turn.

6. Causality and rules for the use of eligible instrumental variables

6.1. *The Causal Criterion for Instrumental Variables*

We stated above that a good instrumental variable should directly or indirectly causally influence the variables in the equation to be estimated in a way independent of the other instrumental variables and that the more direct is such influence, the better. This statement requires some discussion. So far as the limiting example of an instrument completely unrelated to the variables of the model is concerned, the lesson to be drawn might equally well be that instrumental variables must be correlated in the probability limit with at least one of the included variables. While it is easy to see that *some* causal connection must therefore exist, the question naturally arises of why it must be one in which the instrumental variables cause the included ones. If correlation is all that matters, surely the causal link might be reversed or both variables influenced by a common third one.

This is not the case. Consider first the situation in which the proposed instrumental variable is caused in part by variables included in the model. To the extent that this is the case, no advantage is obtained by using the proposed instrumental variable over using the included variables themselves. Obviously, the included variables are more highly correlated with themselves than with the proposed instrument. Further, correlation with the disturbances will be maintained if the proposed instrument is used. To the extent that the proposed instrument is caused by variables unrelated to the included variables, correlation with the disturbances will go down, but so also will correlation with the included variables.

The situation is similar if the proposed instrumental variable and one or more of the included ones are caused in part by a third variable. In this case, it is obviously more efficient to use that third variable itself as an instrument, and, if this is done, no further advantage attaches to the use of the proposed instrumental variable in addition. (The only exception to this occurs if data on the jointly causing variable are not available. In such a case, the proposed instrument could be used to advantage.)

In general, then, an instrumental variable should be known to cause the included variables in the equation, at least indirectly. The closer is such a causal connection the better. As can easily be seen from our discussion of block-recursive systems, however, in many cases the closer is that connection the greater the danger of inconsistency through high correlations with the relevant disturbances. In such systems, for example, current endogenous variables in low-numbered sectors directly cause current endogenous variables in high-numbered sectors (52) while the same endogenous variables lagged are likely to be safer in terms of inconsistency but are also likely to be more remote causes. The value of the result derived at the end of the last section is that it provides a case in which one set of instrumental variables is likely to dominate another set on both criteria.

6.2. *Available Instruments and Multicollinearity*

There is obviously one set of variables which has optimal properties on several counts. These are the exogenous variables explicity included in the model. Such variables are (by assumption) uncorrelated in the probability limit with the disturbances, they also are in close causal connection to the current variables in any equation; indeed, they *are* some of those variables in some cases (53). In the happy event that such exogenous variables are adequate in number and in the non-singularity of their variance-covariance matrix, and that no lagged endogenous variables appear, there is no need to seek further for instrumental variables to use.

Unfortunately, this is unlikely to be the case in an economy-wide econometric model. Such models tend to be almost self-contained with relatively few truly exogenous variables entering at relatively few places. This is especially the case if government policies obey regular rules, follow signals from the economy, and are therefore partly endogenous for purposes of estimation (54). In estimating any equation, all variables not

(52) On causation in general and in decomposable systems (or our block-recursive systems) in particular, see SIMON [29].

(53) They may not cause all such variables even indirectly if the dynamic system is decomposable. Such cases are automatically treated in the rules given below.

(54) This is to be sharply distinguished from the question of whether governmentally controlled variables can be used as *policy* as opposed to estimation instruments.

used as instruments (except the variable explained by the equation) must be replaced by a linear combination of instruments and the dependent variable regressed on such linear combinations. If the second stage of this procedure is not to involve inversion of a singular matrix, then (counting instrumental variables appearing in the equation) there must be at least as many instruments used as there are parameters to be estimated. Further, the linear combinations employed must not be perfectly correlated. Current exogenous variables are simply not generally sufficient to meet this requirement in economy-wide models. Moreover, they do not cause lagged endogenous variables which are likely to be present in a dynamic system.

Clearly, however, if the system is dynamic it will be possible to use *lagged* exogenous variables as well as current ones. Such use may be especially helpful if lagged endogenous variables are to be treated as endogenous and replaced by linear combinations of instruments which can be taken as causing them in part. Indeed, if lagged endogenous variables *are* to be taken as endogenous, then exclusive use of current exogenous variables as instruments will not satisfy the causal criterion for instrumental variables already discussed. Since we have already seen that lagged endogenous variables should be used as instruments only with caution, it follows that lagged exogenous variables may well provide a welcome addition to the collection of available instruments.

Unfortunately, this also is unlikely to suffice. While it is true that one can always secure a sufficient number of instruments by using exogenous variables with larger and larger lags, such a procedure runs into several difficulties. In the first place, since rather long lags may be required, there may be a serious curtailment of available observations at the beginning of the time period to be used. Second, exogenous variables in the relatively distant past will be relatively indirect causes of even the lagged endogenous variables appearing in the equation to be estimated; it follows that their use will fail the causal criteria given and that it may be better to accept some inconsistency by using endogenous variables with lower lags. Finally, after going only a few periods back, the chances are high in practice that adding an exogenous variable with a still higher lag adds a variable which is very highly correlated with the instruments already included and therefore adds little independent causal information (55). While the use of lagged exogenous variables is therefore highly desirable, it may not be of sufficient practical help to allow the search for instrumental variables to end.

Whatever collection of current exogenous, lagged exogenous, and (none, some, or all) lagged endogenous variables are used, however, the multicollinearity difficulty just encountered tends to arise. Some method must be found for dealing with it.

One set of interesting suggestions in this area has been provided by KLOEK and MENNES (56). Essentially, they propose using principal component analysis in various ways on the set of eligible instruments in order to secure orthogonal linear combinations. The endogenous variables are then replaced by their regressions on these linear combinations (possibly together with the eligible instruments actually appearing in the equation to be estimated), and the dependent variable regressed on these surrogates and the instruments appearing in the equation. Variants of this proposal are also examined.

This suggestion has the clear merit of avoiding multicollinearity, as it is designed to do. However, it may eliminate such multicollinearity in an undesirable way. If multicollinearity is present in a regression equation, at least one of the variables therein is adding little causal information to that already contained in the other variables. In replacing a given endogenous variable with its regression on a set of instruments,

(55) This is especially likely if the exogenous variables are principally ones such as population which are mainly trends.

(56) KLOEK and MENNES [17].

therefore, the prime reason for avoiding multicollinearity is that the addition of an instrument which is collinear with the included ones adds little causal information while using up a degree of freedom. The elimination of such multicollinearity ought thus to proceed in such a way as to conserve causal information. The KLOEK-MENNES proposals may result in orthogonal combinations of instruments which are not particularly closely causally related to the included endogenous variables. Thus such proposals may well be inferior to a procedure which eliminates multicollinearity by eliminating instruments which contribute relatively little to the causal explanation of the endogenous variable to be replaced ([56a]). Clearly, this may involve using different sets of instruments in the replacement of different endogenous variables. Proposals along these lines are given below.

It may be objected, however, that such a procedure may eliminate multicollinearity in the regression of the included endogenous variables (other than the left-hand one of the equation) on the chosen instruments only to encounter it again when the dependent variable is regressed on the replaced variables and the instruments appearing in the equation. This is clearly true ([57]); it is unavoidable, however. The fact that the variables to be replaced by combinations of instruments are all part of the system to be estimated guarantees that they themselves *must* be reasonably highly collinear and related to the included instruments. It is impossible to reduce *that* kind of multicollinearity without introducing as instruments noise elements which are unrelated to the included variables, and such introduction clearly gains nothing. If we can secure instrumental variables which are closely causally related to the included variables but relatively uncorrelated with the disturbance of a

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([56a]) This seems to have been one of the outcomes of experimentation with different forms of principal component analysis in practice. See TAYLOR [30a].

([57]) It is also true of the KLOEK-MENNES procedures.

given equation, we have gone as far as we can. The avoidance of multicollinearity is a necessary part of such a procedure because multicollinearity is one sort of failure of the causal criterion discussed above. To attempt to eliminate that multicollinearity which inevitably results from just those causal relations which are to be estimated, however, is self-defeating ([58]).

6.3. *Rules for the Use of Eligible Instrumental Variables*

We have several times pointed out that the causal criterion and that of no correlation with the given disturbance may be inconsistent and that one may only be able to satisfy one more closely by sacrificing the other to a greater extent. In principle, a fully satisfactory treatment of the use of instrumental variables in economy-wide models would involve a full-scale Bayesian analysis of the losses and gains from any particular action. Such an analysis is clearly beyond the scope of the present paper, although any recommended procedure clearly has some judgment of probable losses behind it, however vague such judgment may be.

We shall proceed by assuming that the no-correlation criterion has been used to secure a set of eligible instrumental variables whose use is judged to involve only tolerable inconsistencies in the estimation of a given equation. Note that the set may be different for different equations. Within that set are current and lagged exogenous variables and lagged endo-

([58]) I want to make it clear that I am not accusing Kloek and Mennes of attempting to do this. Their proposals are designed to eliminate multicollinearity in the first stage of the procedure where it is desirable to do so. Their « Method 2 » [17, pp. 51-52] does eliminate collinearity in the second stage between the replaced endogenous variables and the included predetermined ones, but this is not *necessarily* the same as the desirable and irreducible collinearity among the variables in the equation discussed in the text. It may well occur in practice that the Kloek-Mennes proposals lead to desirable results, although an approach using more structural information than does theirs seems preferable.

genous variables sufficiently far in the past that the effects of serial correlation are judged to be negligible over the time period involved. As shown in the preceding section, that time period will generally be shorter for endogenous variables in sectors lower-numbered than that in which the equation to be estimated appears than for endogenous variables in the same or higher-numbered sectors ([59]). Clearly, other things being equal, the use of current and lagged exogenous variables is preferable to the use of lagged endogenous variables and the use of lagged endogenous variables from lower-numbered sectors is preferable to the use of endogenous variables with the same (or possibly even a slightly greater) lag from the same or higher-numbered sectors than that in which the equation to be estimated occurs. We shall suggest ways of modifying the use of the causal criterion to take account of this. For convenience, we shall refer to all the eligible instrumental variables as predetermined and to all other variables as endogenous.

Consider any particular endogenous variable in the equation to be estimated, other than the one explained by that equation. That right-hand endogenous variable will be termed of *zero causal order*. Consider the *structural* equation (either in its original form or with all variables lagged) that explains that variable ([59a]). The variables other than the explained one appearing therein will be called of *first causal order*. Next, consider the structural equations explaining the *first causal order* endogenous variables ([60]). All variables appearing in those equations will be called of *second causal order* with the

([59]) It will not have escaped the reader's notice that very little guidance has been given as to the determination of the absolute magnitude of that time period.

([59a]) There must exist such an equation if the variable in question is normalized. As stated above, lack of such normalization is a form of incomplete specification.

([60]) Observe that endogenous variables appearing in the equation to be estimated other than the particular one with which we begin may be of positive causal order. This includes the endogenous variable to be explained by the equation to be estimated.

exception of the *zero causal order* variable and those endogenous variables of first causal order the equations for which have already been considered. Note that a given predetermined variable may be of more than one causal order. Take now those structural equations explaining endogenous variables of second causal order. All variables appearing in such equations will be called of *third causal order* except for the *endogenous* ones of lower causal order, and so forth. (Any predetermined variables never reached in this procedure are dropped from the eligible set while dealing with the given zero causal order variable.)

The result of this procedure is to use the *a priori* structural information available to subdivide the set of predetermined variables according to closeness of causal relation to a given endogenous variable in the equation to be estimated. Thus, predetermined variables of first causal order are known to cause that endogenous variable directly; predetermined variables of second causal order are known directly to cause other variables which directly cause the given endogenous variable, and so forth. Note again that a given predetermined variable can be of more than one causal order, so that the subdivision need not result in disjunct sets of predetermined variables.

We now provide a complete ordering of the predetermined variables relative to the given endogenous variable of zero causal order ([61]). Let p be the largest number of different causal orders to which any predetermined variable belongs. To each predetermined variable we assign a p-component vector. The first component of that vector is the lowest-numbered causal order to which the given predetermined variable belongs; the second component is the next lowest causal order to which it belongs, and so forth. Vectors corresponding to variables be-

([61]) I am indebted to J. C. G. Boot for aid in the construction of the following formal description.

longing to less than p different causal orders have infinity in the unused places. Thus, for example, if $p=5$, a predetermined variable of first, second and eighth causal order will be assigned the vector: $(1, 2, 8, \infty, \infty)$. The vectors are now ordered lexicographically. That is, any vector, say f, is assigned a number, $\beta(f)$, such that, for any two vectors, say f and h:

(6.1) $\beta(f)>\beta(h)$ if and only if either $f_1>h_1$ or for some $j(1<j\leq p)$

$$f_i=h_i \ (i=1, \ldots, j-1) \text{ and } f_j>h_j.$$

The predetermined variables are then ordered in ascending order of their corresponding β-numbers. This will be called the β-ordering.

Thus predetermined variables of first causal order are assigned lower numbers than predetermined variables of only higher causal orders; predetermined variables of first and second causal order are assigned lower numbers than predetermined variables of first and only causal orders higher than second (or of no higher causal order), and so forth [61a].

The procedure just described gives an *a priori* preference ordering on the set of instrumental variables relative to a given zero causal order endogenous variable. This ordering is in terms of closeness of causal relation. Alternatively, one may wish to modify that ordering to take further account of the danger of inconsistency. This may be done by deciding that current and lagged exogenous variables of a given causal order are always to be preferred to lagged endogenous variables of no lower causal order and that lagged endogenous variables from sectors with lower numbers than that of the equation to

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(61a) This is only one way of constructing such an ordering. If there is specific *a priori* reason to believe that a given instrument is important in influencing the variable to be replaced (for example, if it is known to enter in several different ways with big coefficients) then it should be given a low number. In the absence of such specific information, the ordering given in the text seems a natural way of organizing the structural information.

be estimated are always to be preferred to endogenous variables with the same lag and causal order from the same or higher-numbered sectors. One might even go further and decide that *all* current and lagged exogenous variables of finite causal order are to be preferred to *any* lagged endogenous variable.

However the preference ordering is decided upon, its existence allows us to use *a posteriori* information to choose a set of instruments for the zero causal order endogenous variable in the way about to be described. Once that set has been chosen, that endogenous variable is replaced by its regression on the instruments in the set and the equation in question estimated by least squares regression of the left-hand endogenous variable on the resulting right-hand variables ([62]).

We use *a posteriori* information in combination with the *a priori* preference ordering in the following manner. Suppose that there are T observations in the sample. Regress the zero causal order endogenous variable on the first T-2 instruments in the preference ordering (a regression with one degree of freedom). Now drop the least preferred of these instruments from the regression. Observe whether the multiple correlation of the regression drops significantly as a result. (The standard here may be the significance level of R^2 or simply its value corrected for degrees of freedom.) If correlation does drop significantly, then the T-2nd instrument contributes significantly to the causation of the zero order endogenous variable even in the presence of all instruments which are *a priori* more closely related to that variable than it is. It should therefore be retained. If correlation does not drop significantly, then the variable in question adds nothing and should be omitted.

Now proceed to the T-3rd instrument. If the T-2nd instrument was retained at the previous step, reintroduce it; if not, leave it out. Observe whether omitting the T-3rd instrument reduces the multiple correlation significantly. If so, retain it,

([62]) An important modification of this procedure is described below.

if not, omit it and proceed to the next lower-numbered instrument.

Continue in this way. At every step, a given instrument is tested to see whether it contributes significantly to multiple correlation in the presence of all instruments which are *a priori* preferred to it and all other instruments which have already passed the test. When all instruments have been so tested, the ones remaining are the ones to be used.

6.4. *Discussion of the Rules*

The point of this procedure (or the variants described below) is to replace the right-hand endogenous variables in the equation to be estimated by their regression-calculated values using instruments which satisfy the causal criterion as well as possible while keeping inconsistency at a tolerable level. Certain features require discussion.

In the first place, multicollinearity at this stage of the proceedings is automatically taken care of in a way consistent with the causal criterion. If some set of instruments is highly collinear, then that member of the set which is least preferred on *a priori* grounds will fail to reduce correlation significantly when it is tested as just described. It will then be omitted and the procedure guarantees that it will be the *least* preferred member of the set which is so treated. If the β-ordering is used, this will be the one most distantly structurally related to the endogenous variable which is to be replaced. Multicollinearity will be tolerated where it should be, namely, where despite its presence each instrument in the collinear set adds significant causal information.

Second, it is evident that the procedure described has the property that no variable will be omitted simply because it is highly correlated with other variables already dropped. If two variables add significantly to correlation when both are present but fail to add anything when introduced separately, then the first one to be tested will not be dropped from the regression, as omitting it in the presence of the other instrument will significantly reduce correlation ([62a]). While it is true that variables may be dropped because of correlation with variables less preferred than the T-2nd, which are never tested, the exclusion of the latter variables seems to be a relatively weak reliance on *a priori* information.

This brings us to the next point. Clearly, it is possible in principle that instruments less preferred than the T-2nd would in fact pass the correlation test described if that test were performed after some lower-numbered instruments were tested and dropped. Similarly, an instrument dropped at an early stage might pass the test in the absence of variables *later* dropped because of the increased number of degrees of freedom. One could, of course, repeat the entire procedure in order to test every previously dropped variable after each decision to omit; it seems preferable, however, to rely on the *a priori* preference ordering in practice and to insist that instruments which come late in the β-ordering pass a more stringent empirical test than those which come early. The rationale behind the β-ordering is the belief that it is the earlier instruments in that ordering which contribute most of the causal information, so that it seems quite appropriate to calculate the degrees of freedom for testing a given instrument by subtracting the number of its place in the ordering from the total number of observations (and allowing for the constant term) ([62b]).

Turning to another issue, it may be objected that there is no guarantee that the suggested procedures will result in a non-singular moment matrix to be inverted at the last stage. That is,

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([62a]) This property was missing in the procedure suggested in the first draft of this paper in which variables were added in ascending order of preference and retained if they added significantly to correlation. I am indebted to ALBERT ANDO for helpful discussions on this point.

([62b]) Admittedly, this argument loses some of its force when applied to the modifications of the β-ordering given above.

there may be some set of r endogenous variables to be replaced whose regressions together involve less than r predetermined variables. Alternatively, counting the instruments included in the equation to be estimated there may not be as many instruments used in the final stage as there are parameters to be estimated. This can happen, of course, although it is perhaps relatively unlikely. If it does occur, then it is a sign that the equation in question is unidentifiable from the sample available, that the causal information contained in the sample is insufficient to allow estimation of the equation without relaxing the inconsistency requirements. To put it another way, it can be argued that to rectify this situation by the introduction in the first-stage regressions of variables failing the causal test as described is an *ad hoc* device which adds no causal information. While such variables may in fact appear in such regressions with non-zero coefficients in the probability limit, their use in the sample adds nothing to the quality of the estimates save the ability to secure numbers and disguise the problem.

Of course, such an argument is a bit too strong. Whether a variable adds significantly to correlation is a function of what one means by significance. The problem is thus a continuous rather than a discrete one and should be treated as such. For the criterion of significance used, in some sense, the equation in question cannot be estimated from the sample in the circumstance described; it may be estimatable with a less stringent significance criterion. In practice, if the significance requirements are relaxed, the moment matrix to be inverted will pass from singularity to near-singularity and estimated asymptotic standard errors will be large rather than infinite. The general point is that if multicollinearity cannot be sufficiently eliminated using causal information, little is to be gained by eliminating it by introducing more or less irrelevant variables.

A somewhat related point is that the use of different variables as instruments in the regressions for different endogenous variables in the same equation may result in a situation in which the longest lag involved in one such regression is greater than that involved in others. If data are only available from an initial date, this means that using the regressions as estimated involves eliminating some observations at the beginning of the period that would be retained if the longest-lagged instrument were dropped. In this case some balance must be struck between the gain in efficiency from extra observations and the loss from disregarding causal information if the lagged instrument in question is dropped. It is hard to give a precise guide as to how this should be done. (My personal preference would be for retaining the instrument in most cases.) Such circumstances will fortunately be relatively infrequent as the periods of data collection generally begin further back than those of estimation, at least in models of developed economies. Further, the reduction in available observations attendant on the use of an instrument with a large lag renders it unlikely that the introduction of that instrument adds significantly to correlation.

Finally, the use of different instruments in the regressions replacing different endogenous variables in the equation to be estimated reintroduces the problem of inconsistency. When the equation to be estimated is rewritten with calculated values replacing some or all of the variables, the residual term includes not only the original structural disturbance but also a linear combination of the residuals from the regression equations used in such replacement. When the equation is then estimated by regressing the left-hand variable on the calculated right-hand ones and the instruments explicitly appearing, consistency requires not only zero correlation in the probability limit between the original disturbance and all the variables used in the final regression but also zero correlation in the probability limit between the residuals from the earlier-stage regression equations and all such variables. If the same set of instruments is used when replacing every right-hand endogenous variable and if that set includes the instruments explicitly in the equation, the latter requirement presents no problem since the normal equations of ordinary least squares imply that such correlations are zero even in the sample ([63]). When different instruments are used in the replacement of different variables, however, or when the instruments so used do not include those explicitly in the equation, the danger of inconsistency from this source does arise.

There are several ways of handling this without sacrificing the major benefits of our procedures. One way is simply to argue that those procedures are designed to include in the regression for any right-hand endogenous variable any instrument which is correlated with the residuals from that regression computed without that instrument. The excluded instruments are either those which are known *a priori* not to be direct or indirect causes of the variable to be replaced or those which fail to add significantly to the correlation of the regression in question. The former instruments are known *a priori* not to appear in equations explaining the variable to be replaced and hence cannot be correlated in the probability limit with the residual from the regression unless both they and the replaced variable are affected by some third variable not included in that regression ([64]). Such a third variable cannot be endogenous, however, since in that case the excluded instruments in question would also be endogenous; moreover, our procedure is designed to include explicitly any instrument significantly affecting the variable to be replaced. Any such third variable must therefore be one omitted from the model and it *may* not be stretching things too far to disregard correlations between residuals and excluded instruments stemming from such a source.

([63]) This is the case when the reduced form equations are used, for example, as in the classic version of two-stage least squares.

([64]) If they were non-negligibly caused by the replaced variable itself they would be endogenous, contrary to assumption.

As for instruments which are indirectly causally related to the endogenous variable involved but which fail to add significantly to the correlation of the regression in question, these cannot be significantly correlated with the *sample* residual from that regression. One can therefore argue that the evidence is against their being significantly correlated with that residual in the probability limit.

Such an argument can clearly be pushed too far, however. If there are strong *a priori* reasons to believe that the excluded instruments should be included in view of the causal structure of the model, one may not want to reject correlation in the probability limit because multicollinearity (for the long continuance of which there may be no structural reason) leads to insignificant correlation in the sample. A modified course of action, then, is to include in the regression for any replaced variable any instrument which one believes *a priori* to be important in that regression *and* which appears either in the equation to be estimated or in the regression for any other replaced variable as computed by the procedures described above ([65]). Clearly, not much is lost by doing this since the added variables will not contribute much to the equation in the sample.

Alternatively, one may go the whole way towards guarding against inconsistency from the sonrce under discussion and include in the regression for any replaced variable all instruments which appear in the equation to be estimated or in the regression for any other replaced variable as computed by the described procedures whether or not such instrument is thought *a priori* to be important in explaining the replaced variable. This alternative clearly eliminates the danger under discussion. It may, however, reintroduce multicollinearity and may involve a serious departure from the causal criterion if *a priori* non-causal instruments are thus included. Nevertheless, it does

([65]) Omitting instruments which do *not* so appear does not cause inconsistency.

retain the merit that every instrumental variable used is either explicitly included in the equation or contributes significantly to the causal explanation of at least one variable so included. In practice, there may not be a great deal of difference between these alternatives and the last one described may then be optimal (unless it is unavailable because of the degrees of freedom required).

Whatever variant of our procedures is thought best in practice, they all have the merit of using information on the dynamic and causal structure of the model in securing estimates. The use of such information in some way is vital in the estimation of economy-wide econometric models where the ideal conditions for which most estimators are designed are unlikely to be encountered in practice [66].

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(66) The use of the causal structure of the model itself to choose instrumental variables as described in the text is closely akin to the methods used by BARGER and KLEIN to estimate a system with a triangular matrix of coefficients of current endogenous variables. See [3a].

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DISCUSSION

WOLD

Prof. FISHER has given us an excellent survey of structural properties and estimation techniques for the dynamic econometric models known as interdependent (ID) systems and causal chain (CC) systems. Having over many years emphasized the clearcut rationale of CC systems, also known as recursive systems, I appreciate very much the fair treatment he has given this approach. His review of the ID approach is a very clarifying exposition of the many estimation techniques that have been proposed for ID-systems, a multitude of techniques that are known to work and give reconcilable results when applied to small systems, whereas the situation is far from clear when it comes to large systems with many unknown parameters. The very pluralism of the methods is an indication that the problem of parameter estimation in large ID-systems has not yet found a satisfactory solution.

The questions at issue are technical matter. Hence the first thing is to summarize what the two types models have and have not in common, so as to be able to locate and if possible clear up any divergencies that may exist. Three points will be mentioned.

(*i*) In the beginning of section 2, Prof. FISHER states three assumptions R 1-3 that are to be satisfied by a CC system, all in accordance with current theory of the probability structure and statistical estimation of multirelation models. Assumptions R 2-3 at first sight look very stringent, and in section 2.3 Prof. FISHER argues that they are not likely to be valid for economy-wide econometric models. The first point I wish to make is that the narrow stringency of Assumptions R 2-3 is only apparent. They come in a better light if the relations of the systems are specified in terms of *eo ipso* predictors (an *eo ipso* predictor is the conditional expectation that constitutes the residual-free part of the relation). This type of assumption has the fundamental advantage that it corresponds directly to the operational use for which the relations are intended. Furthermore, R 2-3 become automatically satisfied as an implication, not as an assumption, and this implication in its turn implies that the relations can be consistently estimated by least squares regression.

(*ii*) The theory of *eo ipso* predictors has shed new light on the much-discussed question about the operational significance of the structural relations of ID-systems. With reference to my report to the Study Week for details of the argument, it can be shown that the clearcut cause-effect interpretation of the behavioural relations of CC-systems does extend to ID-systems, but only at the price of a respecification of the system. For example, if the ID-system involves a behavioural relation which specifies the elasticity of investments with respect to profits as 0.4, the respecified assumption will be that 0.4 is the elasticity of savings with respect to *expected* profits. Actual profits and expected profits are different notions, conceptually and observationally; the actual profits are given by the statistical data, whereas the expected profits in the present context are given by the reduced form of the ID-system. The snag is that expected profits as derived from the reduced form stand in no obvious connection with expected profits in the sense of psychological anticipation. The respecification of the ID-system thus involves an element of arbitrariness.

The point just mentioned has a bearing upon the doubts that Prof. FISHER expresses in section 2.3 about the matrix triangularity in Assumption R 1. When Prof. FISHER states that it is somewhat unlikely that economy-wide models have entirely triangular A matrices, nobody can disagree, and what he says is even an understatement. On the other hand, if matrix A is made nontriangular the system becomes interdependent, and then we are confronted with the snag of a possible discrepancy between the two notions of expected values for the current endogenous variables. In other words, one kind of approximation has been replaced by another, and the arguments at issue do not indicate which approximation is preferable. This question brings me to the third point.

(*iii*) Prof. FISHER's considerations for or against CC- and ID-systems are on the whole of a theoretical nature. For my part I would instead make a plea for comparisons of an empirical nature. Above all, my plea is for studies where one and the same observational material is used for the testing of fullfledged CC- and ID-models. Such comparative studies are not as yet in the picture, so it is as yet an open question which type of model performs best in actual applications. In this connection there is the important question what criteria should be used when comparing and evaluating the performances. *Predictive tests* here come to the fore as the real touchstone for the validity of a model; that is, tests where the model as constructed on the basis of past experience is exploited to make forecasts about the unobserved future, and where in due course the forecasts are confronted with the actual developments.

FISHER

Professor WOLD has long been interested in the question of what are desirable properties of econometric models. This is an interesting question and his work and his present comments cast considerable light on it. As an economist, however, I think one ought to be interested not so much in the question: « What are good properties of econometric models? » but rather in the question: « What are the properties which good econometric models have? » In other words, one does not design a model so that it will have desirable properties: one does not design a model in order that it should have minimum delay; one does not design a model so that it will be an *eo ipso* predictor. One designs a model so that it represents something that one believes to be true about the real world. After the model has been designed, and not before, one asks: What properties does it have? And what properties can I give the estimates of the model by using different estimating techniques?

Now, indeed, there are always several ways in which a given real phenomenon can be represented in a model. Given two of these which we believe to be equally valid in terms of economic theory, we would of course choose the one which had better properties in terms of forecasting or in terms of the properties which the estimation techniques appropriate to the model will have. Frequently, however, that is not in fact the primary question. In general, we are not indifferent between two representations and the choice between models does not come on the question: « Will one be recursive and the other simultaneous? » The crucial question is rather that of which model represents what we believe to be true about the real world and which model and estimation technique is appropriate to the use which we intend to make of the results.

THEIL

A question of clarification. Do you intend to split up a large equation system into subsystems and construct principal components of the predetermined variables in each subsystem?

FISHER

Professor THEIL wishes to know whether I recommend splitting up a large equation system into subsystems and constructing principal components of the predetermined variables in each subsystem. This is not what I recommend, although similar procedures have in fact been adopted in economy-wide models in the past. My proposal is to take each equation (and indeed each endogenous variable within each equation) and to get a group of instrumental variables which are particularly appropriate for it. One does this by considering the causal structure of the system. The block-recursive system — in which the full system is divided into sectors and subsystems — is used in my paper to point out that it is safer to use some endogenous variables in some places than in others so that the eligible list of instruments is different for different equations.

HAAVELMO

I wonder whether FISHER would agree to a rough summing up of the situation that might help people who are not so much interested in technical details and who wonder what we get out of all this. The summing up could be something like this: We have small samples in practice, so that large sample properties of estimates are probably not too interesting. Then, if we had a clear situation as far as identification is concerned, that is to say, if we need no subtle tricks in order to ensure identification; and secondly, if by means of mere inspection we can find that there are exogenous variables which vary a great deal as compared with the disturbances u, and the exogenous variables are really uncorrelated with the u's in various directions, both simultaneously and recurrently; then we get reasonably good estimates even without refined methods. Now it is in the in-between cases that we may perhaps gain the most by being very particular about the estimation methods chosen. This would intuitively be the conclusion that one who works in practice with these things would draw. This does not of course in any way reduce the importance of stringent work on estimation methods, but I think it might — if it is reasonably correct — interest those who want to know what the practical conclusion is.

FISHER

I would generally concur with Professor HAAVELMO's summary of the situation. If, in fact, there are a lot of exogenous variables which vary a great deal as compared with the disturbances, then reasonably good estimates can be obtained — although I would not agree that refined methods are not called for. In my paper I pointed out that such situations are likely to be very rare in the estimation of economy-wide econometric models. In such cases, I tried to show that one could improve the results substantially by paying attention to the causal structure of the model rather than by incautiously applying techniques designed for the ideal situation.

FRISCH

I am afraid that what I am going to say will be considered perhaps as black heresy, and if I am saying it, I hope you will understand that, when I voice this opinion, if I say it in a very outspoken way, it is not because of a feeling of unfriendliness towards members of our fraternity, it is merely to save time that I am putting it in an extreme way.

Looking upon the problem of estimation in economy-wide models, I like to adopt the view-point of an economist who is called upon to give his advice to politicians. And if I look upon the matter in this way, I have an uncomfortable feeling that, in statistics, we are too frequently carried away by the terminology we use. You may say that terminology doesn't mean very much, you can call a thing a straw hat, and use the concept in a precise way in your logical deductions. It is not so dangerons to call it a straw hat, because then everybody will understand that this is just a sort of numbering of your concepts. But if you use other words which in every day life have had a certain connotation, you may be obsessed by this connotation and, as I say, be carried away through your terminology.

Looking upon the matter from the view-point of an economist giving advice to a politician, I would suggest that, when we speak of an estimator and the properties of estimators — we accept to put as the *main classification*: 1) Purposefully useful; 2) Purposefully irrelevant and 3) Purposefully detrimental.

I will explain what I mean by « purposefully useful ». Every estimator must be thought of in terms of the *purposes* for which we are going to use to coefficient or magnitude estimated. In this particular case the purpose is perhaps a *political* one. You must say that this is vague. Yes, I am sorry to say that these words are vague and they cannot be defined precisely because the purpose will vary from one application to another, so therefore the specification of what is meant by these properties must vary and can only be made more precise if you are explaining very explicitly for what purpose you are going to use your analytical results. Many of these properties of estimators which are fundamental from the view-point of application are very difficult to handle. Of course we like difficult problems, provided they are not beyond the limit of our ability. Then it would be very interesting to handle the problems. But when they pass *beyond* this stage it is very tempting to pick out certain properties which are not in true sense purposefully useful — they may even be purposefully detrimental but they have the property that *I am able to handle them*. I may decide then to work with these concepts instead of the really important ones which I am not able to handle. Let me take an example. I am going to multiply 13 by 27. I scratch my head and I think. « Oh, multiplication is such a terribly difficult operation, but I am very good at adding fiures, so why don't I add instead the two numbers 13 and 27 ». You wouldn't think that that was a very useful procedure.

I will illustrate what I have said by taking the two words « unbiased » and « consistent ». I am afraid that we have, to some extent, been carried away by the common meaning of these words. What is « biased » and « unbiased »? I would rather have preferred to use the term « not immoral » instead of « unbiased », because if you say that, everybody would have understood that you are just using the term in a technical sense. But if you say « unbiased » people can not become aware that you are fooling with words. It is probably hopeless to suggest a change of terminology, but I feel it would have been better if you had used these words « not immoral » instead of « unbiased ». Next with regard to « consistency ». Take a person who is not able to carry on a « consistent » discussion and a consistent way of using his logic. You would not respect him very much. About his conclusions you would probably say « This is a fallacy » I would rather have preferred that you use the terminology « not fallacious » instead of « consistent » because then again people would have understood that you are really just playing with words. I would seriously suggest that we change terminology to something which is *neutral* and just say precisely what we mean. Instead of « unbiased » I would prefer to say « *expectationally hitting* » because what is involved, is simply that the expectation of the estimate is equal to the thing which we estimate. Instead of « consistent », I would say « targetly converging » in the stochastic sense, because that's what we mean. It may be « converging asymptotically » i.e. it may be « targetly converging » in the limit when the number of observations becomes great.

« Unbiasedness » or, as I would like to call it, « expectationally hittingness » may not really he the property in which we are interested. Take a firm that is selling shoes: women's shoes and men's shoes. The owner of the firm will want very much that a random customer can be satisfied. Now, there are two types of shoes: men's shoes and women's shoes. The « univers » has probably a bimodal distribution. If the owner were to *make a guess* about what shoe the *next customer* would ask for, he would be off the mark if he focussed his attention on the mathematical expectation as derived from that bimodal distribution.

I would say that in this case « unbiassedness » is purposefully irrelevant. I don't think that this discussion about terminology is useless because many people are not able to protect themselves against risk of being dragged into false understanding and false valuation, which will lead away from what is *useful*. If you will allow me, I will pick out a few expressions in Prof. FISHER's presentation, which exemplifies how one may be carried away by the words. I put it down while he was talking. He said that WOLD has shown that this particular estimate — it was ordinary least square if I remember correctly — « retains desirable properties » — well, how do you know what is desirable? And, even worse, later on in his presentation, Prof. FISHER said — and I also took this down — « retain all the desirable properties » — I think he spoke about a full information maximum likelyhood or something that approximated it. It was in this connection that he said « retain *all* the desirable properties ». The immediate question is: « desirable *for what purpose?* » — May I finally give a third example. Professor FISHER spoke about using lagged variables as instruments and advised against this use for statistical reasons. Now, suppose I am in an underdeveloped country. And suppose I am giving advice to a politician who is up against the problem whether he should go in for education on birth control. In an underdeveloped country with a very heavy population increase and with all the implications which this means from the economic and social viewpoint, the problem is important and the politician is pondering very hard the foreseable effect; this case pertains, of course, essentially to lagged variables.

Suppose I come along and say « Well, I must advise against such a policy because it would upset certain calculations of mine regarding some specific properties of my estimates ». As I see it, these properties of my estimates are purposefully irrelevant. In this case it is precisely the lag values one would need to explain demographic development.

Finally, if I may just have one minute. Approaching all these things from the viewpoint of society at large, extending over time, we are accustomed to fall back on time series to a large extent. Sometimes we may use cross-section studies, but many economists and statisticians rely on time series that come from observation of what has happened in the past. Such series are, however, irrelevant for a great number of the estimates of the equations that enter into the equational system we have to use in *development planning*. We have for instance investment project specifications. We will have tens of thousands of such projects. Each of them will be described in engineering terms; for instance the sequence in time in which certain input elements are to be made: labour, products from domestic sectors, imports, etc., the burden which this entails on the balance of payment is extremely important, and so on. In most cases these data can be derived fairly correctly from an engineering analysis. There may, of course, be some uncertainties and you may — according to the price situation — shift a little in the use of input elements, but let us disregard that point for the moment. The engineering analysis will — with a fair degree of approximation — give the essential information. Similarly with regard to the time and the volume in which the capacity effect of the investment emerges. If you are building a hydroelectric power station, then you can say, from an engineering viewpoint that « next year I'll have one machine coming along; the year after, I'll have two more machines », and so on. All these things are given from the engineering viewpoint with a fair degree of accuracy and there is no question of trying to estimate the possible consequences of adopting a section of these 1,000 or 10,000 projects by looking back in our time series and discussing whether a certain time series estimate will be « unbiased » or « immoral » or have some other specific property which you *are able* to handle mathematically, but which have little relevance for the actual problem of development planning.

FISHER

The last point made in my reply to Professor WOLD is of course in agreement with part of Professor FRISCH's remarks. There is a growing literature on Bayesian estimation in which the decision problem to be answered by the model determines the estimation technique. This is a highly interesting development and it is one with which I am in nearly complete sympathy. There are several places in my paper in which this appears rather close to the surface. In general, I have said that the choice is between « consistency » and « efficiency » and I usually take the direction of opting for consistency. However, I tried to point out in the paper that this choice rather depends on what one expects to do with the results after one has obtained them. On the other hand, detailed analysis of the dependency of estimators in economy-wide econometric models on the use that is to be made of the results is difficult and so far only in its infancy. It does seem likely that properties of estimators such as consistency, unbiasedness, efficiency, and so forth, are properties which one believes are likely to be relevant in a rather wide class of decision problems.

Now for that part of Professor FRISCH's comments which deals with the use of persuasive definitions. While I am not out of sympathy with the argument that use of such terms as « unbiased », « consistency », and « instrument », may be misleading in that such words carry connotations from general application, I must say that I think Professor FRISCH's remarks on this point are irrelevant as regards my paper. However unfortunate the use of such terms may be, there is a long and distinguished history of their use in this way and while it may be too bad that such usage has come about, it is rather late for me to do anything about it.

I must, however, comment specifically on Professor FRISCH's discussion of my use of the word « instrument » since I am unable to tell whether he actually misunderstands me or whether this is just another example of the unfortunate use of a word with several connotations. « Instrument » in the theory of economic policy, where we owe much to Professor FRISCH, means a variable which can be controlled by a policy maker and which can be used to move toward those goals which the policy maker finds desirable. « Instrument » as I have used it is short for « instrumental variable » as the context of my paper makes abundantly clear. In this sense, it means a variable which can be taken to be uncorrelated with the disturbances but whose movements influence the endogenous variables of the system. Such a variable is used as an instrument in the sense that advantage is taken of its properties to secure parameter estimates. In Professor FRISCH's example, the birth rate may perfectly well be an instrument in the policy sense and fail to be an instrument in the statistical sense in that its movements when not influenced by the policy maker may be functions of the disturbances and the other endogenous variables of the system. Once again, my usage has quite a long tradition in econometrics behind it.

ALLAIS

This is not at all a criticism of FISHER's paper, but as an economist, I must confess that I feel myself in sympathy with some of Prof. FRISCH's words. I did not intend to say anything, but in the introduction to this Study Week, we read « The econometric method represents a big improvement over non-mathematical methods of studying phenomena connected with economic operations ». I think we must be very careful about the impression which can be given to outsiders by the methods we use. My feeling is that there is a terrible gap between the power of statistical methods and the limitations of economic models and the data. The methods we have are in advance, and very largely in advance, if we compare them to the economic models which we use. We may have wonderful methods to analyse the models which are built but the value of the conclusions will then depend primarily on the value of the models.

Thus the danger is that public opinion or politicians may greatly overestimate what we can do. The precision of the methods is one thing. Their value is another.

FISHER

I am in basic agreement with Professor ALLAIS' remarks. Anyone who has ever done a good deal of empirical econometric work knows perfectly well that the apparent precision which the theory of estimation gives to the results is not in fact present. Whether this is due to the properties of the model or to the character of the data is a matter of some dispute, but it is a common phenomenon.

What my paper is about is those situations so common in practice in which statistical techniques which have very nice properties in the secure situations for which they are designed do not in fact have such nice properties when applied to models of the type one actually has to estimate. In such cases we have to make all sorts of compromises to produce techniques which have properties which one deems desirable. For example, in almost all the original literature on simultaneous equation estimation, everyone simply assumed that there was no serial correlation in the disturbances. In economy-wide models, there is such serial correlation and the usual simultaneous equation estimators lose a good deal of their appeal if used incautiously.

LEONTIEF

In the context of previous discussion I would like to raise again the question of relative advantages and disadvantages of simultaneous as against independent estimation of the empirical parameters entering into different parts of an integrated analytical model. Although from a fundamental philosophical point of view indirect inference and direct observation have much in common, in daily practice of scientific investigation they differ from each other greatly.

So long as we operate with highly aggregative models, indirect inference must dominate the field. Since aggregative variables and parameters, in terms of which relationships are usually described, cannot be observed directly, they necessarily must be estimated indirectly. As soon, however, as disaggregation reaches the critical level at which the individual bits of data used in the theoretical model are identical or nearly identical to those familiar to the producers and consumers of individual goods and services in their daily practice, direct observation becomes possible and indirect inference dispensable. The empirical implementation of an analytical model comprising large sets of simultaneous, or at least interrelated equations, does not require any more indirect estimation of large sets of parameters imbedded in corresponding large sets of simultaneous statistical equations. The observations can be made one by one and essentially independently of each other. If and when this happens controversies concerning the choice among alternative methods of statistical estimation might lose the dominant position which they now occupy in discussion of econometric problems.

FISHER

Professor LEONTIEF in his interesting remarks looks forward to the day in which the use of highly disaggregate data will enable us to concentrate less on simultaneous equation problems. While I think that such a day is probably very far off, I do agree that such problems become of less importance as one goes to more and more disaggregated data. On the other band, there are at least some forms of disaggregation which do not lead to this desirable result. For example, as I argue in my paper, disaggregation in the form of securing data for smaller and smaller time periods does not avoid the simultaneity problem since the disturbances for such time periods are likely to be serially correlated. Whether other forms of disaggregation aid in avoiding simultaneity seems to me likely to depend on the model and the type of disaggregation. I think that so long as one is interested in models of the entire economy, simultaneity will remain an interesting question, even if one deals with data in terms of individual units. This is so, because it remains true, for example, that the income identity holds summing over all units.

While simultaneity introduces numerous problems, I think one should beware of the attitude (which I am aware is not Professor LEONTIEF's) that simultaneity problems should be avoided at the expense of doing violence to the model. There are now several techniques for dealing with such problems, and the fact that they may not be fully satisfactory does not justify the use of a wholly inappropriate technique in a context which is really simultaneous.

KOOPMANS

If I understand Prof. FISHER correctly, use was made of the data two or three times in order to select the set of predetermined variables that is drawn on for help in estimating a particular equation. Should one worry about the effect of repeated use of the same data and if so what kind of errors or lack of efficiency could have been produced thereby?

FISHER

Professor KOOPMANS wishes to know whether my recommendations for the choice of instrumental variables does not involve the double use of the same set of data. I think that it only does so superficially. One does indeed use the data in applying a stopping rule in the regression of endogenous variables on instruments, but the regressions obtained by applying the stopping rule are just the ones which are used in estimating the equation of interest. I do not see that this involves using the data twice any more than does two-stage least squares, choice of instrumental variables by principal components, or any simultaneous equation technique in which reduced form coefficients have first to be calculated.

MAHALANOBIS

I have a purely technical question. Can you break up into two or more random partitions? And secondly, it would be of interest to know what would be the degrees of freedom, that is, in the sense of independent observations, which might be available in concrete examples.

FISHER

Professor MAHALANOBIS wants to know if it is possible to break up the data into two random partitions. Presumably, one would want to do this to avoid the double use of data which I have just suggested is not present. I take it that what one would do would be to use one set of data to choose the instruments and the other set of data to estimate the parameters.

As indicated, I do not believe that this is necessary. In addition, it would be impossible in practice. The Brookings-SSRC model in its first form has something like one hundred equations; in its second, more disaggregate form, there will be many, many more. Each equation involves at least a few parameters. The data will run from the war until about 1962 by quarters, so that rough calculation gives about 64 observations. There are, however, an enormous number of eligible candidates for instruments, far greater than 64. I do not believe that one would want to use only 32 observations either for the estimation of the parameters, or for the choice of instruments from a list which will be far, far longer than 32. However, as stated, since I believe the answer to Professor KOOPMANS' question to be in the negative, the issue need not arise.

DECISION RULES AND SIMULATION TECHNIQUES IN DEVELOPMENT PROGRAMMING

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1. Introduction

Many underdeveloped countries, all Communist countries, and some developed Western countries have their Economic Plans nowadays. Such Plans may differ considerably as to character and scope, but it is quite generally true that they are based on certain considerations as to the goals to be pursued, as to the relationships between certain noncontrolled key variables on the one hand and the variables controlled by the decision-making authority on the other hand, as to the impact of outside forces, and as to the likely development of such outside forces over time. Given a number of assumptions one will arrive at a certain Plan, which specifies that *this* is to be done in year 1, *that* in year 2, and so on. In principle the Plan is optimal in the sense that it pursues the goals as well as possible subject to the constraints under which the economy operates.

But there is the problem of uncertainty. The development of outside forces over time has to be predicted, which cannot

(*) The author is indebted to Mr. J. Boas of the Econometric Institute, Rotterdam, who carried out the simulation experiment described in Section 6.

be done without forecast errors. The relationships describing the impact of these forces and of the variables controlled by the decision maker are not known with certainty either. The decision maker must therefore make his decisions (prepare his Plan) under conditions of uncertainty. Now there is the important feature that this uncertainty diminishes in the course of time. For example, how the outside forces behave in year 1 is unknown at the beginning of the year, at least not known with certainty, so that the decision to be made at the beginning of that year must be based on a forecast which is generally imperfect. At the beginning of year 2 one should know more about how the outside forces have behaved in year 1; also, one may have better ideas about how they will behave in year 2, because this is now « nearer future » than it was a year before. Clearly, the decision maker is in a position to take this new information into account when formulating his decision for year 2. But the Plan does not! The Plan was made at the beginning of year 1 and what it has to say about things to be done in year 2 is therefore necessarily based on the smaller amount of information which was available at the beginning of year 1.

This is of course generally recognized. It is the reason why a Plan is almost never taken completely seriously in the sense that it is executed literally until the very end. Plans are revised regularly in the light of new evidence. But this happens *after* the events, not before as is done by a « strategy » or « decision rule ». The main argument of this paper is that it is worthwhile to consider the possibility of development strategies instead of development plans. That is, rather than fixing once and for all what is to be done in year 1, in year 2, in year 3, ..., we consider a procedure of the following kind: now, at the beginning of year 1, a decision for that year is formulated and at the same time also a rule specifying what is to be done in years 2, 3, ... *depending on* the information that will be available by that time. The advantage of this procedure is not only that it is more systematic but also (and particularly) that, in general, taking account of the possibility that future information may be reacted to contributes to better results from the standpoint of the goal pursued (1).

The primary objective of this paper is to illustrate the method of linear decision rules in the context of development programming. The paper has no pretensions with respect to the development of model building in this field. Models describing the constraints under which the economy operates are undoubtedly important and in fact indispensable for the derivation of decision rules; but it is felt that the introduction of innovations in that area would shift the attention away from the primary objective and, therefore, a very simple capital-income ratio model is introduced in Section 2. In Section 3 a quadratic social preference function is formulated, after which the theory of linear decision rules follows in Sections 4 and 5.

Section 6 deals with the second objective of this paper: the use of simulation techniques in development programming. Since many of the structural relations and also the long-term development of many crucial outside forces are subject to a considerable degree of uncertainty, particularly in the field of growth and development, it seems plausible that simulation techniques can be valuable to indicate the range of variability of the outcomes which result from different policy procedures. In order to concentrate on the main idea, the application is confined to the same simple case as that of the earlier sections.

2. The Model

Let K_t be the stock of capital goods at the end of year t and Y_t national income of that year, both in real terms. One

(1) An additional advantage is that the strategy approach enables the decision maker to formulate predictions (no perfect predictions!) of his own future actions and of their consequences. But this aspect will not be pursued here.

of the equations of the model is

$$K_{t-1} = \rho Y_t \ , \tag{2.1}$$

where ρ is the capital-income ratio, which is assumed to be constant over time. Let C_t be consumption and x_t the savings ratio in year t:

$$C_t = (1 - x_t) Y_t \ . \tag{2.2}$$

Furthermore, we use two identities:

$$C_t + I_t = Y_t \tag{2.3}$$

$$K_t = K_{t-1} + I_t \ , \tag{2.4}$$

where I_t is net investment in year t. Finally, we need an equation describing the development of the population:

$$N_t = (1 + \nu_t) N_{t-1} \ , \tag{2.5}$$

where N_t is the mid-year population size and ν_t its rate of increase.

In what follows we shall be interested particularly in the rate of increase of per capita consumption, C_t/N_t. Using (2.2), (2.1) and (2.5) we find:

$$\frac{C_t}{N_t} = \frac{1 - x_t}{\rho} \frac{K_{t-1}}{(1 + \nu_t) N_{t-1}}$$

and hence:

$$\frac{C_t/N_t}{C_{t-1}/N_{t-1}} = \frac{1 - x_t}{1 - x_{t-1}} \frac{K_{t-1}}{K_{t-2}} \frac{1}{1 + \nu_t} \ . \tag{2.6}$$

Now $K_{t-1}/K_{t-2} = 1 + I_{t-1}/K_{t-2} = 1 + x_{t-1}\ Y_{t-1}/K_{t-2}$ from (2.2) and (2.3). Since $Y_{t-1}/K_{t-2} = 1/\rho$ from (2.1), we conclude that the ratio of K_{t-1} to K_{t-2} is equal to $1 + x_{t-1}/\rho$. Taking logarithms in (2.6), we then find the following expression for the first difference of the logarithm of per capita consumption:

$$\Delta\,[\log\,(C_t/N_t)] = \log\,\frac{1 - x_t}{1 - x_{t-1}} + \log\,(1 + x_{t-1}/\rho) - \\ - \log\,(1 + \nu_t)\ . \tag{2.7}$$

This equation is the only aspect of the model that will be used in the sequel. We shall consider a decision maker who is interested in two things: the savings ratio x_t, which he controls, and the logarithmic rate of change of per capita consumption, which he does not control. The approach to be followed requires that the latter (uncontrolled) variable be expressed linearly in the former. Eq. (2.7) is nonlinear and it will therefore be linearized. Taking all logarithms as natural logarithms, we shall approximate the log of $1 + x_{t-1}/\rho$ by x_{t-1}/ρ. We shall also approximate the log of $1 + \nu_t$ by ν_t (although this is not strictly necessary, since the expression does not involve the savings ratio). For the first term on the right we use:

$$\log\frac{1 - x_t}{1 - x_{t-1}} = \log\left[1 - \frac{x_t - x_{t-1}}{1 - x_{t-1}}\right] \approx -\frac{x_t - x_{t-1}}{1 - x_{t-1}}\,. \tag{2.8}$$

But this is still nonlinear in x_{t-1} and we will therefore apply the following (crude) approximation (2). We should expect that the savings ratio will be of the order of 15 to 25%, so that $1/(1 - x_{t-1})$ is then of the order of 1.2 or 1.3. This range of uncertainty is not very sizable; moreover, we multiply $1/(1 - x_{t-1})$ by $x_t - x_{t-1}$, which is generally close to zero, particularly since we shall put a penalty on large values of

(2) But see footnote 5 below for a more accurate approximation.

$|x_t - x_{t-1}|$ in Section 3. Therefore, we shall approximate the expression (2.8) by $-b(x_t - x_{t-1})$, where b is regarded as a fixed coefficient (about equal to 1¼).

Equation (2.7) is now written as

$$(2.9) \qquad \Delta[\log (C_t/N_t)] = -bx_t + (b + 1/\rho)x_{t-1} - v_t ,$$

which is the form with which we shall work in the remainder of this paper.

3. The preference function

Our decision maker is supposed to formulate a quadratic preference function which he wishes to maximize. We shall consider a very simple quadratic function, which amounts to a sum of squares as far as the successive log-changes in per capita consumption is concerned. This should not be regarded in the sense that we really believe that the decision maker's desires *are* quadratic; it means only that we try to approximate the decision maker's preferences by a quadratic function in the relevant range (in the same way as the constraints of the economy are approximated linearly in the relevant range).

Specifically, let d_t be the « desired rate » of increase of the logarithm of per capita consumption in year t. For example, $d_t = 0.1$ (10%); then, given the quadratic character of the preference function, an actual rate of increase of 5% will have a disutility of $(5-10)^2 = 25$, a 4% increase will have a disutility of $(4-10)^2 = 36$, and so on. Let us write y_t for the discrepancy between the actual and the desired rate of increase in year t:

$$(3.1) \qquad y_t = \Delta[\log (C_t/N_t)] - d_t ,$$

then it follows from (2.9) that y_t is determined as follows:

$$y_t = -bx_t + (b + 1/\rho)x_{t-1} - v_t - d_t \,. \tag{3.2}$$

We consider the sum of squares of the y_t's over a period of T years, where T represents the horizon which the decision maker takes into account. It will prove useful to introduce a matrix notation to handle all years simultaneously:

$$y = \begin{bmatrix} y_1 \\ y_2 \\ \vdots \\ y_T \end{bmatrix}, \quad x = \begin{bmatrix} x_1 \\ x_2 \\ \vdots \\ x_T \end{bmatrix}, \tag{3.3}$$

then (3.2) for $t = 1, \ldots, T$ can be written in the form

$$y = Rx + s \,, \tag{3.4}$$

where

$$R = \begin{bmatrix} -b & 0 & 0 & \ldots & 0 & 0 \\ b + \frac{1}{\rho} & -b & 0 & \ldots & 0 & 0 \\ 0 & b + \frac{1}{\rho} & -b & \ldots & 0 & 0 \\ \vdots & \vdots & \vdots & & \vdots & \vdots \\ 0 & 0 & 0 & \ldots & -b & 0 \\ 0 & 0 & 0 & \ldots & b + \frac{1}{\rho} & -b \end{bmatrix} \tag{3.5}$$

$$(3.6) \qquad s = \begin{bmatrix} \left(b + \frac{1}{\rho}\right) x_0 - \nu_1 - d_1 \\ -\nu_2 - d_2 \\ \vdots \\ -\nu_{T-1} - d_{T-1} \\ -\nu_T - d_T \end{bmatrix},$$

where x_0 is the savings ratio in the year preceding the first (which is taken as given from the past). Note that all R-elements above the diagonal should necessarily vanish, because they represent the effect of controlled variables (x_t) on earlier noncontrolled variables ($y_{t'}$ with $t' < t$). The diagonal elements specify the effectiveness of controlled variables on noncontrolled variables in the same year, and the elements below the diagonal represent lagged effects.

It was stated above that we are interested in minimizing the sum of squares of the discrepancy between actual and desired rates of increase of the logarithm of per capita consumption. This amounts to minimizing $y'y$. But this will be amended to the effect that we shall also be interested in moderate changes of the savings ratio, the argument being that a savings ratio of 20% last year followed by one of 15% this year and then of 25% next year is difficult to realize. To handle this, we introduce the sum of squares of the successive differences of the savings ratio:

$$\sum_{t=1}^{T} (x_t - x_{t-1})^2 = x' \begin{bmatrix} 2 & -1 & 0 & \dots & 0 & 0 \\ -1 & 2 & -1 & \dots & 0 & 0 \\ 0 & -1 & 2 & \dots & 0 & 0 \\ \vdots & \vdots & \vdots & & \vdots & \vdots \\ 0 & 0 & 0 & \dots & 2 & -1 \\ 0 & 0 & 0 & \dots & -1 & 1 \end{bmatrix} x - 2x_0 x_1 + x_0^2 =$$

$$= x' \Delta x - 2x_0 e_1' x + x_0^2 ,$$

where Δ is the $T \times T$ matrix of the second-difference transformation and e_1 the first unit vector of order T (i.e., the first column of the $T \times T$ unit matrix). Note that x_0^2 is a constant (it is given from the past), so that it can be omitted from the preference function. This preference function, which the decision maker wishes to maximize, is then assumed to be of the following form:

$$w(x, y) = g x_0 e_1' x - \frac{1}{2}(g x' \Delta x + y' y) \,. \tag{3.7}$$

Thus the decision maker is supposed to minimize a weighted sum of squares of two sets of differences. One set deals with the successive differences of the savings ratio, the other with the differences between actual and desired log-changes in per capita consumption. Of course, g should be a positive number; it measures the seriousness of a given change in the savings ratio relative to that of a discrepancy between the actual and the desired log-change in consumption of the same numerical size.

4. Expected utility and certainty equivalence

Our problem in mathematical terms can in the first instance be described as that of maximizing the quadratic preference function (3.7) subject to the linear constraint (3.4). But it is readily seen that the real problem is more complicated. For carrying out this conditional maximization requires that the determining factors, such as the rates of increase of the population, are known before. This is evidently not the case, so that we must conclude that the decisions have to be made under conditions of uncertainty. (The ν's are the only uncertain factors which enter into the problem as it is posed here, which is of course highly restrictive, but it is easy enough to extend the list of such factors). We shall attack this problem by assuming that the uncertainty is of the probabilistic type and that the decision maker is interested in maximizing expected utility. That is, he is supposed to maximize the expectation of the preference function (3.7) subject to the constraint (3.4), the latter being interpreted stochastically (with random ν's).

It is worthwhile to consider the implications of this procedure in somewhat more detail. Take the first year, at the beginning of which the decision x_1 has to be made. The rates of increase of the population, $\nu_1, \nu_2, \ldots, \nu_T$, are then unknown and are supposed to be subject to a T-dimensional joint distribution. One year later the decision x_2 has to be made and the decision maker will then know more, particularly about ν_1 but perhaps also about later ν's, because the development of the population during the first year may have shed some light on the probable development during later years. Clearly, the decision maker should be able to use this information gained during the first year when he formulates his decision x_2 at the beginning of the second year. In the same way, at the beginning of the third year he knows still more (particularly about ν_2) and he can use this additional information for his decision x_3. And so on.

It follows that it is the decision maker's task to formulate, for each year $t=1, \ldots, T$, the decision x_t as a function of the information that will be available at that time. Such a series of decisions $x_1, \ldots, x_T$ written as functions of relevant information is a strategy or decision rule. More specifically, the decision maker's task is to find the *maximizing strategy*, i.e., the strategy which maximizes the expectation of the preference function subject to the contraints. In general it is not easy to find this maximizing strategy, but it is very simple when the preference function is quadratic and when the constraints are linear. The solution is found by applying the theorem of « first-period certainty equivalence », which in our case amounts to the following: the first-period decision of the maximizing strategy is identical with the first-period decision that would be made if the uncertainty aspect would be disregarded by replacing all random ν's by their expectations. This means that the problem is reduced, for the decision of the first year (x_1) at least, to an ordinary conditional maximization problem: maximize the quadratic preference function (not its expectation) subject to the constraint (3.4) on the understanding that the vector s of this constraint is replaced by its expectation; i.e., in (3.6) we should replace the ν's by the expectations of the ν's. Clearly, this solves the uncertainty problem in an almost trivial way. For the second-period decision (x_2) one can proceed in precisely the same way one period later, because by that time the second period will have become the first. And so on [3].

5. The maximizing strategy

The results mentioned in the preceding section can be regarded as a separation of the decision problem in two successive steps: first maximize as if there is no uncertainty, then replace certain random variables in the result by their expectations. We shall start with the first step under the assumption that the horizon (T) is so large that it can be

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(3) The first-period certainty equivalence theorem is due to H.A. Simon [2] and was subsequently generalized by the present author [3, 4]. For applications to a microeconomic (paint factory) case reference is made to C.C. Holt *et alii* [1]. For a more extensive discussion including several other applications, see the author's recent monograph [5].

effectively regarded as infinite. The derivations are given in the Appendix and the result for the optimal first-period decision (x_1^0) is as follows:

$$x_1^0 = \lambda x_0 - \frac{\lambda b}{b^2 + b/\rho + g}(\nu_1 + d_1) + \tag{5.1}$$

$$+ \frac{(1-\lambda)\,b + 1/\rho}{b^2 + b/\rho + g} \sum_{t=2}^{\infty} \lambda^{t-1} (\nu_t + d_t) \, ,$$

where

$$\lambda = \frac{[1 + 4\rho^2 (b^2 + b/\rho + g)]^{\frac{1}{2}} - 1}{[1 + 4\rho^2 (b^2 + b/\rho + g)]^{\frac{1}{2}} + 1} \, . \qquad (0 < \lambda < 1) \tag{5.2}$$

The result (5.1) shows that the optimal savings ratio in the first year (x_1^0) consists of two parts, one of which deals with the savings ratio in the preceding year (x_0) and the other with population increases and desired consumption increases in the first year and later (ν_1, d_1, ν_2, d_2, ...). We find that the effect of ν_1 and d_1 on the optimal savings ratio is negative but that the effect of later ν's and d's is positive. This is the natural consequence of the fact that society has to spend for immediate welfare but to save for future welfare.

As an example we take the special case $\nu_t = \nu$, $d_t = d$ for all t; then (5.1) is simplified to

$$x_1^0 = \lambda x_0 + \frac{\lambda/\rho}{(1-\lambda)(b^2 + b/\rho + g)}(\nu + d) \, . \tag{5.3}$$

which means that this year's savings ratio is a certain fraction of last year's savings ratio (λx_0) plus a constant (k, say). For the following year we then have

$$x_2^0 = \lambda x_1^0 + k = \lambda^2 x_0 + \lambda k + k \ ,$$

from which it is evident that the savings ratio in successive years moves gradually from x_0 to

(5.4) $$k + \lambda k + \lambda^2 k + \ldots = \frac{k}{1 - \lambda} = \frac{\lambda/\rho}{(1 - \lambda)^2 \, (b^2 + b/\rho + g)} (\nu + d) \ .$$

Take, e.g., ρ equal to 3 or 4 years, b slightly above 1, and g a small positive number [which is tantamount to saying that a successive change in the savings ratio, $x_t - x_{t-1}$, is considered much less serious than a discrepancy y_t (between actual and desired consumption increase) of the same size]. Then

(5.5) $$b^2 + b/\rho + g \approx 2 \ ,$$

so that λ according to (5.2) is approximately

(5.6) $$\lambda \approx \frac{\sqrt{1 + 8\rho^2} - 1}{\sqrt{1 + 8\rho^2} + 1} \ ,$$

i.e., about 0.8 when ρ is about 3 years or so. Then the limit (5.4) to which the savings ratio approaches is about 3 times

$(\nu + d)$. Thus, if the rate of increase of the population is $\nu = 0.02$ per year and if the desired rate of increase of per capita income is 0.06 per year, the limit of the savings ratio is of the order of 25%.

When the ν's and d's are not constant over time, we have to substitute their values directly into (5.1). It is seen that the influence of future ν's and d's is of the decreasing exponential type. But it is impossible to compute x_1^0 when the future ν's are unknown. We have then to rely on maximizing expected utility and on the first-period certainty equivalence theorem, provided of course that the relevant expectations are known. Note that these expectations are *conditional* expectations, *given* the information available at the moment when the decision must be made. Suppose, e.g., that the rate of increase of the population fluctuates around a mean $\bar{\nu}$ and that it satisfies the following stochastic difference equation:

$$(5.7) \qquad \nu_t - \bar{\nu} = \frac{1}{2}(\nu_{t-1} - \bar{\nu}) + \varepsilon_t \, ,$$

where ε_t is a random variable with zero mean and zero correlations over time. Then the expectation of $\nu_1 - \bar{\nu}$, given the information available at the beginning of the first year, is $\frac{1}{2}(\nu_0 - \bar{\nu})$; that of $\nu_2 - \bar{\nu}$ (under the same condition) is $\frac{1}{4}(\nu_0 - \bar{\nu})$; and so on. By substituting these expectations in the right-hand side of (5.1) we obtain the first-period decision of the maximizing strategy (under the assumption $d_t = d$):

$$(5.8) \qquad \tilde{x}_1 = \lambda x_0 + \frac{\lambda/\rho}{(1-\lambda)(b^2 + b/\rho + g)}(\bar{\nu} + d) - \frac{\lambda(b - 1/\rho)}{(4 - 2\lambda)(b^2 + b/\rho + g)}(\nu_0 - \bar{\nu}) \, .$$

This result shows that when last year's population increase is *above* average ($\nu_0 > \bar{\nu}$), the savings ratio to be applied in this year is *below* the level that would be applied if the population increase would be known to remain constant at the value $\bar{\nu}$. This is apparently due to the fact that this year's population increase is then also expected to be above average, which has a negative influence on the savings ratio of this year, see (5.1). It is true that the population increases of later years, too, are expected to be above average, which has a positive influence, but this effect is evidently of less importance.

The desired rate of increase of per capita consumption, d_t, need not to be constant. A simple alternative specification is

$$d_t = d - (d - d_1)r^{t-1}, \qquad (0 < r < 1, \ d_1 < d) \tag{5.9}$$

which implies that the desire of the first year is confined to d_1, after which it increases gradually and approaches d in the limit. The first-period decision of the maximizing strategy under conditions (5.9) and (5.7) is:

$$\begin{aligned} \tilde{x}_1 = \lambda x_0 &+ \frac{\lambda/\rho}{(1-\lambda)(b^2 + b/\rho + g)}(\bar{\nu} + d_1) + \\ &+ \frac{\lambda(1-r)[(1-\lambda)b + 1/\rho]}{(1-\lambda)(1-\lambda r)(b^2 + b/\rho + g)}(d - d_1) - \\ &- \frac{\lambda(b - 1/\rho)}{(4 - 2\lambda)(b^2 + b/\rho + g)}(\nu_0 - \bar{\nu}) . \end{aligned} \tag{5.10}$$

On comparing this result with (5.8), we find that the savings ratio is equal to that of (5.8) under the assumption that the desired increase is constant at the level d_1 except that a certain multiple $d - d_1$ has to be added. This multiple is obviously positive, since the increasing desires in later years require more saving in the beginning.

6. The simulation technique

We shall now apply the ideas set forth numerically by means of a simulation technique. Thus we consider a number of countries whose economies are taken care of by an equal number of decision makers. These decision makers control the savings ratios of their respective countries and do so, year after year, on the basis of the decision rule (5.10). That is, each of them maximizes the expectation of the utility function (3.7) subject to the random constraint (3.4), where it is assumed that the desired values of the log-changes in per capita consumption are of the form (5.9) and that the rate of change of the population satisfies the stochastic difference equation (5.7). This equation supplies the random element of the process, which is of course the rationale of the simulation technique.

Fifty countries have been considered, each during a period of 50 years. All start with an initial savings ratio (x_0) of 10%. The first-year desired increase (d_1) of per capita consumption is 2½%, the long-run desired increase (d) is 7½%. The coefficient r of (5.9) is put equal to 0.95, which implies that after about $t = 15$ years there is a desired increase d_t half-way between the first-year and the long-run desire, $d_t = \frac{1}{2}(d_1 + d) = 5\%$. The expected value of the rate of increase of the population, $\bar{\nu}$, is taken equal to 0.015. The random variables ε_t of (5.7) have been generated as normal variates with zero mean and three alternative standard deviations: 0.005, 0.01 and 0.02. These alternatives are chosen to illustrate the importance of different degrees of uncertainty (4). The values of the standard deviations are of course on the high side when they are considered as describing a distribution relating to population changes; but this is hardly relevant in the present connection, since the random elements of this study should be regarded as representing uncertainty in development problems in general. The autoregressive scheme (5.7) is started up with a value for $v_0 - \bar{v}$, which is a random normal variable with zero mean and a variance equal to $\frac{4}{3}$ times the variance of the ε's. It is easily verified that this is the variance of $v_t - \bar{v}$ for any t when we assumed that the autoregressive scheme has been in operation since Adam and Eve.

The b-value chosen is 1¼ in accordance with the remarks made at the end of Section 2. For the capital-income ratio (ρ) we take 3⅓ years. The coefficient g of the preference function (3.7) is put equal to 0.1, which means that a change $|x_t - x_{t-1}|$ in the savings ratio is considered ten times less serious than a discrepancy y_t (between desired and realized log-change in per capita consumption) of the same size.

It would go too far to mention all results for all countries in all years separately. One sample case is presented in Table 1. It shows that the savings ratio is increased from 10 to almost 13% in the first year, to almost 15% in the second year, and so on, after which the further increases tend to become smaller and smaller. In some years there are decreases rather than increases. After 50 years the savings ratio is close to 30%. In the first two years there is a decrease (between 1 and 2%) in per capita consumption, witness the negative values

(4) Note that the ε's generated are identical for each triple (apart from an adjustment such that their standard deviations are 0.005, 0.01 and 0.02, respectively). That is, for each country i and each year t (where i, $t = 1, \ldots, 50$) one single ε is generated. This was done to guarantee a perfect *ceteris paribus* situation for the three different standard deviations.

of the first two elements of the second column. This is the natural consequence of the necessity to start the development by reducing consumption at the beginning. It is of course not the Law of the Medes and the Persians that the period of successive decreases should last two years, since the process is subject to random fluctuations. Table 2 gives a survey of results and reveals that 2 years is about the average period during which consumption decreases. When there is little random variability ($\sigma_\varepsilon = 0.005$) the individual periods of decline are concentrated closely around this average; but for larger standard deviations there is also more variability of the decline periods, as could be expected. The category « other cases » refers to those in which the period of decline is interrupted by one or more years during which per capita consumption increases rather than decreases; e.g., decreases in the first two years followed by an increase in the third and a decrease in the fourth year. The signs of changes in the fifth year and later have been disregarded; they are negative in some isolated cases for $\sigma_\varepsilon = 0.02$.

Table 1 shows further that in the first decade the total logarithmic increase of per capita consumption is 0.1611, which corresponds to a percentage increase of about 17. In the second decade it is much larger: 0.6757 corresponding to a percentage increase of almost 100. These increases, too, are subject to random variability. A survey of the quartiles of decade growth rates is presented in Table 3. The results show a gradual increase of these quartiles over time. Of course, the increase is not so regular in every individual case! This is illustrated by Table 1, which indicates that in that case the decade growth rate decreases in the third and fourth decade. For the set of all data generated it appears that different values of σ_ε do not lead to much difference as far as the median growth rates are concerned, but they do lead to important differences in dispersion.

TABLE I. — *A sample case of 50 years development* ($\sigma_\varepsilon = 0.01$).

Year	Savings ratio (× 1000)	Log-change in per capita consumption (× 10,000)	Decade log-change (× 10,000)
1	127	—191	
2	149	—128	
3	167	30	
4	182	195	
5	196	142	
6	207	319	
7	216	268	
8	224	308	
9	229	310	
10	234	357	1611
11	238	650	
12	246	677	
13	254	755	
14	261	621	
15	265	464	
16	265	603	
17	266	660	
18	269	741	
19	272	799	
20	276	787	6757
21	280	914	
22	284	648	
23	284	669	
24	283	886	
25	286	712	
26	287	701	
27	286	437	
28	282	454	
29	278	600	
30	277	488	6508
31	276	623	
32	277	482	
33	277	702	
34	279	541	
35	280	684	
36	282	441	
37	280	558	
38	280	593	
39	280	708	
40	283	541	5873
41	283	747	
42	286	652	
43	287	541	
44	286	733	
45	288	688	
46	289	753	
47	291	769	
48	293	777	
49	295	711	
50	295	647	7017

TABLE 2. — *Periods of initial decline of per capita consumption.*

	Number of cases when the standard deviation of ε is		
	0.005	0.01	0.02
Zero	—	1	4
One year	10	11	7
Two years	35	22	13
Three years	5	10	11
Four years	—	1	3
Other cases	—	5	12

TABLE 3. — *Quartiles of decade growth rates of per capita consumption* (× 10,000).

	First decade	Second decade	Third decade	Fourth decade	Fifth decade
			$\sigma_\varepsilon = 0.005$		
Lower quartile . .	1959	5626	6494	6747	7095
Median	2184	5730	6738	7014	7283
Upper quartile . .	2433	5885	6880	7254	7483
			$\sigma_\varepsilon = 0.01$		
Lower quartile . .	1718	5462	6330	6475	6976
Median	2166	5672	6818	7010	7344
Upper quartile . .	2664	5980	7102	7489	7752
			$\sigma_\varepsilon = 0.02$		
Lower quartile . .	1231	5133	6004	5934	6736
Median	2130	5553	6980	7004	7474
Upper quartile . .	3126	6169	7546	7961	8288

The final judgment on the performance of the process must be based on the criterion function. If we confine ourselves to the first fifty years, the function can be written in the form:

$$(6.1) \qquad 0.1 \sum_{t=1}^{50} (x_t - x_{t-1})^2 + \sum_{t=1}^{50} y_t^2,$$

the expectation of which we try to minimize. The values which the function (6.1) takes in the 50×3 cases are subject to a distribution and will therefore differ from case to case. However, the distribution of the performance measures (6.1) is not very exciting. A more useful measure, particularly in the context of the present paper, is a *relative* performance measure describing how well the policy has done compared with two alternative approaches: the « perfect » approach, which forecasts correctly all future values which the chance mechanism will produce, and the « naive » approach, which does not use the information which is available to the decision maker at the moment when he has to make his decision. The former approach is based on equation (5.1), its ν's being generated by the ε's of (5.7) as these were actually generated by the normal chance mechanism. [Strictly speaking, (5.1) and (5.7) require that we use an infinite number of ε's, which is somewhat unpractical, but the 25 which have been generated beyond the 50 years of the experiment are sufficient for our purpose]. The second, naive approach is based on (5.10) except that the last term (in $\nu_0 - \bar{\nu}$) is deleted. That is, the decision maker just acts as if the rate of increase of the population has been and will remain constant at the level $\bar{\nu}$. The savings ratio of the last policy is not affected by any random element and can therefore be computed for any year t at the beginning of the

50-year period [5]. It is the same for all 50 countries and it can be regarded as a primitive kind of fixed-plan policy under disregard of any kind of new information.

The relative performance measure that will be used takes the form of a ratio:

$$\frac{(6.1)_{\text{strategy}} - (6.1)_{\text{perfect}}}{(6.1)_{\text{naive}} - (6.1)_{\text{perfect}}} .$$

That is, we take the strategy value of the function (6.1) as a deviation from the value which this function takes in case the perfect approach is adopted and express this difference as a fraction of the similar difference for the naive approach. The ratio is zero when the strategy approach yields as good results as the perfect approach; it is one if the strategy approach is as good or as bad as the naive approach. We should obviously expect that the ratio is generally between 0 and 1, and this is indeed the case as we see from Table 4.

TABLE 4. — *Relative performance of the strategy approach.*

	Standard deviation of the ε' s		
	0.005	0.01	0.02
Lower quartile . . .	0.816	0.844	0.866
Median	0.896	0.911	0.916
Upper quartile . . .	0.995	0.973	0.976

[5] In eq. (2.8) we approximated the logarithm of the ratio of $1-x_t$ to $1-x_{t-1}$ by the ratio of $-(x_t-x_{t-1})$ to $1-x_{t-1}$, which was approximated further to $-b(x_t-x_{t-1})$ to obtain a linear expression. The latter approxi-

The median turns out to be of the order of 0.9, which means that in about 50% of all cases the value of the objective function (6.1) was reduced by the strategy approach by at least 10% (when our yardstick is the distance from naivety to perfection). Whether this is to be considered as much or as little is a question of taste; the present author must confess that he is not impressed by this figure. But a more important question is whether the strategy approach leads to sufficiently superior outcomes in general, and this can only be taught by future research. The results of Table 4 suggest further that the dispersion of the individual relative performances tends to decrease when the standard deviation of the ε's becomes smaller. This is a somewhat surprising result if it would be true; to find out whether it is true or not requires a larger experiment.

Simulation experiments are useful to show the random variability of outcomes whenever the model on which they are based is too complicated to be handled analytically by the ordinary methods of probability theory. The model used here cannot claim to be complicated, but, in a more general context, it is not difficult to think of other models in development programming which do have this feature. Even in this simple case, however, the method illustrates clearly something which is obvious to those who think in probability terms but which is not so obvious to those who think in terms of nonstochastic economics; viz., that a procedure which is good in general is not necessarily good (at least not good in a limited time period) in all individual cases. For example, the relative performance measure is above 1 in almost 20% of all cases. Thus, if we

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mation can be improved upon if we replace b by the reciprocal of 1 minus the x_{t-1} of the fixed-plan policy, because it turns out (in the present application at least) that the fixed-plan savings ratios do not differ too much from the corresponding savings ratios of most of the strategy cases. This alteration would imply that the multiplicative coefficients of eq. (2.9) cease to be constant over time, which makes the computations more complicated, but probably not in a hopeless manner.

advocate the strategy method to Turkey, Egypt and Iran, it may turn out after half a century that our advice was good for Turkey and Egypt, but not for Iran. This should be stated frankly to the policy makers concerned, which can be done easily with the aid of a simulation experiment. In fact, this is another, practical advantage of the simulation method: presenting its results in tabular and graphical form to laymen is far simpler than the presentation of many sheets of algebra. This advantage remains even when it is true, as in the case considered here, that the model used is not very complicated.

Appendix

Maximizing the quadratic preference function (3.7) subject to the constraint (3.4) can be done conveniently by using this constraint to eliminate the vector of noncontrolled variables. The result is

$$(A.1) \qquad w(x, \; Rx + s) = k_0 + k'x + \frac{1}{2} x'Kx ,$$

where $$k_0 = s's$$

$$(A.2) \qquad k = gx_0e_1 - R's$$
$$K = -(g\Delta + R'R) .$$

The optimal decision is then

$$(A.3) \qquad x^0 = -K^{-1}k = (g\Delta + R'R)^{-1}(gx_0e_1 - R's) ,$$

which shows that our first task is to find the inverse of $g\Delta + R'R$. Let us write

$$(A.4) \qquad c = b + \frac{1}{\rho}$$

then we have

$$R = \begin{bmatrix} -b & 0 & 0 & \dots \\ c & -b & 0 & \dots \\ 0 & c & -b & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}, \quad R'R = \begin{bmatrix} b^2+c^2 & -bc & 0 & \dots \\ -bc & b^2+c^2 & -bc & \dots \\ 0 & -bc & b^2+c^2 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}$$

and hence

$$\text{(A.5)} \qquad g\Delta + R'R = \begin{bmatrix} b^2+c^2+2g & -(bc+g) & 0 & \dots \\ -(bc+g) & b^2+c^2+2g & -(bc+g) & \dots \\ 0 & -(bc+g) & b^2+c^2+2g & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix},$$

which is an infinite band matrix in the infinite-horizon case. The derivations will be somewhat simplified if we write (A.5) in the following form:

(A.6)

$$\begin{aligned} g\Delta + R'R \\ = 2(bc+g) \end{aligned} \begin{bmatrix} 1+\dfrac{1}{2\rho^2(bc+g)} & -\dfrac{1}{2} & 0 & \dots \\ -\dfrac{1}{2} & 1+\dfrac{1}{2\rho^2(bc+g)} & -\dfrac{1}{2} & \dots \\ 0 & -\dfrac{1}{2} & 1+\dfrac{1}{2\rho^2(bc+g)} & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}.$$

The matrix on the right [disregarding the factor 2 $(bc+g)$] can be written as a scalar (p) times the product of two simple matrices:

$$p\begin{bmatrix} 1 & -\lambda & 0 & \dots \\ 0 & 1 & -\lambda & \dots \\ 0 & 0 & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}\begin{bmatrix} 1 & 0 & 0 & \dots \\ -\lambda & 1 & 0 & \dots \\ 0 & -\lambda & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix} = \begin{bmatrix} p(1+\lambda^2) & -p\lambda & 0 & \dots \\ -p\lambda & p(1+\lambda^2) & -p\lambda & \dots \\ 0 & -p\lambda & p(1+\lambda^2) & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}$$

so that we can identify:

$$p(1+\lambda^2) = 1 + \frac{1}{2\rho^2(bc+g)}, \qquad -p\lambda = -\frac{1}{2}.$$

This implies $2p\lambda = 1$. When added to and subtracted from the equation for $p(1+\lambda^2)$ we obtain expressions for $p(1+\lambda)^2$ and $p(1-\lambda)^2$:

$$p(1+\lambda)^2 = 2 + \frac{1}{2\rho^2(bc+g)}, \qquad p(1-\lambda)^2 = \frac{1}{2\rho^2(bc+g)}.$$

Taking the ratio and then square roots leads to a linear equation in λ with the following solution:

(A.7) $$\lambda = \frac{[1+4\rho^2(bc+g)]^{\frac{1}{2}} - 1}{[1+4\rho^2(bc+g)]^{\frac{1}{2}} + 1} \qquad (0 < \lambda < 1)$$

and the solution for p is then $1/2\lambda$. On combining these results with (A.6) we conclude:

(A.8) $$g\Delta + R'R = \frac{bc+g}{\lambda}\begin{bmatrix} 1 & -\lambda & 0 & \dots \\ 0 & 1 & -\lambda & \dots \\ 0 & 0 & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}\begin{bmatrix} 1 & 0 & 0 & \dots \\ -\lambda & 1 & 0 & \dots \\ 0 & -\lambda & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}.$$

It is now easy to determine the inverse of $g\Delta + R'R$, since

$$\begin{bmatrix} 1 & 0 & 0 & \dots \\ -\lambda & 1 & 0 & \dots \\ 0 & -\lambda & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}^{-1} = \begin{bmatrix} 1 & 0 & 0 & \dots \\ \lambda & 1 & 0 & \dots \\ \lambda^2 & \lambda & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}$$

and

$$\begin{bmatrix} 1 & 0 & 0 & \dots \\ -\lambda & 1 & 0 & \dots \\ 0 & -\lambda & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}^{-1} \begin{bmatrix} 1 & -\lambda & 0 & \dots \\ 0 & 1 & -\lambda & \dots \\ 0 & 0 & 1 & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix}^{-1}$$

$$= \begin{bmatrix} 1 & \lambda & \lambda^2 & \lambda^3 & \lambda^4 & \dots \\ \lambda & \lambda^2+1 & \lambda^3+\lambda & \lambda^4+\lambda^2 & \lambda^5+\lambda^3 & \dots \\ \lambda^2 & \lambda^3+\lambda & \lambda^4+\lambda^2+1 & \lambda^5+\lambda^3+\lambda & \lambda^6+\lambda^4+\lambda^2 & \dots \\ \lambda^3 & \lambda^4+\lambda^2 & \lambda^5+\lambda^3+\lambda & \lambda^6+\lambda^4+\lambda^2+1 & \lambda^7+\lambda^5+\lambda^3+\lambda & \dots \\ \lambda^4 & \lambda^5+\lambda^3 & \lambda^6+\lambda^4+\lambda^2 & \lambda^7+\lambda^5+\lambda^3+\lambda & \lambda^8+\lambda^6+\lambda^4+\lambda^2+1 & \dots \\ \vdots & \vdots & \vdots & \vdots & \vdots & \end{bmatrix} .$$

The typical $(t, t')^{th}$ element is therefore

$$\sum_{i=1}^{\mathrm{Min}\,(t,t')} \lambda^{t+t'-2i} = \frac{\lambda^{|t-t'|} - \lambda^{t+t'}}{1 - \lambda^2}$$

and so we obtain for the typical element of $(g\Delta + R'R)^{-1}$:

$$\text{(A.9)} \qquad (g\Delta + R'R)^{tt'} = \frac{\lambda}{(1 - \lambda^2)\,(bc + g)}\,(\lambda^{|t-t'|} - \lambda^{t+t'}) .$$

Our second task, according to (A.3), is to postmultiply this inverse by the vector

$$(A.10)\quad gx_0e_1 - R's = \begin{bmatrix} gx_0 \\ 0 \\ 0 \\ \vdots \end{bmatrix} + \begin{bmatrix} -b & c & 0 & \dots \\ 0 & -b & c & \dots \\ 0 & 0 & -b & \dots \\ \vdots & \vdots & \vdots & \end{bmatrix} \begin{bmatrix} -cx_0 + \nu_1 + d_1 \\ \nu_2 + d_2 \\ \nu_3 + d_3 \\ \vdots \end{bmatrix}$$

$$= \begin{bmatrix} (bc+g)x_0 - b(\nu_1 + d_1) + c(\nu_2 + d_2) \\ -b(\nu_2 + d_2) + c(\nu_3 + d_3) \\ -b(\nu_3 + d_3) + c(\nu_4 + d_4) \\ \vdots \end{bmatrix} .$$

Then, after postmultiplying the inverse by this vector we obtain the optimal decision vector x^0 of which the t^{th} component is:

$$(A.11)\quad x_t^0 = \lambda^t x_0 - \frac{\lambda^t b}{bc+g}(\nu_1 + d_1)$$

$$- \frac{1}{(1-\lambda^2)(bc+g)} \sum_{i=2}^{t} \{(c-\lambda b)\lambda^{t+i} + (b-\lambda c)\lambda^{t-i+1}\} (\nu_i + d_i)$$

$$+ \frac{(1-\lambda^{2t})(c-\lambda b)}{(1-\lambda^2)(bc+g)} \sum_{i=t+1}^{\infty} \lambda^{i-t}(\nu_i + d_i) \quad ,$$

which is equivalent to (5.1) for the first component ($t = 1$).

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DISCUSSION

ISARD

First, I want to ask what you would consider to be a significant value for the median (on page 23), or the criteria which would govern what a significant value might be. Could you develop this point further? Second, I was wondering whether, in doing your research, you had considered any other concept of rational behaviour besides the one of maximizing expected utility. Suppose you had taken a more conservative approach toward the problem, say a max-min approach. What would have happened? Would there have been much difference? After all, how many political figures involved in economic planning would think in terms of maximizing expected utility?

FRISCH

Let me start by complimenting Professor THEIL for the simplicity of the model he used, and I am not saying this in a sarcastic way. I really mean that frequently we can exhibit certain fundamental properties of a problem, by using simplified examples. And of course you save a lot of time and mental effort if you use simplified examples.

Now for other aspects at Professor THEIL's paper I may perhaps be a little more critical. First of all, regarding the very principles of using rules of strategy. I really doubt whether that is a practical proposition in actual economic planning. To me it would seem much more efficient, much more practical, to use what I call and what I will speak about on Thursday — the principle of moving planning. That is to say, each year you are going to work out a completely new, say, five-year plan, taking account of the new informations you have got, what ever new slants to your preference function you would like to give and so on. Let me give you an example, suppose I am out in the woods and I am lost in the darkness of the night. I am lost; I do not know where I am. All right, what to do? Will have to wait till morning and the sunrise to find my way. Now what would be the use of sitting down and enumerating to myself the various alternatives that might happen and how I should then react to them? Suppose in the morning, when the sun comes up, I see that there is an abyss to the north. I would not go there. A river to the east; I would not try to pass that. If I discover a hill to the left I may decide to go up to the top of that hill and look around. It is little use from the practical point of view, to enumerate all the possibilities and to decide beforehand about the action I ought to take if any given alternative materialises. It is much better to wait till the morning comes and then decide on the basis of actual alternatives, what to do. That would be an illustration of my principle of moving planning. Then there is the question of this desirable rate of increase, or desired rate of increase in consumption. If I have understood Professor THEIL's presentation correctly this is taken as a datum. That is correct, isn't it? (Professor THEIL answers yes) All right. Now I can easily understand that from the pre-programming viewpoint you can impose a *lower bound* on consumption and on the rate of increase in consumption, perhaps from nutritional data, calorie contents, right vitamins and so on. But it is very difficult for me to understand that the precise size of the rate of increase of consumption can be fixed in advance.

This would be what I call the pre-programming attitude. To me the rate of increase in consumption is something that *will come out* of a programming analysis, which starts from much more basic data.

Now only one more remark which is not as important as the two first I made; that is regarding the procedure of taking the rate of saving as an instrument in the political sense. I do not see how you can take the rate of saving as a political instrument because it is not something on which the policy maker can decide. It is not one of his parameters of action. The saving rate is rather one of the consequences that will emerge from his decisions on a lot of other things. It is a consequence from his decision upon action and can only be spoken about in the post-programming sense, not in a pre-programming sense.

LEONTIEF

In presenting his method of sequential decision-making, Professor THEIL apparently commits the decision maker to use the same procedure in each stage of the process. But is this really necessary? Can one not view each step of the process as involving a decision over a limited interval of time at the end of which all the available information, including the information concerning the correctness of previous anticipations, provides a new, essentially independent basis for the next decision? The possibility of modifying the decision rule itself from one period of time to the next should not be excluded.

FISHER

I have a question related to a simulation experiment not yet performed by Professor THEIL. One of the results of his paper is that, in terms of the criterion function as a whole, the strategy approach does about 10 per cent better than what Professor THEIL calls the naive approach. I am curious to know how that 10 per cent is made up. In particular, I would like to know how the approach does in terms of the rate of growth of consumption taken separately on the one hand and in terms of the changes in the savings ratio necessitated also taken separately, on the other hand. It would seem to me to be of some interest to know whether the gain comes principally from the variable which one is ultimately interested in or whether it comes from being able to cut down on the number of changes of policy needed to get the variable of ultimate interest up as high as possible.

THEIL

Professor LEONTIEF's question is clearly the most fundamental one that has been raised in the discussion. I think it is worthwhile to make a distinction between strategies in general and the specific case of quadratic preference functions and linear constraints. As to strategies in general, it can be proved fairly easily by means of an example that if you take a decision step by step and if you do *not* combine your present decisions with those of later decisions (as is the case when a decision rule or strategy is used), you may lose in terms of expected utility. In fact, it makes also difference as to the first period decision. However, in the special situation of quadratic preference functions and linear constraints, this is different. The first-period certain equivalence theorem enables us to compute this maximizing first-period decision in a very simple way by just neglecting this difficult procedure of comparing all these possible strategies.

Professor FRISCH dislikes the idea of taking the rate of savings as an instrument. This is the result of the simplicity of the model. Making it more realistic would have forced me to go into a much more elaborate model and I am afraid that in that case I would have lost the main point, which is, as Professor LEONTIEF put it so aptly, not about the particular model or the particular preference function, but just the general idea of this kind of decision making with regard to problems of development planning. Regarding another remark made by Professor FRISCH as to the distinction between moving planning instead of a strategy, I think really that the difference is not as big as he thinks it is. Particularly this possibility of using the certainty equivalence procedure makes that effectively we are working with a moving horizon, we could call it moving planning. This can be particularly easily justified when one works with an infinite horizon.

I should also add tbat this approach of maximizing the expectation of a quadratic preference function over time subject to linear constraints is the only case for which it is possible to handle three difficulties simultaneously: uncertainty, maximization over time, and many variables. The method of « dynamic programming » breaks down when the number of variables is not extremely small. However, inequality constraints cannot be handled by my approach (Professor FRISCH suggested the use of lower bounds). On the other hand, the approach has several interesting advantages, for which I would like to refer to my *Optimal Decision Rules for Government and Industry*.

Furthermore, Professor FRISCH talked about *a priori* desired values of the quadratic preference functions. I agree with him that it would be worthwhile to have a preliminary study as to how we should really define these desires in any scientific manner. I think it is rather difficult to do and for the moment I would like to suggest that this is being solved by a process of experimentation.

Professor ISARD asked two questions. He wondered — it was page 23 — what I had expected for this relative performance of the strategy approach and I expressed more or less my dissatisfaction that 0.9 came out of it. I have no precise ideas about what is going to come out of this in general. This is a matter that will be found in future research. To take an example, right now we know rather well when a correlation coefficient is high or when it is low. But if you go back a sufficient number of decades, when this coefficient was formulated for the first time, the man who invented it certainly had no precise idea about when he should judge his coefficient high or when low. The only thing he knew was that the coefficient was bound to lie between -1 and $+1$; whether .7 was high or whether .99 was high, that certainly was not a matter that could be decided at that time. In the same way I hope I am able to give an appropriate answer after ten years or so.

Regarding expected utility to be replaced by the max-min approach, I have my doubts. I think the max-min approach is always a little difficult by the time your random variables have an infinite range. And also regarding expected utility, this has a rather firm foundation given by VON NEUMAN and MORGENSTERN; they showed that under a number of rather innocent assumptions a rational man behaves as if he maximizes expected utility. It is of course quite another affair whether these preferences can be represented in the way I do, but I think that given the large number of difficulties which we are bound to have in formulating a preference function anyhow, this is a matter of relatively minor concern.

Finally, regarding Professor FISHER, I must say that no computations have been made on the part of the loss function which is due to the instruments and the part which is due to the non-controlled variables. But the basic data are available, so the computations can be made.

FRISCH

Professor THEIL excused himself for the non-realism of the choice of instruments. He excused himself by pointing to the simplicity of the model. I should say that this is an excellent example of how a simple model can reveal very pertinent facts and conclusions. In this particularly simple model, which I complimented Professor THEIL on, I should say that the capital output ratio is a much better example of a real instrument, but THEIL took that simply as a constant that was given. That was my first remark.

Second, it seems that Professor THEIL connected the idea of moving planning with the idea of an infinite horizon. There is absolutely no connection between the two. When I speak about moving planning, I simply mean that I am not committed to any predetermined rule of strategy but simply I am evaluating the whole situation in a new and entirely free way, and that, of course, I may do even if my planning period is a short one — three, four, five years or something. So it has nothing to do with the idea of an infinite horizon.

Professor THEIL also made a remark on the MORGENSTERN theory of expected values of utility. I do not think you can justify this approach by saying that it is an expression of rational behaviour or something else pertaining to the substance matter of the problem. The whole thing here is only a formal one, and resides in the fact that you introduce an assumption which makes utility *additive*. And if you do that, if you put that up as an axiom, the you can derive a lot of consequences regarding sub-optimality. Prof. THEIL's use of the word suboptimal is really an example of what you may call persuasive definition, because optimality in this particular case is in the end precisely an expression for the idea of expected utility.

THEIL

This idea of moving planning and moving horizons can also be applied in the case of a « truncated » horizon of, say, three years. Then the decision maker is supposed to look three years ahead at the beginning of every year, and he should evaluate all things all over again. Hence there is not as much difference with Professor FRISCH's ideas as he thinks there is. Regarding sub-optimality, I define as the optimal decision the decision which maximizes the preference function, not the expectation of the preference function, subject to the constraints as they actually are. There is therefore no reason to speak about a « persuasive terminology ».

SOME OBSERVATIONS ON COUNTERCYCLICAL FISCAL POLICY AND ITS EFFECTS ON ECONOMIC GROWTH

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1. Introductory Remarks

A leading idea in « Post-Keynesian » economic thinking has been the strong emphasis on fiscal policy as a means of steering an economy towards full and efficient use of its resources. But for this purpose, it is held, the policy must be « radical » and « unorthodox », and not of the old-fashioned, balanced-budget type. The line of reasoning, stated briefly, seems to run approximately as follows: « Orthodox » fiscal policy based on a balanced budget and relatively modest economic activity on the part of the state is responsible for unemployment and waste of resources during business depressions. The task of a good fiscal policy should be to keep effective demand high enough — if necessary through very large budget deficits — in order to maintain full employment also when private investment activity is low. Even more far-reaching is the particular ideology that often seems to accompany this kind of reasoning, viz. that a government actually has fulfilled its main obligations as an economic policy-maker if it adheres to a fiscal policy as described.

This strong emphasis on fiscal measures as an instrument of economic policy raises several interesting questions. For one thing, it seems quite curious that, once possible defects of an automatic market mechanism are admitted, the explanation should simply be: « wrong fiscal policy ». The choice to pin all the blame for depressions and idle resources on « old-fashioned » budget practices alone seems rather arbitrary, in view of the many other possibilities there are to remold the economic system by means of policy decisions. Another question is whether a flexible budget policy can actually solve the relatively simple problem of maintaining full employment in a macroeconomic sense. A constraint in the form of demand for relatively stable prices could be sufficient to make the problem very difficult, even hopeless (1). Here we shall, however, not concern ourselves with these and related questions, although they are interesting enough. What we want to discuss is the following more straightforward question.

Let us accept the idea that full employment can be maintained by regulating effective demand and that this regulation can be carried out by means of a policy of deficit spending. Let us further assume that such a policy can be practiced without too serious and unwanted side effects in the form of inflation, reduced labor efficiency, or the like. And let there also be no minimizing of the tremendous improvement that such a policy could mean as against the alternative of a passive laissez faire principle. But this being recognized, there is still the important question of whether such a full employment policy would be « optimal » in any reasonable meaning of that phrase. In other words, is the goal that such a full employment policy sets itself sufficiently high to satisfy modern requirements concerning efficient use of a nation's resources? That is the question to which the rest of this paper is devoted.

(1) Cf. P.A. Samuelson and R. Solow, *Analytical Aspects of Anti-inflation Policy*, « American Economic Review », May 1960, p. 177-194.

2. Some Definitions and Assumptions

We shall carry out our analysis under the assumption of a closed economy consisting of two sub-sectors, a private sector and a public, or government, sector. We shall need the following variables in the models to be considered.

(1) $x(t)$ = total net national product

(2) $x_P(t)$ = part of x consumed by private sector

(3) $x_O(t)$ = part of x consumed by public sector (« collective consumption »)

(4) $k_P(t)$ = part of x representing private net investment

(5) $k_O(t)$ = part of x representing public net investment

(6) $k(t)$ = $k_P(t) + k_O(t)$

(7) $K(t)$ = total stock of productive capital of all kinds, in the whole economy

(8) $y(t)$ = disposable income of the private sector

(9) $m(t)$ = rate of total government deficit spending.

All variables are to be regarded as in *constant prices*. Except for K, the variables are all *flows*, i.e. they have the dimension of « per unit of time ». More information concerning the meaning ascribed to the variables is given implicitly by the following definitional relations (using hereafter the simpler notation of x, x_P, etc. instead of $x(t)$, $x_P(t)$, etc.):

(2.1) $$x = x_P + x_O + k_P + k_O$$

(2.2) $$y = x_P + k_P + m$$

(2.3) $$\frac{dK}{dt} = \dot{K} = k_P + k_O$$

We propose to work with the following simplifying assumptions concerning the structure of the economy considered.

Assumption 1. The capacity to produce of the economy is assumed to be proportional to the stock of capital, i.e. equal to aK, where 'a' is a constant over time. We shall assume that population and total labor force are constants and that the part of total labor force employed at any time is a function of the part of total capacity actually in use. The coefficient a, therefore, includes also the effect of labor input. (More specifically, if $\overline{N}$ is the total (constant) labor force, N actual employment, and x actual total output, we could assume that $N/\overline{N}$ is equal to x/aK).

Assumption 2. The propensity to consume of the private sector is given by

$$(2.4) \qquad x_p = \alpha y + \beta$$

where α and β are constants over time, and such that $0 < \alpha < 1$ and $\beta > 0$.

Assumption 3. Public consumption, x_0, is assumed to be proportional to the size of the economy as measured by its production capacity. We thus have

$$(2.5) \qquad x_0 = \gamma a K$$

where γ is a constant over time.

Regarding the coefficients a, α, β, γ and the initial value of K, we shall assume that $\alpha(1-\gamma)aK + \beta + \gamma aK < aK$, i.e. that the maximum rate of public and private consumption does not exceed capacity (cf. Assumption 4 below).

Assumption 4. The level of public investment activity is assumed to be proportional to total « desired savings » in the economy when operating at full capacity. The meaning of this needs a little further explanation. Available resources at full capacity, after allowing for public consumption x_0, is equal to $(1-\gamma)aK$. If an income corresponding to this flow of resources were at the disposal of consumers, they would want to spend $\alpha(1-\gamma)aK+\beta$ of it for x_P. The remainder, available for public and private investment activity, would be equal to $(1-\alpha)(1-\gamma)aK-\beta$ (which is assumed to be initially positive). Our « assumption 4 » can then be written as

$$(2.6) \qquad k_0 = \delta\ [(1-\alpha)(1-\gamma)aK-\beta]$$

where δ is a constant over time. Alternative levels of δ (including $\delta=0$ or $\delta=1$) may be considered.

Under the specifications describing the details of Assumption 4 the *maximum* flow of production *available for private investment* would be $(1-\delta)\ [(1-\alpha)(1-\gamma)aK-\beta]$.

More special assumptions concerning the nature of fiscal policy on the one hand, and the nature of private investment activity on the other hand will be introduced in Section 4.

The simplifying assumptions introduced above are, of course, very drastic and grossly unrealistic in many aspects. In one respect, however, they are not disturbingly nnrealistic, namely in their degree of similarity to the kind of models on which much of the theory of fiscal measures for full employment is based. If this is correct, it would seem of interest and fair enough to study the efficiency of fiscal policy under the assumptions that we have introduced.

3. Some Preliminary Conclusions Concerning the Consequences of the « Old » vs the « New » Fiscal Policy

On the basis of the — as yet incomplete — model above it is possible to illustrate the differences between the « old » and the « new » ideas of fiscal policy and to bring out some of their striking characteristics.

We shall regard private investment, k_P, as the independent and freely variable element of the model and study the consequences of variations in this part of total activity.

Let us define « old-fashioned » fiscal policy as a policy where *m is kept constantly equal to zero*. (It may perhaps also be reasonable to assume that this policy would be coupled with a state of affairs where δ, indicating the public part of investment activity, is rather small.) From (2.2) and (2.4) we then derive the now rather « threadbare » textbook relation

$$x_P = \frac{\alpha}{1-\alpha} k_P + \frac{\beta}{1-\alpha}, \qquad (m=0) \tag{3.1}$$

provided, of course, that $x = x_P + x_O + k_P + k_O \leq aK$. This « classical » relation of a neutral fiscal policy shows one property that would make good common sense to most people, viz. that when the economy is in a state of stagnation or slow progress (i.e. k_P small), then people do not feel that they can spend much on consumption either. The senseless aspect of the situation is, of course, that both x_P and k_P may be far below the level that available capacity permits.

The « new » type of fiscal policy, on the other hand, can be illustrated by requiring that $x_P + x_O + k_P + k_O = aK$, which will determine the necessary level of m for a given k_P. Using the relations of Section 2 this yields the following equations

$$(3.2) \qquad x_P = (1-\gamma)aK - k_P - k_O,$$

$$(3.3) \qquad m = \frac{(1-\alpha)(1-\gamma)}{\alpha} aK - \frac{1}{\alpha} k_P - \frac{1-\alpha}{\alpha} k_O - \frac{\beta}{\alpha},$$

$$(3.4) \qquad y = \frac{1}{\alpha}\Big((1-\gamma)aK - k_P - k_O - \beta\Big).$$

These relations of a « fiscal policy for full employment » show some interesting — not to say disturbing — aspects. First, we find that the less economic progress the society is making (i.e. the smaller k_P is), the more it spends on current consumption. Second, and this explains the high rate of consumption, the less economic progress the society is making, the more people earn! This follows from relation (3.4). Third, the less economic progress the society is making, the more people feel that they are providing for the future in the form of saving! This follows by calculating $y - x_P$, which yields

$$y - x_P = \frac{1-\alpha}{\alpha}(1-\gamma)aK - \frac{1-\alpha}{\alpha} k_P - \frac{1-\alpha}{\alpha} k_O - \frac{\beta}{\alpha}.$$

Another way of putting this last statement is to say that the slower the real economic progress of society the higher the rate $(m - k_O)$ of « unfounded paper claims » added to the wealth of the private sector.

In spite of the obvious improvement over the « old » policy in that capacity is being utilized, it would seem rather difficult to imagine that the people of an economy would have a welfare function under which the strange properties exhibited above could be an optimal choice if only the « real » constraints of the system, viz. capacity to produce, were to count.

4. Effects of Cyclical Variations in Private Investment on the Rate of Economic Growth

In order to illustrate the possible effects of variable private investment under a fiscal policy for full employment we shall now make a strict, but not entirely unrealistic, assumption about the behavior of private investment. We shall assume that it is cyclical with variable amplitude around some variable (but presumably positive) level. More specifically, we shall assume k_P to have the following time shape:

$$k_P = A(t)\ [\sin \omega t + B], \tag{4.1}$$

where $A(t)$ is some non-negative function and B a constant.

We want to propose some fairly reasonable assumptions in order to determine the factor $A(t)$ and the constant B. We shall suppose that when the desire of private investors to invest is at its peak, it is sufficiently high to exhaust whatever capacity is available for this purpose. When, on the other hand, the desire to invest is low, we shall assume that its practical lower level is zero.

The second of these assumptions will be satisfied if we put

$$B = 1. \tag{4.2}$$

The determination of $A(t)$ is a little more complicated. It depends on, among other things, to what extent it is possible to vary the government parameter m.

In point of principle the available capacity for k_P could be made large by operating with a *negative* and numerically large value of m. However, experience in recent years from many countries has shown very clearly that it is, *politically*, very difficult to get m very much below zero. In fact, if government investment k_O is of some size, it has proved next to impossible to make even $(m - k_O)$ substantially negative (except, perhaps, through the camouflage of an inflationary process yielding « unintended » higher tax returns). As a practical assumption in the present context we shall, therefore, impose the constraint that

$$(4.3) \qquad \text{Minimum value of } (m - k_O) = 0.$$

Consequently, we find that the maximum value of k_P is limited by the government policies for x_O and k_O as determined by (2.5) and (1.6), respectively, and by the lowest value of x_P that can be obtained under the constraint (4.3). This implies that

$$(4.4) \qquad k_P^{\max} = (1-\delta)\,[(1-\alpha)(1-\gamma)aK-\beta].$$

From (4.4) and (4.2) it then follows that it may be reasonable to determine $A(t)$ in (4.1) by setting

$$(4.5) \qquad A(t) = \frac{1}{2}(1-\delta)\,[(1-\alpha)(1-\gamma)aK-\beta].$$

We then obtain the following differential equation describing the development of total capital under the full-employment fiscal policy considered,

$$(4.6) \qquad k = k_P + k_O \equiv \dot{K} = \frac{1}{2}(1-\delta)\,[(1-\alpha)(1-\gamma)aK-\beta]\,[\sin \omega t + 1] + \delta\,[(1-\alpha)(1-\gamma)aK-\beta].$$

The general solution of this equation has the form

$$K = He^{h(t)} + \overline{K} \tag{4.7}$$

where H is an arbitrary constant to be determined by the initial conditions and where the function $h(t)$ and the constant $\overline{K}$ are given by

$$h(t) = -\frac{1}{2}(1-\alpha)(1-\gamma)(1-\delta)\frac{a}{\omega}\cos\omega t + (1-\alpha)(1-\gamma)a\left[\frac{1}{2}(1-\delta)+\delta\right]t, \tag{4.8}$$

$$\overline{K} = \frac{\beta}{(1-\alpha)(1-\gamma)a}. \tag{4.9}$$

It is seen that $h(t)$ is composed of a pure cyclical component and a trend element, while $\dot{h}(t)$ is a linear function of the pure cycle alone. From the constraints that we have imposed upon k_O and k_P, making them uniformly *non-negative*, it is obvious that H must be positive and that $\dot{h}(t)$ can never be negative. Inspecting our basic model equations we find that x, x_O, and k_O are linear functions of K alone, while x_P, k_P, y, and m are linear functions of K and $\dot{K}$. From these considerations we can draw the following conclusions.

Conclusion 1

Even if fiscal policy is sufficiently « radical » to maintain full use of capacity (i.e. $x = aK$) at all times, the rate of growth will depend essentially upon the extent, $(1-\delta)$, to which the initiative concerning investment is left in the hands of private investors.

It is interesting to compare the two extreme cases $\delta = 0$ and $\delta = 1$. In the first case there is no investment activity on the part of the government. The second case corresponds, formally, to a situation where the government is the sole investor. This second case need not, however, be interpreted in such a strict sense. It may well include the case where the government is not actually an investor but merely acts as planning and co-ordinating body for *all* investments in the community. (Indeed, various fiscal measures, other than a simple policy of deficit spending as discussed above, may well be useful in order to implement such a general investment program.) From (4.6) we can then draw the following conclusion.

Conclusion 2

If all investment is private ($\delta = 0$), the trend rate of growth will be *one half* of the trend rate of growth when all investment is public or publicly directed, ($\delta = 1$).

In judging these rather strong conclusions it is, of course, essential that we keep in mind the assumptions on which the results are based. Thus, to mention only one thing, we have not even touched upon the relative merits of technical efficiency of public vs. private investment activity. Nevertheless, the results obtained show a definite tendency to reduced growth because of fluctuations in private investment, a tendency which cannot be waved aside as just a peculiarity of our special model.

The model has several other rather interesting aspects.

Thus, for example, the rate of accumulation of « unfounded wealth » of the private sector will, on the average account for a part of total accumulation which is the larger the higher is the share $(1 - \delta)$ of private investment. How this is to be interpreted from a welfare point of view, could be a subject of considerable speculation.

Another obvious characteristic of the model is that the part of total product which is *consumed* will, on the average, be the higher the larger is the share of private investment. But, consumption too, will be cyclical and will not be based on a consideration of the actual resources that the consumers, taken together, have at their disposal at any time.

5. Remarks on Possible Effects of Induced Private Investment

It might be argued that the assumptions made about private investment, k_P, in the preceding section are unreasonable because no account is taken of the possibilities of induced investment. Such investment could have two main components. The first of these, relating investment to the level of total production activity, could be based on the hypothesis that when total activity is high, profit *expectations* are high, making some latent investment projects more attractive. The second main component could be some kind of acceleration effect.

Without going into details it is, nevertheless, fairly obvious how such elements of induced investment would work in the model framework considered. Roughly speaking, the effect as far as the maximum need for deficit spending (m) is concerned, would be similar to that of a combined increase in α and in δ. As for the rate of growth of K, the effect (under a fiscal policy for full employment) would be similar to an increase in δ. Of course, one cannot say, without further investigation, whether the absolute magnitude of the cyclical variations in total investment, or the absolute maximum level of m at any given time, would be larger or smaller than they would have been without such induced investment. The obvious reason for complications in this respect is the fact that the whole time path of the variables will be changed as a result of a change in the characteristic parameters of the model.

There are at least two reasons why we do not want to follow up this analysis of inducement any further in the present context. One reason is that the theoretical foundation of the idea of induced investment is in itself rather dubious. Thus, as I have tried to show elsewhere (1), it is not easy to justify a systematic tendency to induced investment by any « classical » principle of producers' behavior in maximizing profits. But the second and more important reason is that the possibility of large-scale induced investment is in fact irrelevant to the main argument to which this paper is devoted. This last statement may need a little further explanation.

The main line of argument of the « new » and « radical » principles of fiscal policy for full employment has, as far as I have understood it, been *a*) that the government should not be afraid to accept *very large* budget deficits if necessary, *b*) that in certain periods *very large* budget deficits will actually be required, and *c*) that if such a « radical » policy is adhered to, the government has done its main job as an economic policy maker.

Now, if this is a correct interpretation of the line of thinking, it would seem to me to be rather strange to argue that induced investment is so important that only modest variations in the budget deficit would be required. Such an assumption is also contradicted by facts. Experience has shown that private investment may be subject to large variations which cannot easily be explained by any initial change in effective demand for consumer goods.

The idea that a « radical » fiscal policy for full employment would not in fact need to operate with very large budget de-

(1) Cf. my book on *A Study in the Theory of Investment*. University of Chicago Press, 1960.

ficits is, I think, based on a confusion between two different behavior patterns as far as private investment is concerned. On the one hand we have the dubious effect of increased effective demand upon investment in the case where investment is assumed to be based on existing capacity and current earnings. On the other hand we have the possible stimulating effect of the *mere knowledge that the government would use very large deficits if necessary.* This latter effect would then be a result not of the fact that total activity is high but of the *conviction that total activity would not be permitted to become low.*

The second of these two investment theories has, I think, considerable strength. But it has the rather interesting implication that the *actual use* of very large budget deficits would not be the proper means of stimulating private investment, except perhaps for effects in the way of « teaching a lesson for the next depression ». It is likely, however, that definite and concrete over-all plans for the various parts of economic activity feasible within the capacity available would be more convincing as far as creating « business confidence » is concerned.

DISCUSSION

LEONTIEF

May I ask Professor HAAVELMO to answer the following question? His equation 3.3 involves a relationship between the magnitude of a deficit and that of public investment. A deficit is measured in monetary terms while investment in real. Professor HAAVELMO visualizes apparently the possibility of a situation in which large or small deficit might be combined with either large or small public investment. It would be helpful if he could interpret in somewhat more operational terms the practical meaning and implication of either one of these different possible combinations.

HAAVELMO

I refer to the definitions (2.1) and (2.2). I can derive the equations (3.2) - (3.4) from the definitions plus the consumption function (2.4) and the relation (2.5). m is simply government outlay net of taxes. Of this outlay the only real counterparts are public consumption x_0 and public investment k_0. The rest is government subsidies or expenditures on « worthless things ». m for full employment will be large if total investment, public and private, is small.

ISARD

My comment is general. The paper is excellent, but I miss the regional variable. HAAVELMO talks about investment programming and yet we know in national economic planning that a basic problem is how to allocate investment among the different regions. I am very conscious, too, of the different investment incentives required in different regions. And so forth. Thus, althoug I do not wish to be critical of HAAVELMO's excellent statement, I do hope that he and others might perhaps consider sometime in the future introducing in a simple way the regional variable in their model.

ALLAIS

I must confess I have been provoked by the conclusion of the model according to which the rate of growth is doubled under a regime of public investment. I have searched for the explanation, because, in a model which is mathematically coherent, the explanation of the conclusion must lie in the hypotheses adopted. I therefore have four remarks to make.

The first is that there is one apparently absolutely inoffensive hypothesis on page 4. That is, the capacity of the economy to produce is assumed to be proportional to the stock of capital (assumption 1). At first sight that is quite natural and in my paper I intend to stress the fact that the capital output ratio is practically constant and I intend to propose to you an explanation of this constancy.

But, from the practical constancy of the capital output ratio, it is impossible to conclude to any proportionality of real income to real capital and specifically I intend to show that, for a given population and with given technological knowledge, there is a maximum value for real national income whatever the value of real capital.

If you make the assumption that population is constant and if you don't make it explicit somewhere that there is some technical progress (and this is the case in HAAVELMO's paper), it is impossible to derive the conclusion that there can be an indefinite increase in real consumed income, as is the case in HAAVELMO's paper (relation 4.8).

My second observation is the following: in any case is the formula verified by facts? Do we observe that there is a correlation between the rate of growth and the volume of public investment? As far as I know, it is impossible to derive that conclusion from the data we have.

A third point: It seems to me it is very difficult to assume some oscillations of private investment (equation 4.1) without introducing the monetary aspects explicitly. In fact it is certain that if there is a depression it can be fought efficiently by making public investment, but under one very important condition. This is, that the public investment be combined with creation of money. If public investment is not combined with the issue of new money, the global effect for the whole economy will remain the same as it was in the past. With public investment there can be a positive multiplier effect but if this public investment is financed by a diminution of spending elsewhere, there is a negative multiplier effect elsewhere and the global effect on the whole economy is 0. Thus to have the advantage of full employment, it is not public investment which is important but the creation of new money. And if, instead of undertaking new public investment, one could imagine the state spending its money in some other way, for instance by giving subsidies to people, anything which is not investment but new expenditure, the same effects will be generated. So, in my opinion, what is very important for full employment is the overall expenditure of the economy as a whole and not public investment.

My fourth point is that public investment is also subjected to cyclical fluctuations and if equation (4.1) were valid for public investment, the results would be absolutely different.

The HAAVELMO paper is very interesting, and, for me, quite thought provoking, but in my opinion the conclusion at which it arrives derives directly from hypotheses which are very questionable.

First, theoretically, the conclusion concerning the influence of the public investment on the rate of growth depends largely on a very strong hypothesis which I personally cannot accept (assumption 1). Secondly, empirically, there is no correlation showing that the rate of growth is doubled when all investment is publicly directed. Third, for full employment, what is important is not investment but spending, and spending depends essentially on monetary policy. Finally, the fourth hypothesis (4.1) is a very questionable one to the extent that it assumes that public investment does not fluctuate at all.

FISHER

I have a question concerning the conclusion of part three of the paper. I am concerned particularly about the place in which it is found that the fiscal policy for full employment has some peculiar properties. In particular, HAAVELMO shows that such policies lower the rate of private investment and for that matter the rate of public investment. The more that is spent on current consumption and the more that is being earned evidently the more it is felt that the future will take care of itself. HAAVELMO states that it seems difficult to imagine that people would actually have a welfare function in which a policy with these properties would be an optimal choice. However, this overlooks a crucial point which is expressed in the equations in the paper. This is that capital stock does in fact appear with a positive coefficient. It is thus true that while with a given capital stock people would invest less under full employment, this would have the result of lowering the capital stock below what it would have been otherwise and therefore such reduction of investment could not continue to happen for a long period of time. It follows that if people had a welfare function which extends over a reasonably long period, they might well find that an optimum choice would be fiscal policy for full employment because they would not then find it to be true that the less progress they were making, the more they were spending. Professor HAA-

VELMO's conclusion overlooks the role of capital stock in the model and therefore overlooks the presence of long-run effects.

ALLAIS

May I add a remark? The conclusion that the rate of growth is greater if we have public investment in the model, is evident without any calculation since Professor HAAVELMO introduces two strong hypotheses. The first, as I have already stressed, is that real national income is proportional to real capital. The second one is the acceptance of an oscillation in K_p (formula 4.1), at the same time making the assumption that public investment is increasing when this oscillation is present. With such an hypothesis, it is evident without any calculation that in the end there must be a greater rate of growth when the volume of public investment is significant.

HAAVELMO

First, some general remarks. Let me be quite emphatic about my ideas on public investment in this connection. There is no kind of political preference involved, as to who is to carry out investment. Those who actually carry out investment activity might well be the private sector in all cases. That is, what I call public investment here may just be the part planned, financed or otherwise supported by the government — that doesn't change my formulae. If, as has been suggested, a bigger and better model were developed, this might come out more clearly. I am personally not particularly fond of this kind of simple models, certainly not for planning purposes. I have just presented it, as I said, to use it as a base for criticism of a way of thinking.

Now for the more specific points — ALLAIS made his points in two rounds, perhaps we could take them jointly. Given my objective for this paper I don't think the assumption about production capacity being proportional to capital is very central. But this would of course depend essentially on what alternatives are considered more realistic. I should also like to add an explanatory remark in connection with my assumption of constant population. The reason why I made this assumption here is one of simplification and at least off hand I don't think that this particular assumption is central in explaining my main results. Then there was the remark that actually in statistical figures there is not very much correlation between the rate of public investment and growth. Well, I don't know about that in detail, but I would dare say that probably we have not had so much of such policy that it would show up very much in the figures. My model illustrates more a policy that is being talked about, rather than a policy that has actually been carried out in full. Then there was the point about business cycles as an element in the model. I admit that this is not a business cycle theory and it was not meant to be. I have made the strong assumption about cyclical movements in private investment just to see how the system works if investment runs that way. I could refer to many sources where assumptions have been made about the autonomy of private investment.

Then there was the comment from Mr. FISHER. He correctly pointed out, in connection with the formula he referred to, that if you want to consider matters over time, of course the amount of capital changes gradually. Now the point of the formulas on page 7, which he referred to, is just to make some comparative studies, assuming that you could instantaneously shift the rate of private investment around, since this is the autonomous factor in my model. I just wanted to see what the instantaneous effect is and I can also, at the same time, answer another comment that comes in here. I am not saying that it is bad that you get more consumption and less investment or that it is good that you get high growth and lower consumption. I'm just saying that I doubt very much whether cyclical movements in the preferences are realistic. I will also add that one must be aware of the essential dynamic aspects of the model. It is by no means certain that you will not in the second case get much greater consumption and rate of growth of capital in the future. Therefore it is doubtful whether one can say that one alternative leads to more consumption and the other leads to more investment generally. This may shift over time. That brings me to the question of the underlying preference function in general. I have not made a statement of right or wrong here. I have just said that if one wants growth, here's how matters work. I'm not saying that it is impossible that one should want precisely the development that we get by the model under the assumption of investment being mainly private. There is one point in that connection however; as far as I see it, the only way that a social preference function — if we assume such a thing — shows up in this kind of economic model, is through the consumption function. And there is nothing there that should lead us to think that people have strange cyclical variations in their preferences. In fact, people may not even be aware of the investment cycle in my model. They have constantly full employment. Their disposable income, it is true, is higher when the economy is not growing than when it grows. But consumers, counting in money, don't see that the country is not growing and therefore they are consuming. I think that this is a fair interpretation of the meaning of the consumption function. If people have high income, they will naturally think that they can afford to consume more and still provide for the future while actually they are only piling up money which the government owes them. I think now that, even though I have not mentioned the names of all those who commented, I have answered more or less all the questions.

ALLAIS

This conclusion that the rate of growth is increased if you have public investment instead of private investment is so important from the point of view of both economic theory and policy, that I think I must insist again on three other points.

First point. If in equation 2.4 α is equal to 1, the rate of growth is 0. In this case, whether there is public investment or private investment, the rate is exactly the same. This is quite disturbing because if public investment is more efficient, one would expect that this conclusion would be independent of α. The logic of the model cannot be attacked in any way; it is absolutely correct. But it is necessary to stress the hypothesis from which the conclusions are derived. Thus, my first point is: if α is equal to 1, public investment has no role at all in increasing the rate of growth.

My second point is: HAAVELMO has said that the hypothesis that real income is proportional to capital does not play any role in the final results. But if it were assumed that real income cannot increase indefinitely as a result of increasing investment, the conclusions of the model would be absolutely different.

And, on the contrary, if it were assumed that real income is increasing proportionally to real capital, we would be admitting an hypothesis which is in contradiction with the facts.

If one assumes an indefinite increase of real national income resulting from indefinitely accumulating capital, this is equivalent that the stage of decreasing returns to capital is never reached. And this is not confirmed by observation.

Thirdly, the hypothesis of a cycle of oscillation for K_p is essential for the conclusions. HAAVELMO is completely right in saying that fluctuations of K_p have been observed in the past, and I am ready to admit equation (4.1) at least as a first approximation. But as far as public investment is concerned it has never been observed to be able to compensate for the fluctuations of private investment. In addition, we have never observed an absence of fluctuations in public investment. Thus, I could propose another model conforming with information relating to the past and show that public investment has exactly the same drawbacks as private investment for the rate of growth.

At all times, we must be very careful about the use politicians could make of such a model. They might believe that a general demonstration had been given of the superiority of public investment, ignoring the very strong and very questionable hypotheses from which this demonstration was derived.

MALINVAUD

If I have rightly understood Professor HAAVELMO, it seems to me he is comparing situations for which the degree of exogenous irregularity in the economic system is not the same. For instance, his result according to which the rate of growth would be smaller if all the investment were private than if it were public, depends on a comparison between a situation in which fluctuations in effective demand would come from private investment with a situation in which all the investment would be public *and steady*. If such is the comparison, is it quite fair? Should not we compare situations in which the degree of exogenous irregularity would be about the same? A French economist could well argue that in postwar France, fluctuations did not come much from private investment but much more from public expenditures including public investments which were at times influenced by political changes.

KOOPMANS

I have just one question. Is the statement that the fraction of investment that is private affects the rate of growth dependent on the presence of fluctuations in private investment. Or is that statement reached in a part of the paper where the fluctuation had not been introduced?

HAAVELMO

Professor MALINVAUD had a question about cycles in public investment. I am studying the effects of a certain kind of policy concerning public investment, not the effects of empirically observed public investment. Incidentally, in my model there will in general be included cycles in public investment too. KOOPMANS has asked whether the conclusions about growth depend on the assumption of cycles in private investment. The answer is « yes ».

WOLD

When reading Professor HAAVELMO's paper I was struck by the contrast between the simple and seemingly innocent assumptions on the one side, and the rather startling implications and conclusions on the other, and at the same time it puzzled me that he gives little or no comment whether his results can or cannot be reconciled with current theories. Somehow I got the feeling that Professor HAAVELMO has written his paper with tongue in cheek, and this impression was confirmed as I consulted him about his paper one of the first days of the Study Week. I should now like to ask whether I have understood his intentions correctly, namely that the paper illustrates the danger of mixing together the theories of two different regimes of economic conditions: on the one hand the serious depression around 1930, and the Keynesian theory of measures to get rid of the depression, on the other hand the modern theories of economic growth? More specifically, is it the point of the paper that the Keynesian assumptions are appropriate for a regime of unemployment and unused capacity, whereas these same assumptions lead to unrealistic and startling conclusions in a regime of full employment and full utilization of capacity? If so, how are the hypotheses underlying Professor HAAVELMO's simple model to be sorted out between the two regimes? A clarifying answer to these questions would add greatly to the appropriate understanding of Professor HAAVELMO's important model.

HAAVELMO

I am not sure that I understand which parts of my paper Professor WOLD wants me to explain. My model is similar to other elementary models frequently used as a basis for recommending certain short-run countercyclical fiscal policy measures. The purpose of my paper is to illustrate possible long-run effects of such policy measures. My main conclusion is that such countercyclical policies, though very much better than nothing, may still be quite far from what could reasonably be called an optimal policy for economic growth.

BALANCED GROWTH AND TECHNICAL PROGRESS IN A LOG-LINEAR MULTI-SECTORAL ECONOMY

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1. Introduction

In this paper we are concerned with an economy where each good may serve the capital requirements as well as the current production requirements of all the various industries. We assume that each industry has a production function of the COBB-DOUGLAS type (1). We also assume that the constant returns to scale prevail in each industry and that the marginal productivity of any factor equals the price-ratio between the factor and the product.

As for the consumer's behaviour we follow Mrs. JOAN ROBINSON and J. VON NEUMANN in assuming that only workers consume and only capitalists save. We assume that all workers

(1) Such a system may be called a WALRAS-MOORE system. See HENRY L. MOORE, *Synthetic Economics* (New York, Macmillan, 1929). A similar model has recently been examined by RADNER. See R. RADNER, *Notes on the Theory of Economic Planning*, Center of Economic Research, Training Seminar Series 2 (Athens) 1963, and *Optimal Growth in a Linear-Logarithmic Economy*, unpublished (November 1962).

are identical in the sense that each of them can only offer one unit of labour and their utility functions are identical. Furthermore, we assume that the utility function is log-linear in the quantities of the commodities consumed by the worker.

We obtain a set of output-determining equations and a set of price-determining equations. In the former, we explicitly take into account not only the current inter-industrial demand but also the investment demand due to the multi-sectoral acceleration principle; and the latter states that prices are determined so as to cover capital losses as well as the unit cost of production.

As the input coefficients (as well as the consumption of various goods per worker) depend on prices, the mechanism of determination of outputs is influenced by the price-valuation mechanism, though the latter is independent of the former by virtue of the prevalence of the constant returns to scale. We can show that the characteristic roots of the whole system appear in pairs with their reciprocals. This leads to a Turnpike Theorem which asserts that there is a long-run tendency for the optimal path of economic growth to approximate to the path of steady balanced growth at the maximal rate.

In the second section of this paper, we examine various effects of technological changes, neutral and biased, on the long-run equilibrium prices and on the long-run output configuration to which efficient paths converge. We shall also deal with effects of technological changes on the allocation of labour among various industries.

2. A Turnpike Theorem

1. Let us consider an economy consisting of n industries, whose products may serve capital requirements as well as current production requirements of various industries. Let

$x_{i,t}$ ($i=1, \ldots, n$) be the volume of output of good i (by industry i) in period t, and $x_{ji,t}$ ($i=1, \ldots, n$; $j=0, 1, \ldots, n$) the volume of current input of good j into industry i in period t, where the 0-th good stands for the sole primary factor of production, 'labour'. Finally, let $s_{ji,t}$ (i, $j=1, \ldots, n$) be the volume of capital input of good j into industry i in period t.

It is assumed that the production function of each industry is of the COBB-DOUGLAS type, i.e.

$$(1) \qquad x_{i,t}=F_i \prod_{j=0}^{n} x_{ji,t}{}^{a_{ji}} \prod_{j=1}^{n} s_{ji,t}{}^{b_{ji}} \qquad (i=1, \ldots, n) ,$$

where F_i, a_{ji}, and b_{ji} are all constant and non-negative. It is also assumed that the constant returns to scale prevail, so that

$$(2) \qquad \sum_{j=0}^{n} a_{ji} + \sum_{j=1}^{n} b_{ji} = 1 ,$$

or

$$(3) \qquad 1 = F_i \prod_{j=0}^{n} \left(\frac{x_{ji,t}}{x_{i,t}}\right)^{a_{ji}} \prod_{j=1}^{n} \left(\frac{s_{ji,t}}{x_{i,t}}\right)^{b_{ji}} .$$

In each industry, unit cost is to be minimized; furthermore, it equals the price of output when competitive equilibrium prevails. It is well-known that the marginal conditions may be put in the form:

$$(4) \qquad \begin{aligned} \frac{p_{j,t}\, x_{ji,t}}{p_{i,t}\, x_{i,t}} &= a_{ji} \qquad (j=0, 1, \ldots, n) , \\ \frac{q_{k,t}\, s_{ki,t}}{p_{i,t}\, x_{i,t}} &= b_{ki} \qquad (k=1, \ldots, n) , \end{aligned}$$

where $p_{j,t}$ is the price of good j in period t and $q_{k,t}$ the price of capital-service k in period t. Substituting for $x_{ji,t}/x_{i,t}$ and $s_{ki,t}/x_{i,t}$ from (4) and taking logs, we may put (3) in the form

$$(5) \qquad \log p_{i,t} - \sum_{j=1}^{n} a_{ji} \log p_{j,t} - \sum_{j=1}^{n} b_{ji} \log q_{j,t} - a_{oi} \log p_{o,t} + G_i = 0 \quad ,$$

where

$$(6) \qquad G_i = \log F_i + \sum_{j=0}^{n} a_{ji} \log a_{ji} + \sum_{j=1}^{n} b_{ji} \log b_{ji} \quad .$$

Let us now consider a person who has a given sum of money M available for expenditure. If he lends that amount to someone for one period at the prevailing rate of interest r, he will enter the next period with amount $(1+r)M$. Alternatively, he may spend M on goods; if he spends it exclusively on a capital good k in period t, he obtains $M/p_{k,t}$ units of that good. By letting them out hire, he receives income by the amount $q_{k,t}(M/p_{k,t})$ in period t, which will grow to $(1+r)q_{k,t}(M/p_{k,t})$ at the beginning of period $t+1$. Although the capital goods he owns will be worn at a certain rate (say) d_k in the process of production, he will still own $(1-d_k)(M/p_{k,t})$ units of good k at the beginning of period $t+1$, which will be evaluated as $(1-d_k)(M/p_{k,t})p_{k,t+1}$ at the price in period $t+1$. In equilibrium neither option can be advantageous over the other, so that

$$(1+r) M = (1+r) q_{k,t} (M/p_{k,t}) + (1-d_k) (M/p_{k,t}) p_{k,t+1} \quad .$$

By dividing both sides by $M/p_{k,t}$, this may be put in a simple form

$$(7) \qquad (1+r) p_{k,t} = (1+r) q_{k,t} + (1-d_k) p_{k,t+1} \quad .$$

In the state of the long-run equilibrium where all prices remain constant over time, we have from (5) and (7)

$$\log p_i - \sum_{j=1}^{n} a_{ji} \log p_j - \sum_{j=1}^{n} b_{ji} \log q_j - a_{oi} \log p_o = -G_i \tag{8}$$

$$q_k = \frac{r + d_k}{1 + r} p_k \quad . \tag{9}$$

Substituting for q_k from (9), and subtracting (2) multiplied by $\log p_o$, we may rewrite (8) in the form

$$\log v_i - \sum_{j=1}^{n} (a_{ji} + b_{ji}) \log v_j = \sum_{j=1}^{n} b_{ji} \log \frac{r + d_j}{1 + r} - G_i \quad (i = 1, \ldots, n) \tag{10}$$

where v_i is the price of good i in terms of labour, i.e. $v_i = p_i/p_o$. Equations (10) allow us to determine the long-run equilibrium wage price, $v_1, \ldots, v_n$, once the rate of interest r is given.

Let us now turn to the output-determination side of our system. At the beginning of period $t+1$, industry i has good j of the amount $(1 - d_j)s_{ji,t}$, so that the total amount of good j available in the economy is $\sum_{i=1}^{n} (1 - d_j)s_{ji,t}$. This, together with the output of good j in period $t+1$, is distributed among producers and consumers.

As for consumers, we assume that capitalists do not consume and workers who are identical and can offer only one unit of labour spend their income upon various commodities without making any savings. Let y_{it} $(i = 1, \ldots, n)$ be the consumption of good i per worker in period t. These amounts will be determined so as to maximize the utility function

$$u(y_{1t}, \ldots, y_{nt}) = U y_{1t}^{h_1} y_{2t}^{h_2} \ldots y_{nt}^{h_n} \qquad (h_i \geqq 0, \; U > 0)$$

subject to the budget constraint

$$p_{ot} = \sum_{i=1}^{n} y_{it} p_{it} .$$

The conventional procedure of maximization leads to

$$\frac{h_i}{h_1} \frac{y_{1t}}{y_{it}} = \frac{p_{it}}{p_{1t}} \qquad (i = 1, \ldots, n) .$$

These conditions, together with the budget equation, yield

$$(11) \qquad y_{it} = g_i \frac{p_{ot}}{p_{it}} \qquad (i = 1, \ldots, n) ,$$

where $g_i = h_i / \sum_{j=1}^{n} h_j$.

As the industrial demand for good j in period $t+1$ is $\sum_{i=1}^{n} (x_{ji,t+1} + s_{ji,t+1})$ and workers' consumption of good j is $y_{j,t+1} \sum_{i=1}^{n} x_{oi,t+1}$, the supply-demand balance of good j is established when

$$(12) \qquad \sum_{i=1}^{n} (1 - d_j) s_{ji,t} + x_{j,t+1} = \sum_{i=1}^{n} (x_{ji,t+1} + s_{ji,t+1}) + \\ + y_{j,t+1} \sum_{i=1}^{n} x_{oi,t+1} .$$

Let us now write:

$$x_1 = (x_{1t}, \ldots, x_{nt}), \qquad d = \begin{pmatrix} d_1 & & & o \\ & \cdot & & \\ & & \cdot & \\ o & & & d_n \end{pmatrix} ,$$

$$A_t = \begin{pmatrix} a_{11}\, p_{1t}/p_{1t} & \ldots & a_{n1}\, p_{1t}/p_{nt} \\ \cdots & \cdots & \cdots \\ a_{1n}\, p_{nt}/p_{1t} & \ldots & a_{nn}\, p_{nt}/p_{nt} \end{pmatrix} ,$$

$$B_t = \begin{pmatrix} b_{11}\, p_{1t}/q_{1t} & \ldots & b_{n1}\, p_{1t}/q_{nt} \\ \cdots & \cdots & \cdots \\ b_{1n}\, p_{nt}/q_{1t} & \ldots & b_{nn}\, p_{nt}/q_{nt} \end{pmatrix} ,$$

$$C_t = \begin{pmatrix} c_{11}\, p_{1t}/p_{1t} & \ldots & c_{n1}\, p_{1t}/p_{nt} \\ \cdots & \cdots & \cdots \\ c_{1n}\, p_{nt}/p_{1t} & \ldots & c_{nn}\, p_{nt}/p_{nt} \end{pmatrix} ,$$

where $c_{ij} = a_{oj} g_i$. Taking (4) and (11) into account, we have from (12)

$$(13) \qquad x_t\, B_t\, (I - d) + x_{t+1} = x_{t+1}\, A_{t+1} + x_{t+1}\, B_{t+1} + x_{t+1}\, C_{t+1} \; .$$

When long-run equilibrium prices prevail, it is shown that the dynamic input-output system (13) is reduced to:

$$(14) \qquad x_t\, B(I - d) + x_{t+1}(I - A - B - C) = o,$$

whose characteristic equation is

$$(15) \qquad |B(I - d) + \mu\, (I - A - B - C)| = o,$$

where A, B, and C are matrices A_t, B_t, and C_t evaluated at $p_{it} = p_i$ ($i = 0, 1, \ldots, n$). In view of (2), (9) and $\sum_{i=1}^{n} g_i = 1$ we find that one of the characteristic numbers $\mu_1, \ldots, \mu_n$ is $1 + r$. We can also show that the eigen-vector $\bar{x}$ associated with $1 + r$ is non-negative. It is clear that we have

$$(16) \qquad \bar{x}\, B(I - d) + (1 + r)\bar{x}(I - A - B - C) = 0.$$

Thus (14) has a particular solution $(1 + r)^t \bar{x}$ which is referred to as a balanced growth solution. We also refer to a state fulfilling (8) (9), and (16) as a state of balanced growth.

2. So far we have treated the rate of interest as a given constant and have shown that to any assigned value of it there corresponds a state of balanced growth. It is impossible, however, for the rate of growth of outputs to exceed the rate of growth of the working population for a long time, because the scarcity of labour will sooner or later emerge. In the contrary case where the labour force is increasing at a rate higher than the rate of growth of outputs, the ratio of the number of unemployed to the number of employed workers continues to rise. In the following, therefore, we are concerned with finding a rate of balanced growth at which the growth of outputs is in harmony with that of the labour force.

We begin with examining the effects of a change in the rate of interest on the long-run equilibrium prices. Differentiating (10) with respect to r, we get

$$H = (I - a - b)\frac{d \log v}{dr} \quad ,$$

where

$$H = \begin{pmatrix} \sum_{j=1}^{n} b_{ji} \frac{1 - d_j}{(r + d_j)(1 + r)} \\ \vdots \\ \sum_{j=1}^{n} b_{jn} \frac{1 - d_j}{(r + d_j)(1 + r)} \end{pmatrix}, \quad \frac{d \log v}{dr} = \begin{pmatrix} \frac{d \log v_1}{dr} \\ \vdots \\ \frac{d \log v_n}{dr} \end{pmatrix},$$

$$a = \begin{pmatrix} a_{11} \ldots a_{n1} \\ \ldots\ldots\ldots \\ a_{1n} \ldots a_{nn} \end{pmatrix}, \qquad b = \begin{pmatrix} b_{11} \ldots b_{n1} \\ \ldots\ldots\ldots \\ b_{1n} \ldots b_{nn} \end{pmatrix}.$$

It is seen that $a+b$ is a non-negative matrix whose column sums are less than unity; hence $(I - a - b)^{-1} \geq 0$. As $d_j < 1$, $H > 0$. Therefore, it is at once seen that

$$\frac{d \log v}{dr} > 0.^{(2)}$$

In words, all the long-run equilibrium prices increase when the interest rises.

When the long-run equilibrium prices prevail, the consumption of good i (per worker) will be g_i/v_i (see (11)). As $g_i \geq 0$ for all i and $g_i > 0$ for at least one i, we at once see from the inequality above that an increase in the interest rate gives

(2) Furthermore, when the capital-labour ratios of all industries are equal to each other, i.e. when the Marxian composition of capital prevails, then g is proportionate to the column vector $[a_{01}, \ldots, a_{0n}]$, so that

$$\frac{d \log v_1}{d r} = \frac{d \log v_2}{d r} = \ldots = \frac{d \log v_n}{d r},$$

that is, all the prices in terms of labour increase proportionately.

rise to a decrease in the consumption of at least one good with no increase in the consumption of any other good; that is,

$$\frac{d\gamma}{dr} \leq 0,^{(3)} \tag{17}$$

where

$$\gamma = [g_1/v_1, g_2/v_2, \ldots, g_n/v_n] .$$

Let us now assume that the working population grows at a rate depending upon the long-run equilibrium consumption γ: The rate of growth of the labour force ρ is negative for very low level of γ, 0 for the subsistence level of γ, and then increases with the rise in γ until ρ reaches a certain maximum, after which ρ will decrease gradually but will increase again when γ reaches the level of an affluent society (4).

We are now in a position to be able to fix the rate of interest (5). Measure the rate of growth of the labour force and the rate of balanced growth of outputs along the vertical axis, and the rate of interest along the horizontal axis. Considering (17), we find that our assumption on the labour-force-growth-rate function implies the curve $\rho\rho'$ in Figure 1. On the other hand, as the rate of balanced growth of outputs equals the rate of interest, the relation between them is simply expressed by the 45° line. It is obvious that, to sustain the demand-supply balance of labour, the condition that the rate of balanced growth of outputs equal the growth rate of the labour force is to be fulfilled. It is seen from Figure 1*a* and 1*b* that there are at least one and at most three equilibria. The greatest equilib-

(3) Let X be a vector; $X \leq 0$ (or $X \geq 0$) means that all components of X are non-positive (or non-negative) and at least one of them is strictly negative (or strictly positive).

(4) Professor S. C. TSIANG makes a similar assumption in his analysis of the Rostovian stages. See S. C. TSIANG, *A Model of Economic Growth in Rostovian Stages*, « Econometrica », XXXII (1964), pp. 619-48.

(5) A similar argument is found in my *Equilibrium, Stability and Growth*, Ch. III (Oxford, Clarendon Press, 1964).

rium rate of interest, r^o, gives the maximum rate at which all industry can grow in balance without labour shortages. We may, therefore, refer to r^o as the VON NEUMANN rate of interest. The long-run equilibrium prices v^o_i corresponding to r^o are referred to as the VON NEUMANN normalized prices.

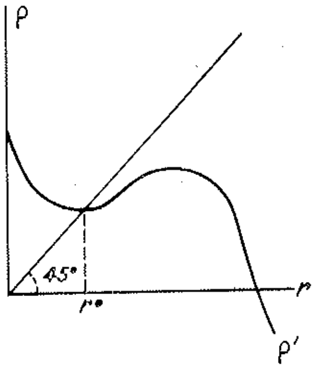

FIGURE 1-*a*

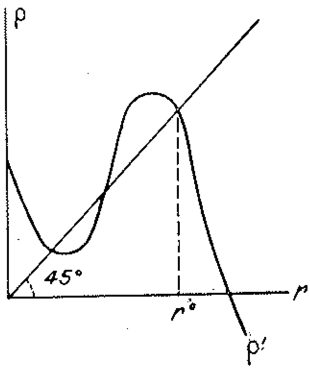

FIGURE 1-*b*

3. In the following we fix r at the VON NEUMANN rate r^o. We assume that wages are paid at the rate such that it enables the workers, if they chose, to buy the same amounts of all goods as those which they would buy at the long-run equilibrium prices, $v_1^o, \ldots, v_n^o$ (corresponding to r^o); that is, the money-wage rate p_{ot} is adjusted so as to maintain the real-wage rate at the VON NEUMANN state. We have

$$p_{ot} = \sum_{i=1}^{n} (g_i/v_i^o)\, p_{it} \quad . \tag{18}$$

Furthermore, we assume that the workers grow at the VON NEUMANN rate throughout the period during which the real-wages are kept at the VON NEUMANN level.

Let us denote the VON NEUMANN non-normalized prices (i.e. the long-run equilibrium non-normalized prices corresponding to the VON NEUMANN rate of interest) by $p_o^o, p_1^o, \ldots, p_n^o, q_1^o, \ldots, q_n^o$. Equation (5) may be linearized in the neighbourhood of the VON NEUMANN prices in the following way.

We get from (7) and (9)

$$\Delta q_{k,t} = \Delta p_{k,t} - \frac{1 - d_k}{1 + r} \Delta p_{k,t+1} \quad . \tag{19}$$

where $\Delta q_{k,t} = q_{k,t} - q_k^o$, and $\Delta p_{k,t} = p_{k,t} - p_k^o$. Expanding the left-hand side of (5) in a TAYLOR series and neglecting higher-power terms, we have by virtue of (19)

$$\bar{p}_{i,t} - \sum_{j=1}^{n} a_{ji} \bar{p}_{j,t} - (1+r^o) \sum_{j=1}^{n} \left(\frac{1}{r^o + d_j} b_{ji} \right) \bar{p}_{j,t} + \\ + \sum_{j=1}^{n} \left(\frac{1 - d_j}{r^o + d_j} b_{ji} \right) \bar{p}_{j,t+1} - a_{oi} \bar{p}_{ot} = 0 \ , \tag{20}$$

where $\bar{p}_{i,t} = \Delta p_{i,t} / p_i^o$. We also have from (18)

$$\bar{p}_{ot} = \sum_{i=1}^{n} g_i \bar{p}_{i,t} \quad . \tag{21}$$

Substituting for $\bar{p}_{o,t}$ from this and writing (20) in matrix form, we get

$$[I - a - (1 + r^o) b e^{-1} - c] \ \bar{p}_t + b(I - d) e^{-1} \bar{p}_{t+1} = 0 \tag{22}$$

where

$$c = \begin{pmatrix} a_{o1} g_1 & \dots & a_{o1} g_n \\ \dots & \dots & \dots \\ a_{on} g_1 & \dots & a_{on} g_n \end{pmatrix}, \quad \overline{p}_t = \begin{pmatrix} \overline{p}_{1,t} \\ \vdots \\ \overline{p}_{n,t} \end{pmatrix},$$

$$d = \begin{pmatrix} d_1 & & \text{o} \\ & \ddots & \\ \text{o} & & d_n \end{pmatrix}, \quad e = \begin{pmatrix} r^o + d_1 & & \text{o} \\ & \ddots & \\ \text{o} & & r^o + d_n \end{pmatrix}.$$

Next define diagonal matrices P^o, P_t and $\overline{P}_t$ as:

$$P^o = \begin{pmatrix} p_1{}^o & & \text{o} \\ & \ddots & \\ \text{o} & & p_n{}^o \end{pmatrix}, \quad P_t = \begin{pmatrix} p_{1t} & & \text{o} \\ & \ddots & \\ \text{o} & & p_{nt} \end{pmatrix}, \quad \overline{P}_t = \begin{pmatrix} \overline{p}_{1t} & & \text{o} \\ & \ddots & \\ \text{o} & & \overline{p}_{nt} \end{pmatrix}.$$

Similarly, let Q^o, Q_t, and $\overline{Q}_t$ be diagonal matrices with diagonal elements $q_i{}^o$, q_{it}, and $\overline{q}_{it}$ respectively, where $\overline{q}_{it} = \Delta q_{it}/q_i{}^o$.

In view of (7) and (9), we have

$$\text{(23)} \qquad Q^o = (1 + r^o)^{-1} e \ P^o,$$

$$\text{(24)} \qquad \overline{Q}_t = (1 + r^o) e^{-1} \overline{P}_t - (I - d) e^{-1} \overline{P}_{t+1}.$$

We can at once verify the following relations:

$$\text{(25)} \qquad \begin{aligned} A_t &= P_t \ a \ P_t{}^{-1} \cong A^o + \overline{P}_t A^o - A^o \overline{P}_t, \\ B_t &= P_t \ b \ Q_t{}^{-1} \cong B^o + \overline{P}_t B^o - B^o \overline{Q}_t, \\ C_t &= P_t \ c \ P_t{}^{-1} \cong C^o + \overline{P}_t C^o - C^o \overline{P}_t, \end{aligned}$$

where

$$(26)\qquad A^o = P^o\, a(P^o)^{-1}\ ,\ B^o = P^o\, b(Q^o)^{-1}\ ,\ C^o = P^o\, c(P^o)^{-1}\ .$$

Let us denote the balanced growth solution to (14) corresponding to the VON NEUMANN rate of interest by $(1+r^o)^t\bar{x}^o$; then $\bar{x}^o$ is the eigen-vector $\bar{x}$ of (15) evaluated at $r = r^o$, $P_t = P^o$ and $Q_t = Q^o$. Define a vector z_t as

$$(27)\qquad z_t = (1+r^o)^{-t}x_t - \bar{x}^o.$$

We may now put (13) in the form:

$$(28)\qquad (\bar{x}^o + z_t)B_t(I - d) + \\ + (1+r^o)(x^o + z_{t+1})\,(I - A_{t+1} - B_{t+1} - C_{t+1}) = 0.$$

Substitute for A_t, B_t, and C_t from (25), and neglect higher-power terms such as $z_t\bar{P}_tA^o$, $z_tB^o\bar{Q}^t$, etc.; in view of (16), and (24), we may linearize (28) as:

$$(29)\qquad (I - d)\,(B^o)'z_t' + (1+r^o)[I - (A^o + B^o + C^o)']\ z'_{t+1} + \\ + D^o\bar{p}_t + E^o\bar{p}_{t+1} + F^o\bar{p}_{t+2} = 0,$$

where D^o, E^o, and F^o are some $n \times n$ matrices whose elements are independent of z and $\bar{p}$, and a prime applied to a vector (or a matrix) denotes the transposition of that vector (or that matrix).

Equations (22) and (29) describe movements of prices and outputs in the neighbourhood of the VON NEUMANN equilibrium. The characteristic equation of the whole system is:

(30)

$$\begin{vmatrix} \lambda(b(\mathrm{I}-d)e^{-1}+(\mathrm{I}-a-(1+r^o)be^{-1}-c) & 0 \\ \lambda^2\mathrm{F}^o+\lambda\mathrm{E}^o+\mathrm{D}^o & \lambda(1+r^o)[\mathrm{I}-(\mathrm{A}^o+\mathrm{B}^o+\mathrm{C}^o)']+(\mathrm{I}-d)(\mathrm{B}^o)' \end{vmatrix}$$

$= 0.$

It is clear that $\lambda^2\mathrm{F}^o+\lambda\mathrm{E}^o+\mathrm{D}^o$ has no effect on the determination of the characteristic numbers. It follows that n roots, $\lambda_1, \ldots, \lambda_n$ of (30) equal n roots, $\nu_1, \ldots, \nu_n$ of the characteristic equation of the price system (22),

$$(31) \qquad |\nu b(\mathrm{I}-d)e^{-1}+(\mathrm{I}-a-(1+r^o)be^{-1}-c)|=0.$$

and the other n roots, $\lambda_{n+1}, \ldots, \lambda_{2n}$ of (30) equal the roots, $\mu_1, \ldots, \mu_n$, of

$$|\mu[\mathrm{I}-(\mathrm{A}^o+\mathrm{B}^o+\mathrm{C}^o)']+(\mathrm{I}-d)(\mathrm{B}^o)'|=0$$

divided by $(1+r^o)$, i.e. $\lambda_{n+i}=\mu_i/(1+r^o)$. As one of μ_i's is $1+r^o$, the λ corresponding to it, say λ_{n+1}, is unity. Furthermore, by virtue of (23) and (26), we may write

$$\mu(\mathrm{I}-\mathrm{A}^o-\mathrm{B}^o-\mathrm{C}^o)+\mathrm{B}^o(1-d)$$
$$=\mathrm{P}^o\left[\frac{\mu}{1+r^o}(\mathrm{I}-a-(1+r^o)be^{-1}-c)+be^{-1}(\mathrm{I}-d)\right](\mathrm{P}^o)^{-1}.$$

It is clear that this, together with (31), yields $\frac{\mu_i}{1+r^o}=\frac{1}{\nu_i}$ $(i=1, \ldots, n)$; hence

$$\lambda_{n+i} = \frac{1}{\lambda_i} \qquad (i = 1, \ldots, n).$$

That is, the characteristic numbers of the whole system appear in pairs with their reciprocals.

We now make the following assumption which is the joker allowing us to avoid cyclic exceptions to the Turnpike Theorem; that is, *all characteristic numbers* λ_i *other than* $\lambda_1 = \lambda_{n+1}$ *have absolute value different from unity*. We also assume, for simplicity, that 1 is the sole characteristic number which is multiple. It can be shown that the solutions to (22) and (29) may be written in the following forms:

$$(32) \quad \begin{aligned} \bar{p}_t &= \alpha_1\pi_1 + \alpha_2\pi_2\lambda_2{}^t + \ldots + \alpha_n\pi_n\lambda_n{}^t, \\ z_t' &= [I + D + \ldots + D^t]\ \alpha_1\xi_1 + \alpha_2\xi_2\lambda^t{}_2 + \ldots + \alpha_n\xi_n\lambda^t{}_n + \\ &\quad + \beta_1\xi_{n+1} + \beta_2\xi_{n+2}\lambda^t{}_{n+2} + \ldots + \beta_n\xi_{2n}\lambda^t{}_{2n}, \end{aligned}$$

where

$$D = (1 + r^o)^{-1}[I - (A^o + B^o + C^o)']^{-1}(I - d)\ (B^o)',$$

and π_i $(i = 1, \ldots, n)$ and ξ_i $(i = 1, .., 2n)$ are n-dimensional column vectors; in particular, ξ_{n+i} is the eigen-vector of D associated with λ_{n+i}. Scalars, $\alpha_1, \ldots, \alpha_n, \beta_1, \ldots, \beta_n$, can be determined by $2n$ equations:

$$(33) \quad \begin{aligned} z_0' &= \alpha_1\xi_1 + \alpha_2\xi_2 + \ldots + \alpha_n\xi_n + \beta_1\xi_{n+1} + \beta_2\xi_{n+2} + \ldots + \beta_n\xi_{2n} \\ z_T' &= [I + D + \ldots + D^T]\alpha_1\xi_1 + \alpha_2\xi_2\lambda_2^T + \ldots + \alpha_n\xi_n\lambda_n^T + \\ &\quad + \beta_1\xi_{n+1} + \beta_2\xi_{n+2}\lambda^T_{n+2} + \ldots + \beta_n\xi_{2n}\lambda^T_{2n}. \end{aligned}$$

Let L stand for the number of workers available in period o. We have assumed that throughout the whole process the real-wages are maintained at the VON NEUMANN level, so that the labour force grows at the VON NEUMANN rate. The labour force in period t is, therefore, given by $(i+r^o)^t$L. On the other hand, we at once find from (4) that the demand for labour in period t is given by $\sum_{i=1}^{n} a_{oi} \frac{p_{it}}{p_{ot}} x_{it}$. Hence the deficiency of labour does not emerge when

$$(34) \qquad \sum_{i=1}^{n} a_{oi} \frac{p_{it}}{p_{ot}} x_{it} \leqq (1+r^o)^t \mathrm{L}$$

hold for all t.

It is obvious that x_o (i.e. x_t when $t=0$) is historically given. We have from (27)

$$z_0 = x_o - \bar{x}^o .$$

Let x^* (>0) be the vector of output configuration at the terminus which is prescribed by the growth programme. A path $(x_o, x_1, \ldots, x_\mathrm{T})$ which maximizes η such that

$$(35) \qquad \eta\, x^* = (1+r^o)^{-\mathrm{T}} x_\mathrm{T}$$

is said optimal.

In view of (18), we have from (34)

$$(36) \qquad \sum_i \frac{a_{oi}}{\gamma_i} \frac{\gamma_i p_{i\mathrm{T}}}{\sum_i \gamma_i p_{i\mathrm{T}}} \frac{x_{i\mathrm{T}}}{(1+r^o)^\mathrm{T}} + \sum_j a_{oj} \frac{p_{j\mathrm{T}}}{\sum_i \gamma_i p_{i\mathrm{T}}} \frac{x_{j\mathrm{T}}}{(1+r^o)^\mathrm{T}} \leqq \mathrm{L},$$

where $\gamma_i = g_i / v_i^o$, and the i-summation takes place over all goods i with $g_i > 0$ and the j-summation over all goods j with $g_j = 0$. It follows from (35) and (36) that η is bounded and hence $z_T = \eta x^* - \bar{x}^o$ is also bounded.

When T becomes large with x_o and x^* remaining unchanged, it is seen from (33) that those α_i which are associated with $|\lambda_i| > 1$ and those β_j with $|\lambda_{n+j}| > 1$, as well as α_1 are shown to become small, because the absolute values of λ_j^T, λ_{n+j}^T, and $I + D + \ldots + D^T$ are shown to become very large, but z_T is bounded. All the other α_i and β_j are not very small, but those λ_i and λ_{n+j} which are associated with them have absolute values less than unity. It can now be seen that when the programming period T is sufficiently long, z'_t well approximates $\beta_1 \xi_{n+1}$ most of the period T. From the definitions of ξ_{n+1} and $(\bar{x}^o)'$ it can be shown that they are proportionate to each other, i.e. $\xi_{n+1} = \sigma (\bar{x}^o)'$. By virtue of (27) we finally find that in the very long-run programme outputs of industries almost always grow like $x_t \cong (1 + r^o)^t (\beta_1 \sigma + 1)\bar{x}^o$. The turnpike property is thus established.

3. Technical Change

1. In this section we concentrate our attention on the state of the von Neumann balanced growth, the convergence to which has been discussed in the preceding section. We shall be concerned with comparative statical analysis of effects on the von Neumann equilibrium of technological changes (or changes in the parameters of the production functions) which may be arranged in the following classes: (1) an increase in F_i, all other parameters remaining unchanged, (2) a substitution of b_{ki} for a_{oi} accompanying an increase in F_i, and (3) a converse substitution between b_{ki} and a_{oi} associated with an increase in F_i. If there were no changes in prices and the wage rate,

a technical change of the class (1) would not give rise to any effect on the capital-labour ratio of industry i [6], while that of the class (2) (or (3)) would increase (or decrease) the capital-labour ratio of industry i. We may, therefore, call technical changes of these three classes neutral, labour-saving, and capital-saving respectively.

It is true that we may also conceive of a technical change such that a substitution is made between a_{ki} and a_{oi}. We have a technical change of the material-using type if the substitution is in favour of a_{ki}, and of the material-saving type if it is in favour of a_{oi}. To these cases the following analysis of the biased invention can be applied *mutatis mutandis*.

2. Let us first be concerned with a technological change of the neutral type. Suppose F_i to vary, other parameters and the rate of interest remaining unchanged. We have from (10)

$$\frac{d \log v}{d F_i} = -(I - a - b)^{-1} J_i , \tag{37}$$

where $d \log v / dF_i$ is a column vector with components $d \log v_j / dF_i$ $(j=1, \ldots, n)$ and J_i is a column vector whose i-th component is $1/F_i$, while all other components are zero. Since $a+b$ is a non-negative matrix whose row sums are less than 1, it is shown [7] that $(I-a-b)^{-1} > 0$ and that the i-th element of the main diagonal of $(I-a-b)^{-1}$ is greater than any off-

(6) We see from (4) that the capital labour ratio of industry i (at the constant prices $p_o^o, q_1^o, \ldots, q_n^o$) is

$$\frac{\sum_k q_k^o \, s_{ki,t}}{p_o^o \, x_{oi,t}} = \frac{\sum_k b_{ki} \, q_k^o / q_{kt}}{a_{oi} \, p_o^o / p_{ot}} .$$

(7) METZLER L. A., *A Multiple-Country Theory of Income Transfers*, « Journal of Political Economy », LIX (February, 1951), p. 21.

diagonal element of the i-th column of $(\mathrm{I} - a - b)^{-1}$. Accordingly,

$$(38) \qquad \frac{d\log v_i}{d\mathrm{F}_i} < \frac{d\log v_j}{d\mathrm{F}_i} \leqq 0 \qquad j \neq i \ .$$

In words, if industry i makes a technical improvement of the neutral type, the long-run equilibrium prices of good i decrease proportionately more than the other prices.

Clearly (38) implies $d\gamma/d\mathrm{F}_i \geq 0$; therefore, the curve $\rho\rho'$ in Figure 1 shifts upward in the diminishing phases and downward in the increasing phase (see Figure 2*a*). The intersection of the new curve $\rho''\ \rho'''$ after the technical change with the 45° line results in a new von Neumann rate of interest r^1. Usually we have $r^1 > r^0$. But as in the case illustrated in Figure 2*b* a technological change of the neutral type may perversely bring forth a decrease in the von Neumann rate of growth. We shall refer to the increasing part $\omega\omega'$ of the curve $\rho\rho'$ as the perverse part and to the other part as the normal part. (See Figures 2*a* and 2*b*). In any case, however, we have $d\gamma/d\mathrm{F}_i \geq 0$, so that

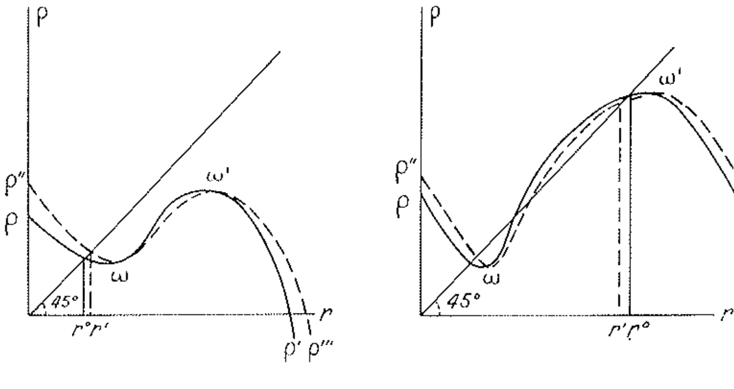

Figure 2-*a* Figure 2-*b*

the technological improvement gives rise to an increase in the real-wages.

The VON NEUMANN output configurations $\bar{x}^o$ and $\bar{x}^1$ before and after the technical change are determined by

$$\bar{x}^o B^o(I-d)+(1+r^o)\bar{x}^o(I-A^o-B^o-C^o)=0.$$

and

$$\bar{x}^1 B^1\ (I-d)+(1+r^1)\bar{x}^1\ (I-A^1-B^1-C^1)=0$$

respectively. Taking the definitions of A, B, and C into account, we have

$$(39)\qquad \begin{aligned} &(1+r^o)\bar{x}^o P^o\ [I-a-b-c]\ (P^o)^{-1}=0\ ,\\ &(1+r^1)\bar{x}^1 P^1\ [I-a-b-c]\ (P^1)^{-1}=0. \end{aligned}$$

Hence $\bar{x}^o P^o$ and $\bar{x}^1 P^1$ are proportionate to each other. Since (38) implies $p_i^1/p_i^o < p_j^1/p_j^o$, we obtain

$$\bar{x}_i^1/\bar{x}_i^o > \bar{x}_j^1/\bar{x}_j^o \qquad j \neq i\ .$$

In words, if industry i makes a technical improvement of the neutral type, the relative weight of that industry in the VON NEUMANN state becomes heavier than before.

Finally, it is at once seen from (4) that when the VON NEUMANN equilibrium prevails labour is distributed among industries in the ratios:

$$(40)\qquad x_{o1}:x_{o2}:\ \ldots :x_{on}=a_{o1}v_1\bar{x}_1:\ a_{o2}v_2\bar{x}_2:\ \ldots :\ a_{on}v_n\bar{x}_n.$$

As $\overline{x}^1P^1$ is proportional to $\overline{x}^oP^o$, we find that the technical change does not affect the distribution of labour.

3. Let us now examine effects of a technological change of the labour-saving type. Suppose an increase in F_i gives rise to an increase in b_{ki} and a decrease in a_{oi}, other parameters remaining unchanged. We assume that the constant returns to scale prevail before and after the technical invention.

Taking account of the fact that b_{ki} and a_{oi} are functions of F_i fulfilling (2) and remembering the definition of G_i, we obtain, by differentiating (10) and solving,

$$\frac{d \log v}{d F_i} = -(I - a - b)^{-1} M_i, \tag{41}$$

where M_i is a column vector such that its i-th component m_i is

$$m_i = \frac{1}{F_i} + \log \left[\left(b_{ki} \frac{1+r}{r+d_k} \right) / (a_{oi} v_k) \right] \frac{d b_{ki}}{d F_i} \tag{42}$$

and all other components are zero. We have $db_{ki}/dF_i > 0$ by the definition of the technical change of the labour-saving type. But $(b_{ki}\frac{1+r}{r+d_k})/(a_{oi}v_k)$ many be greater or less than unity. As was shown in Section 2, v_k increases when r increases. This together with $1 > d_k$ implies that $(b_{ki}\frac{1+r}{r+d_k})/(a_{oi}v_k)$ is a diminishing function of r; it may take a value less than unity when r is large, but greater than unity when small.

When m_i is positive, we have from (41)

$$\frac{d \log v}{d F_i} < 0.$$

Furthermore, we have

$$\frac{d \log v_i}{d \mathrm{F}_i} < \frac{d \log v_j}{d \mathrm{F}_i} \qquad (j \neq i) ,$$

because $a + b$ is a non-negative matrix with row sums less than one. This leads to $d\gamma/d\mathrm{F}_i \geq 0$. It is obvious that in the opposite case where m_i is negative we obtain $d\gamma/d\mathrm{F}_i \leq 0$. Taking into account the fact that m_i may be negative only when r is greater than a certain rate $\bar{r}$, we have Figures 3*a*, 3*b*, and 3*c*.

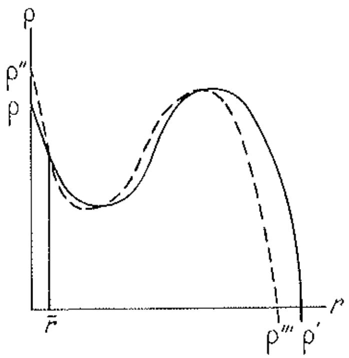

FIGURE 3-*a*

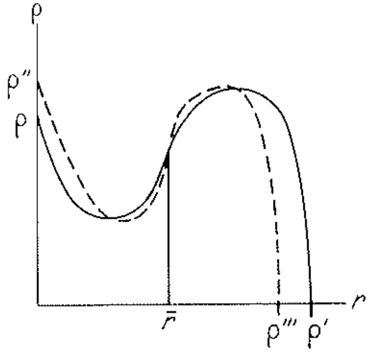

FIGURE 3-*b*

Suppose that the curve ρρ' intersects the 45° line on a point in the normal part. The intersection gives the old VON NEUMANN rate of interest r^o before the technological change. We can easily verify that the technical change of the labour-saving type results in increases in both γ and r, if $r^o < \bar{r}$, and decreases in both γ and r, if $r^o > \bar{r}$. The latter is a situation which would not be preferable from workers' viewpoint as well as from capitalists viewpoint. We may, therefore, say that a new method of production of the labour-saving type is not adopted

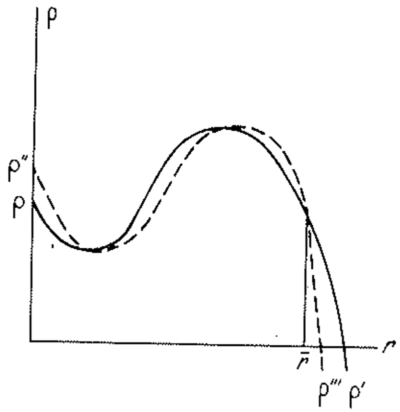

FIGURE 3-c

when r^o exceeds $\bar{r}$. (Similarly, a new technological method of the capital-saving type is not adopted when r^o falls short of $\bar{r}$). When r^o belongs to the perverse part of the curve $\rho\rho'$, however, we obtain the following result: A technological improvement of the labour-saving type yields an increase in γ but a decrease in r if $r^o < \bar{r}$, and *vice versa*. Thus, either γ or r increases, so that the biased technological change may be adopted, irrespective of the relation of r^o to $\bar{r}$.

The following argument is independent of the location of r^o and its relation to $\bar{r}$. Let us normalize the VON NEUMANN output configuration $\bar{x}^o$ so as to make the i-th component of $\bar{x}^o P^o$ (i.e. $\bar{x}^o v_i^o$) equal 1. It follows from (39) that before the technical change we have

$$(43) \qquad l_j - \sum_{s \neq i}^{n} l_s(a_{js} + b_{js}) - \sum_{s \neq i}^{n} l_s c_{js} = a_{ji} + b_{ji} + c_{ji}$$
$$(j = 1, \ldots, i-1, \; i+1, \ldots, n),$$

where $c_{js} = a_{os} g_j$ and l_j is the j-th component of the normalized VON NEUMANN output configuration, i.e. $l_j = \overline{x}_j^o v_j^o / \overline{x}_1^o v_i^o$. Differentiating (43) with respect to F_i, we get

$$\left(\frac{dl_1}{d F_i}, \ \ldots, \ \frac{dl_{i-1}}{d F_i}, \ \frac{dl_{i+1}}{d F_i}, \ \ldots, \ \frac{dl_n}{d F_i}\right) \tag{44}$$
$$= (f_1, \ldots, f_{i-1}, f_{i+1}, \ldots, f_n) \ (I - a_i - b_i - c_i)^{-1}$$

where

$$f_j = g_j \, da_{oi}/dF_i \qquad \text{if} \quad j \neq k,$$
$$f_k = db_{ki}/dF_i + g_k \, da_{oi}/dF_i,$$

and a_i is an $(n\text{-}1) \times (n\text{-}1)$ matrix obtained by omitting the i-th row and the i-th column from a; b_i and c_i are similarly defined. As $a_i + b_i + c_i$ is a non-negative matrix with row sums less than unity, any diagonal element of $(I - a_i - b_i - c_i)^{-1}$ is greater than off-diagonal elements of the corresponding column. On the other hand, by our definition of the technical change of the labour-saving type, we have

$$\frac{d\, b_{ki}}{d\, F_i} = -\frac{d\, a_{oi}}{d\, F_i} > 0 \ . \tag{45}$$

In view of $g_k < 1$ $(j = 1, \ldots, n)$ and $\sum_{s \neq i}^{n} g_s < 1$ (8), (45) leads to

$$f_k > 0, \ f_j < 0 \ (j \neq k) \ \text{and} \ \sum_{m \neq i}^{n} f_m > 0.$$

(8) We assume that g_i is positive.

Hence we have from (45)

$$\frac{d\,l_k}{d\,F_i} > 0 \,. \tag{46}$$

In words, a biased technical improvement which a substitution of b_{ki} for a_{oi} accompanied increases the ratio of the value of output of industry k to that of industry i. It is also seen from (40) and (46) that the amount of labour used by industry k is increased in comparison with the amount employed by industry i.

4. A similar argument may be applied to examine effects of a technological change of the capital-saving type. Suppose a substitution between a_{oi} and b_{ki} in favour of a_{oi} accompanies an increase in F_i. Differentiating (10), we obtain (41) and (42), where $\frac{db_{ki}}{d\,F_i}$ is now negative. Let $\bar{\bar{r}}$ be the value of $\bar{r}$ which makes $m_i = 0$. We shall arrive at the following final results:

$$\frac{d\log v_i}{d\,F_i} < \frac{d\log f_j}{d\,F_i} \leq 0, \qquad \text{if} \quad r^o > \bar{\bar{r}}, \tag{47}$$

$$\frac{d\,l_k}{d\,F_i} < 0 \,. \tag{48}$$

(47) states that when industry i makes a technical improvement which saves the capital good k, and r^o is greater than $\bar{r}$, then the prices of all goods (in terms of labour) will diminish, and that of i will do so by the largest percentage. (48) implies that

labour is redistributed in favour of industry i in its relation to industry k. Inequalities (47) and (48) yield

$$\frac{\bar{x}_k^1}{\bar{x}_i^1} < \frac{\bar{x}_k^0}{\bar{x}_i^0},$$

that is, a technical change of that type decreases the ratio of the quantity of output of industry k to that of industry i.

DISCUSSION

MALINVAUD

May I question Professor MORISHIMA on the implications of his analysis? The studies made thus far on the turnpike theory have been concerned with the optimal pattern of development of an economy in which there is no final consumption. You have succeeded in escaping from the latter restriction; but you have tied up consumption to the general growth of the economy by other rules. I do not see very clearly what kind of conclusion you intend to draw from the analysis.

You may be thinking that, by extending the turnpike theorem to a case allowing for consumption, you are giving indications about the qualitative features of the programs which would be optimal for development. You may also be aiming at describing how the actual process of growth has occurred.

I should like to know exactly your intentions. Do you explore indications for programming? or do you describe what happens in a capitalist economy?

MORISHIMA

The aim of this study is to extend the recent results of growth economics (especially the turnpike theorem) to a model with endogenous population growth and flexible consumption demands.

MALINVAUD

Yes, I understand that it is your immediate intention; but you probably have farther reaching intentions. You want to find new extensions of the turnpike theorem; but the final purpose of the exercise is not clear to me.

According to the purpose, I would have to look at the paper in different ways. If it is purely descriptive, then we should be careful that the hypotheses provide, at least as a first approximation, a proper description of what happened during the process of growth. If it is oriented towards planning, then we must look at whether too many constraints have not be imposed; because, if such were the case, the results might have little significance for planning.

Thus, I should appreciate if you could say a little more about these broader issues. Perhaps this is not the rule of the game. But Professor MAHALANOBIS induced us to look somewhat beyond the formal aspects of our theories.

MORISHIMA

Well, in various turnpike theorems so far established as well as in the original VON NEUMANN model, it is assumed that the supply of labour can be expanded indefinitely at the subsistence level of real wages; so that it completely ignores the problem of deficiency of labour, one of the most serious obstacles to a rapid growth. In fact, it is a defect of NEUMANN's theory of growth that no attention is paid to HARROD's observation that the natural rate of growth sets a limit to the maximum average value of the actual rate of growth over a long period.

In this study I am concerned with an economy where the planning authorities (or capitalists collectively) make an efficient investment planning to produce, at the end of the programming period, various outputs in desired proportions. If wages were fixed at the subsistence level, capitalists could accumulate stocks of capital goods at a maximum rate, but labour would remain stationary or could grow at a very small positive rate; so that a bottleneck due to a deficiency of labour would inevitably emerge sooner or later; hence the rate of real wages should be one of the variables of the programming.

Once wages are greater than the subsistence level, workers can make choice among consumption goods, and the growth programme depends on consumer's choice unless the consumption goods are rationed by the planning authorities as will usually be done during a war. I am sure that a planning model in which workers are allowed to choose consumption goods freely and the rate of growth of population is finite and depends on the per-capita consumption deserves serious attention and should be granted a citizenship of our science.

DORFMAN

Professor MORISHIMA's elegant model illustrates the ethical perplexities that Professor KOOPMANS has just discussed. For look: the balanced growth rate is given by the intersection of the curve showing the growth rate of the labor force as a function of the real wage with the curve showing the equilibrium rate of interest also as a function of the real wage. Technical change shifts this point of intersection, but if it increases the real wage it decreases the rate of balanced growth, and vice versa. This model therefore portrays a trade-off between high real wages and high rates of growth, which, of course, is to be expected in a full employment model. The novelty is that this conflict of interest between present and future results from the possibility of technological change, whereas previous derivations of this conflict assumed constant technology. Be that as it may, this conflict presents just the sort of ethical problem that concerned Professor KOOPMANS.

This problem might disappear from Professor MORISHIMA's model if the curve portraying the rate of growth of the labor force also shifted. It probably does shift in real economies. In fact, one of the crucial problems of economic development is to make it shift so that increases in the real wage are not absorbed by increases in the population. This raises a somewhat different ethical problem.

MORISHIMA

In this study I have pointed out that the conflict of interest between workers and capitalists (or the conflict between present and future) will he observed *only* when the original per-capita consumption before the technical invention is located in the « perverse » part of the curve showing the growth rate of the labour force as a function of the per-capita consumption (i.e. that part of the curve in which an increase in the per-capita consumption gives rise to a decrease in the rate of growth of the labour force). In the « normal » part there is no conflict; the extra output due to the increase in productivity is shared between the « present » and the « future ».

MALINVAUD

Concerning the model itself, I am not very happy about the hypothesis that savings come only from capitalists. Not so mnch because I would strongly disagree on the assumption that capitalists are saving the larger part of their income, and workers consuming the larger part of theirs. But, for already some time in our economies, governments have interfered in the distribution of income. Hence, I do not find the hypothesis made by Professor MORISHIMA very well suited for the practical questions which it is our ultimate air to answer.

MORISHIMA

My model, although it is oriented toward planning, has no public sector. It is implicitly assumed that any plan for a rapid growth proposed by the authorities is accepted by capitalists; so that they may make a plan requiring that all capitalists' income is automatically reinvested.

It is true that our assumption does not well fit any actual economy unless all industries are nationalized; in fact, in a mixed economy there are private and public sectors. The planning authorities will push forward their plan mainly through public sectors, while private sectors can act against it. It would, however, be beyond the present state of our techniques to establish the turnpike theorem in a mixed economy.

PASINETTI

I have some doubts on Professor MORISHIMA's paper, which do not concern, of course, the mathematics of it (which is admirable) but its assumptions and thus its bearing on the type of world in which we live. Professor MORISHIMA's model is intended to be a development of that of VON NEUMANN, and in order to explain my doubts, it may be useful to compare the two models.

VON NEUMANN, as we know, was concerned with a hypothetical society in which there is no technical progress and economic growth takes place at constant technical coefficients; relative prices as well as *per-capita* incomes remain constant as time goes on. In such conditions, no assumption is necessary about individual preferences, which can be accepted as given, whatever they may be. (It is only necessary to assume that people are, by and large and irrespective of time, of the same type). Such a scheme thus happens to have the mathematically interesting property that balanced growth (i.e. expansion of production of each commodity according to the income-elasticity of total demand for it) is a *proportional* economic growth.

I have had the opportunity of arguing myself in the paper I have presented to this Study Week, that the economic expansion which VON NEUMANN has considered is a very unrealistic type of economic growth. Yet, I would take VON NEUMANN's model as a very important first analytical step. And I should always look favourably on any development of it, ***provided that*** this development goes in the direction of a more practically relevant type of analysis, by relaxing some of the unrealistic assumptions.

Now, my question is: does Professor MORISHIMA move in this direction? If I understand him correctly, I must say he does not. By considering technical change — and thus increasing *per-capita* incomes — he has been compelled to introduce specific assumptions on consumers' preferences. He has assumed that all individual utility functions, besides being « identical », are « log-linear in the quantities of the commodities consumed. » This means that, when *per-capita* incomes increase, each consumer — if relative prices do not change — is supposed to increase his demand for each commodity in exactly the same proportion.

These are *further* assumptions with respect to VON NEUMANN's. And what is unfortunate is that these additional assumptions are not only unrealistic; they postulate a behaviour which we know to be impossible, at least among human beings. As ERNST ENGEL pointed out more than a century ago, when *per-capita* incomes increase, the demand for each commodity does not tend to increase proportionately; which means that utility functions are not log-linear.

To postulate a consumers' behaviour which goes against one of the strongest empirical laws of economics (ENGEL's law) makes Professor MORISHIMA's analysis more — instead of making it less — artificial than that of VON NEUMANN. I have been wondering why Professor MORISHIMA has made such assumptions; and the only reason that occurs to me is that they are the only ones that allow a model with technical change to keep the mathematically elegant property of proportional economic growth.

If this is the case, I must confess to be very disturbed. I feel that this is just the way in which mathematics can do economics a great disservice. For, in this direction, instead of using mathematics as an analytical tool for the interpretation of economic phenomena, we risk developing elegant mathematical models for their own sake, and *then* making whatever assumption may be necessary to give them an economic interpretation.

MORISHIMA

My interpretation of VON NEUMANN's theory of growth is somewhat different from Dr. PASINETTI's. He says that NEUMANN's model does not need any assumption about individual preferences, which can be accepted as given, whatever they may be. But, according to my interpretation which, I hope, is an orthodox one, NEUMANN made drastic homogeneity assumptions, not only on productions but also on the consumption side: He assumed that wages are held at the subsistence level so that necessities of life consumed by a worker (the consumption coefficients) are biologically determined and independent of prices. On these assumptions he showed that an economy can expand in constant proportions.

Now, in order to make his model more realistic we must, first of all, release it from the assumption of the subsistence wages. As people get richer, the proportions in which they divide their expenditure between various consumption goods, will depend on prices (because they can now choose among goods); and the proportions will vary when the *per-capita* income increases (ENGEL's law). In such a situation, it will be wiser and more profitable to forget as a first approximation the effects of the *per-capita* income on the consumption coefficients and to take full advantage of the assumptions for simplification than to confront the actual world directly and to loose one's way in its complexity. I believe it would be useful in making economic policies as well as in advancing theories to have found that a Neumannian growth equilibrium with constant proportions is still possible even when the consumption coefficients depend on prices, unless the income-elasticity of the demand for some consumption good is different from unity. It is of course true, however, that a second step toward the reality should take account of increasing or decreasing returns to scale on the production side and of deviations of the income-elasticities from unity on the consumptiou side.

MAHALANOBIS

I shall not speak on many of the detailed aspects of this particular model, because I am not competent to do so. The treatment of an important question by a very simplified model, I believe, throws some light on policy decisions.

Also, I should like to raise one question, not with reference to the particular model just now discussed, but of a broader nature, which is of importance to an underdeveloped country like India, as distinguished from Japan and advanced countries, regarding the usefulness of complicated models generally. I should like briefly to mention our experience that when some complicated models are used, the question of unreliability of data becomes of crucial importance; Dr. JOHNSON has drawn attention to this point in his paper, and Professor LEONTIEF has also referred to it earlier. In another intervention I tried to indicate two gaps between the world of reality and the model. Firstly, the gap arising from the lack of availability or the lack of reliability of data; and secondly, another gap between the data and the model. These are questions, of course, of a very general nature, which however deserve serious attention of econometricians.

KOOPMANS

I appreciate the support of Prof. MAHALANOBIS for what I tried to say earlier. At the same time I do not go as far as he does if I understand what he said to mean that there is something wrong in complicated-ness itself. I think we are entitled to make our models as complicated as we can manage as long as by that extra complication we obtain added insight, and the extra complications do not prohibit communication of the findings. We are working at the frontier of our collective understanding of these problems, and while ultimately we hope to end up with models that reflect reality better and which therefore have to be more complicated, at the moment we can't do much better than stumble along as we are doing.

MAHALANOBIS

I should agree entirely. A model may be, should be indeed, as complicated as is necessary for an explanatory model or a decision model or to serve some other purpose. I am in complete sympathy with the study of such models. Even when adequate statistical data or other types of information are not available, working with such models in an imaginative way may lead to the collection of relevant data; this itself is a very useful task. Or, such studies may lead to advances in theory as often happens in physics. I have therefore nothing against using complicated models in any way. The questions which I raised had some ambiguity, and I think Dr. KOOPMANS' observation were justified. One object was that sometimes simple models are quite adequate.

DORFMAN

Yesterday we had a long discussion of the types of economic model, which left me very uncomfortable. We then seemed to conclude that there were three types of model, explanatory models, forecasting models, and decision models, in Professor FRISCH's terminology. But I went away feeling that we had forgotten about some other kinds, and today we have had three examples of models that cannot be used for explaining any empirical observations, or forecasting the future, or making practical decisions. Today's models are intended to illuminate the logical consequences of some assumptions or conditions. We might call them a fourth type, logical or hypothetical models. I suspect that there are many other types of model also.

MALINVAUD

Since we apparently have some time this afternoon, I may perhaps be permited to come back on the motivations for my own paper.

Professor KOOPMANS has indicated this morning that one of the reasons for his own research was to explore the consequences of assuming a particular kind of utility for choices over time. This was certainly also a part of my own motivations. But I was still more strongly motivated by the need to see clearly what we should do when we use models with several periods, as a guide for planning or programming.

We can certainly do some programming by considering timeless models. Such models proved useful in various countries, in India for instance. But multiperiod models are necessary for many questions: the choice of investments, the discussion of public policies influencing the saving ratio, even the study of consequences that would follow a progressive reduction of the length of work.

In the multiperiod models now used by programming, terminal conditions are imposed. For instance, the future up to 1970 is divided into three periods and the capital structure to be in place at the end of 1970 is fixed a priori. Since such terminal conditions are, to some extent, arbitrary, one may have serious doubts on the adequacy of the procedure.

My research should be considered as an attempt to explore fully the nature of the solution in a much simplified model in order to understand better the implications of assuming arbitrary terminal conditions, and perhaps also to find new ways of dealing with the difficulty. All this is done for a one commodity world, but should give us some useful insights on what is likely to occur in more disaggregated models.

DORFMAN

Are you disagreeing with me?

MALINVAUD

No, I am not! I am in full agreement with Professor DORFMAN. The preceding comments are merely intended to show what I had in mind when speaking of the motivations for a research.

MAHALANOBIS

I do not think there are basic differences of opinion but while agreeing with many of the observations made here, I am trying to draw attention to one point — that when certain periods, 10 years or 15 years, are taken as the time horizon in making government decisions, there are necessarily makeshift arrangements. In an underdeveloped country which starts experiments in the way of making policy decisions which are intended to be implemented (I am calling these experiments), and also starts exercises in the way of building models, sometimes it may be possible to feed the models by numerical data or sometimes it may not be. There is a more general point about models which also require information other than statistical data in a numerical form. I was therefore speaking of wider experience by which I meant in respect of any model, judgement as to what are relevant factors, or what are their order of priority. I have an impression that there is sometimes a good deal of faith in model making which with the help of very highpowered computors would supply push-button solutions. This is why I referred to the limits of usefulness, depending on whether a model is capable of being fed by quantitative data or involving non-quantitative judgements, or factors of selection, and also to what extent the solution depends on the degree of accuracy of the information which has to be supplied in a quantitative form. One great service to the underdeveloped countries would be to discuss the usefulness as well as the limitations of models in application to practical affairs.

WOLD

I have two comments. The first refers to the entire group of four papers presented to-day [1], all of which deal with purely theoretical highly simplified models of economic growth. This kind of logical analysis has a strong appeal, and that it has a wide appeal is clear from the fact that the three other papers are closely interrelated both with regard to problems and results.

My second comment refers specifically to Professor MORISHIMA's paper, and is a question about the proportionality assumption on page 7 [2]. Am I right to understand that this assumption requires, for example, that the demand for food remains a constant fraction of GNP as the economy grows? If yes, the assumption is in radical contrast to the whole outlook of a later paper to-day, that of Professor GALE JOHNSON. There the whole emphasis is shifted. There exists no such proportionality; on the contrary, the lack of proportionality is a universal handicap for the farming population.

ALLAIS

I have two remarks to make. The first was made by Prof. WOLD, namely that there are some quite strong hypotheses in the model.

Secondly, I have unfortunately not had the time to study Professor MORISHIMA's paper very carefully since I was working in the other group. But so far as I can judge, the MORISHIMA's model is a special case of a general theory I presented this year in my paper for the Cambridge Round Table of the International Economic Association. Thank you.

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(1) KOOPMANS, MALINVAUD, MORISHIMA, PASINETTI.

(2) The discussions having been organized in two separate groups during a first phase of the Study Week, my question belongs to the second phase of joint discussion. Now when reading the proofs I see that a similar question was posed in the separate discussion, but I repeat the question to mark the importance of the issue.

MORISHIMA

With regard to Professor WOLD's second question, please see my reply to PASINETTI. I am sorry that I cannot reply Professor ALLAIS, for I have not yet read his Cambridge paper. As I am a very slow reader, it will unfortunately spend a long time before it finally appears at the front of the queue of my backlogs, although the time required to bring it from Paris to Osaka has been shortened to 20 hours. Thank you.

FIN DE LA I^e PARTIE