Ph. D. Dissertation 4700

OPTIMUM PLANNING FOR A DYNAMIC ECONOMY

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PREFACE

Those who influence, guide, or plan economic systems are seldom concerned with the best of all possible worlds, or with the best of all possible plans. The working political economist is happy enough if some general objectives are achieved sooner or later, if certain dangers are avoided, if the direction of social change seems to him on balance good, or at any rate not so bad as might have been expected. A government that has seen the national product grow at four per cent per head per annum will be relatively unmoved by the plea that different policies might have achieved a five per cent rate of growth, or prevented an undesirable shift in relative wealth towards certain social groups. The economic planner himself is likely to be satisfied with the approval ensured by steady improvement: to better what is good enough when ignorance about present and future is so great seems at best unnecessary, at worst a foolish risk.

The apparent wisdom of the practical man is no sure guide for the theoretical economist. The economist, poor man, has to talk to many audiences, amongst whom practical planners are by no means the least important. But the first audience he must satisfy is himself. Economists are unduly embarrassed when they are found talking to themselves: their first duty, surely, is to find answers satisfactory to their own understanding, and to those arbiters of right thinking, the canons of reason. Problems unknown to the practical man, or by him contemptuously ignored, may rightly occupy the best hours of the economist's mind. For if he cannot answer the questions of the world to his own satisfaction, he is in no strong position to foist his prejudices upon those who act. We do well to recognise that different audiences demand different languages. The arcane terminology of theoretical economics is not the best of mediums for addressing a Planning Commission: nor should we suppose that theoretical economics must be readily accessible to the planners, even when their practical problems are under discussion. When we know the answers, to the problems, we can find the arguments for the man who has to act upon them. Answers will not be found if we do not submit the problems to the full rigours of analysis, regardless of the difficulties of communication that we may be preparing for ourselves.

In brief, that is my justification. I have taken a fundamental planning problem, the determination of the right rate of aggregate investment, and sought to analyse it as thoroughly as I could, for an economy in which production possibilities change with time and are not completely certain. In doing so, I have been able to extend our knowledge of the problem to a point where recommendations may begin to be realistic. More important, I think that the discussion casts some light on the way in which optimal policies for more complex problems can be derived, and in particular offers some hope of making a sensible allowance for uncertainty. Why has only a "simple" problem been studied? In the first place, the problem of the optimal investment policy is only simple in a relative sense, being logically the basic planning problem, but, for me, a difficult one to solve. Secondly, I believe that a thorough study of basic problems is the only way to understanding in this field, like most others. The theory of planning is in its infancy. In infancy, most of us seem simple.

The firmly original work of the thesis is the working out of optimum investment policies for the model economy in chapters IV - VII. The model economy itself is a one-good economy, where there is both technical change and uncertainty. It is worked out mathematically. The mathematical tools are calculus and some probability theory. Advanced theorems are not used: however the mathematical argument is by no means simple. The conclusions, which are not in all respects what one would expect, are discussed in chapter VIII. The first two chapters are devoted to an outline justification of the methods used. When value judgments enter, agreement leaves at once. Hence any method of treating questions of value requires a defence. My own approach is no complete departure from tradition. The ghostly presence of Marshall and Pigou, and other Cambridge economists of a past age, will be apparent. Yet it would be dishonest to disclaim all novelty: there is much to which the shades would take exception. My wish is to find a welfare economics that is not entirely irrelevant to economic planning in the underdeveloped world. The arguments of the first two chapters are meant to be entirely relevant to that end.

Professor J.R.N. Stone has been my supervisor during the time that this thesis, and the work it reports, has been in preparation. He introduced me to Ramsey's work and the optimum saving problem. Furthermore, it was while attending the discussions of the "Growth Project", under his leadership, at the Cambridge University Department of Applied Economics, that I began to think seriously about problems of planning. I began to wonder (a) about the relation between economic models and the aims (often hidden) that they might serve, and (b) about the relation between uncertainty and the economic decisions that ought to be taken. It was these questions that

led me to the work of this thesis; and Ramsey's work came in again by the back door.

The ideas and arguments of the thesis have benefited most from the many conversations I have had with Mr.D.G.Champernowne, and Dr.F.H.Hahn. Others whose arguments have helped me to see more clearly the error of my ways or their ways include Mr.A.K.Bagchi, Mr.M.H.Dobb, Professor W.M.Gorman, Dr.A.K.Sen, and members of the mathematical economics seminar at Cambridge, and seminars at Nuffield College, Oxford, and at Massachusetts Institute of Technology. The responsibility for what is here is mine: what it would have been without their help does not bear contemplation.

Sometimes I am abashed at the eccentricity of my views. But what is presented here is the only way in which I can understand what I take to be an important problem, and the only style in which I could finally justify an economic policy to myself.

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29 September 1963.

CONTENTS.

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NOTES.

1. The pages of the chapters are numbered independently.

2. The equations appearing in chapters IV - VI are, for convenience, numbered consecutively.

3. All logarithms used are natural; that is, their base is e.

4. Bibliographical references are given by enclosing the surname of the author and the date of the work in brackets - e.g. (Arrow 1951a). When more than one work of the same year is referred to, letters are used to distinguish. Complete references are given in the bibliography.

CHAPTER I ETHICS IN ECONOMICS

- 1 -

It is part of the fascination of economics that economic decisions have to be based on inadequate data, and are frequently taken in the light of aims that are not so much ill-conceived as scarcely conceived at all. Whether they are the decisions of a private business, a public authority, or a national government, they are - and must be - rooted in set habits and existing institutions; while whatever explicitly rational arguments and calculations may have entered into the decisions are partial and tentative. Economics is forced to ask the question: what ought these decisions to be?

I do not argue that we should seek to replace all habit and institution by rational and conscious argument: it is neither desirable nor possible. Nor would I go so far as to argue that man is best served by a maximum of conscious reason and a minimum of tradition and habit. But, for me, the purpose of economics is to study the introduction of rationality into the framework of convention within which economic actions happen; so as to provide an overall critical judgment of economic systems, and suggest ways in which the right economic actions can be taken. To this end, both empirical studies of economic situations and developments and the logical exploration of empirical hypotheses are essential. We must also ask what the right decisions would be.

This last requirement presents itself differently to different generations. The conscience of the economist finds new concerns and new possibilities. The present generation of economists has to struggle with the many particular investment decisions about which the advanced countries are becoming more self-conscious. But the acute problem is that of underdevelopment. We ought to have something to say about the methods of economic planning by which the emergent nations are trying, or could try, to remove the curse of poverty. Both these classes of problems are spoken of as problems of economic planning. A modicum of common sense fortified with some training in economics can, no doubt, be of practical help to planners. We cannot be satisfied with that. A theory of planning, developed to the point where the assumptions can claim some degree of realism, could provide planning with a rational basis. The present thesis is meant to be part of the groundwork for such a theory. In terms of its final aim, and the type of decision it is interested in, it is part of a theory of planning. Logically, it is part of the theory of right economic decisions.

If the aim of economics is as I have suggested, a theory of right economic decisions should do two things. It should provide specific methods of calculation, which those concerned with taking economic decisions can use (subject to expert advice, of course). It should also be, by implication, a general qualitative criticism of institutions and habits, and even of the economic system as a whole. Yet work on these two sets of questions has, in general, shown a peculiar dichotomy, which seems to me a criticism of both. On the one hand there are various contributions to the theory of economic planning, ranging from the practical activities of econometricians to the theoretical work of some economists who have concerned themselves with general features of the planning situation (1). On the

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(1) The quality of such work is very uneven. As an example of the first group, one can mention Professor Chenery's work, e.g. (Chenery 1959). A good example of the second is (Dobb 1960).

other, there are the various forms of welfare economics.[2] Although a spectrum between these extremes exists, the central zone does not shine strongly. It is with this zone that I am concerned in the present thesis.

Typical of the practically important questions that a theory of planning must try to answer are: How rapid a rate of economic growth should a country aim at? How much should be spent on irrigation projects this year? How labour-intensive should the techniques of production be? Such questions want prescriptions in answer. Some prescriptions have no worry about the aims they serve, as when one instructs a child on winding a watch. It is seldom that economic questions are of so simple a kind: the impact of economics on people is too complex and subtle. The prescriptions in answer to such questions must be moral prescriptions, in the sense that they rest upon moral considerations.[1] There is rather little in economics with which the economist can be said to be satisfied, but perhaps he feels most embarrassed and least competent when he must treat questions like these,

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(1) It is rather difficult to describe satisfactorily what is meant by the terms "moral" and "ethical". In this thesis, they are generally used interchangeably to refer to questions of value that are not determined for us by universally accepted social norms (in the way that watch-winding and doing mathematics are.)

(2) The field is so large that references would be otiose. Particularly interesting for us here is Solow's review of (Dobb 1950) from the point of view of welfare economics, (Solow 1962). Most of the discussion on economic planning between the wars (as reflected in (Hayek 1935) for example) seems to belong to the second category.

especially if he is aware of the breadth of the issues involved. If the existing corpus of economic theory is to provide any help, we should expect to find it in that branch of theory known as Welfare Economics. Oddly enough, one finds most illumination in discussions of the subject or in some applications of its propositions (for example, those in (Pigou 1952)) than in the body of propositions that strictly make up theoretical welfare economics. This distillation of welfare economics into a systematic theory, begun by Walras and Pareto, and now to be found most elegantly presented in such works as (Graaff 1957) and (Debreu 1961), I shall - rather cavalierly - call the Traditional Welfare Economics. It seems to be clearly distinguished from the more pragmatic tradition of Marshall, Pigou, and Robertson; and the distinction is sometimes expressed by referring to the two schools as New and Old Welfare Economics, respectively. The latter is somewhat discredited, being thought to be badly tainted with naive psychology; the former, more wedded to a grand Design, has seemed distinctly fragile. In outline, what I shall do in this thesis, bears a close resemblance to some features of welfare economics. I am therefore rather concerned that it should be rightly interpreted. Welfare economics has been roundly criticised for not helping - or worse, for misleading - the practical economist. The critique is standard economics and cannot be ignored.[1]. If my own position is to be made plain, something must be said about the widely-accepted critique of welfare economics, especially in the form of a systematic examination of the optimality properties of perfectly competitive economies.

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(1) (Little 1957) is the standard work. An extreme view is presented with clarity and vigour in (Graaff 1957).

- 2 - Theoretical welfare economics consists substantially of a discussion of conditions for Pareto optima. It is therefore attacked both on the grounds that the assumptions necessary to derive the conditions are too restrictive, and on the grounds that a Pareto optimum is not a very interesting object. It is convenient to take the latter argument first. If an economy is defined simply by the set of production possibilities, and by the preference orderings of its members, the number of Pareto optima is large, usually infinite. For example, if two people have to share a cake, any division is Pareto-optimal, since more for the one means less for the other. One does not feel moral respect for a man who wants the rules of distribution and exchange so restricted that a unique Pareto-optimum exists. He is assuming away an important part of the problem. Formally, the problem can be transformed by introducing a Social Welfare Function.(1) This is a function mapping the sets of individual preference orderings into an ordering of the set of possible distributions of commodities. It is a straightforward generalization of the addition of individual utilities which came naturally to a more utilitarian generation of economists. The function has to satisfy the condition that one distribution of commodities is not preferred to another if no individual prefers his allocation under the first arrangement to his allocation under the second. Certain surprising difficulties in constructing such a function were uncovered by Professor Arrow (Arrow 1951). But the chief difficulty is to know what it is and where it comes from. It is, after all, if it is to apply to an actual economy, an enormously detailed function, of a great many variables.

Once such a function is introduced, however, some of the objections to the assumptions made in order to derive conditions for an optimum are

(1) Cf the exposition in (Samuelson 1947).

absorbed into the criticism that the Social Welfare Function does not help. For external effects and indivisibilities do not affect the possibility of computating the optimum situation by means of the Social Welfare Function; although they may render impossible certain ways of achieving it by means of prices, income transfers, etc. The possibility of achieving the optimum by means of a perfectlycompetitive economy, modified by income transfers, is, indeed, the main concern of traditional welfare economics. This is met, quite properly, with the criticism that the calculation of the right income transfers requires complete knowledge of the Welfare Function. However these important questions about possible systems do not help very much in determining optimal planning decisions, and I shall not concern myself with that side of the subject.

As far as production goes, the critics are chiefly worried by the feeling that the structure of the traditional welfare economics is designed for stationary economies and must find non-stationary economies hard to digest. The infinite time-horizon and the presence of uncertainty are obviously new features that may be difficult to incorporate. It is also felt, with reason, that the absence of external effects is a much less plausible assumption when the production of the same commodity at different times must be treated - for the purposes of theory - as the production of two different commodities. This last point reduces again to the problem of the Social Welfare Function; but the other points are of real importance since a stationary economy is presumably not the best possible state.

There is no essential difficulty about extending the ideas of welfare economics to encompass a finite period of time. All one does is, as I have mentioned, to regard "porridge-on-Monday" and "porridge-on-Tuesday" as separate commodities.(1) So long as the time-horizon is finite, however, the

(1) This is done in (Debreu 1961)

stock of goods remaining at the end of the period must somehow be valued, which is impossible.[1] It is possible to let the time-horizon tend to infinity. (cf. (Malinvaud 1953)). It would seem to be much less easy to decide whether an optimum exists when this is done; and there are even some problems about defining the optimum (which I shall touch on later): but in fact no points of fundamental principle are raised.

Suspicion of an infinite time-horizon is by no means without foundation, however. At any rate within the framework of traditional welfare economics, there is reason behind the instinctive objection. Human beings with articulated, consistent, and completely detailed preferences for a period of a week strain the economist's fertile imagination sufficiently without the added burden of stretching that week into eternity. Uncertainty about the possible transformations of the economy increases as we look further into the future: consequently the precise specifications apparently required by the theory bear little comparison with actual knowledge - or even with conceivable knowledge. These two points are connected but not equivalent; I shall leave the first for the moment and concentrate on the problem of uncertainty.

Surprisingly, uncertainty is seldom mentioned in the standard treatments of welfare economics. However, some attempts to bring it within the framework have been made. Since the problem is important, I must mention three ways in which it can be attempted:-

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(1) Graaff, somewhat oddly, regards it as a political decision.

(1) It may be assumed that the analysis in terms of definite, sure values of the variables is applicable when there is uncertainty. Although we are not in fact sure of the values of the variables, we may be able to assign an "expected value" to each variable. Perhaps this is the belief of those who prefer not to mention uncertainty at all. If we are content to have approximations, there is more to be said in favour of this view than may at first sight appear. But it is very easy to think up cases where it is clearly invalid. Suppose that the expected value of the annual output of foodgrains in a poor country could be increased if a new, but as yet incompletely tested, method of cultivation were introduced throughout the country, Most probably this increase in expected output would be gained at the expense of an increased risk of famine resulting from harvest failure. The experience of several countries, on a large and small scale, in recent years, should make us doubt whether the risk is worth taking.

(2) In an appendix to "The Economics of Welfare", Pigou suggested that uncertainty should be treated as a factor of production. (Pigou 1952, p.771). To be more precise, he suggested that we treat uncertainty-bearing as a factor of production. One could, I think, raise the objection that the programme of definition and measurement that Pigou outlines could not in fact be carried out, because the unit of uncertainty as he defines it depends on the particular market prices prevailing, in that the objective project corresponding to one unit of a particular kind of uncertainty-bearing would be different if market prices were different; but the chief objection is that the trick fails to do what is wanted. Production with uncertainty-bearing among the variable factors still produces uncertain results, and we are not told how these are to be compared. The logical conclusion of Pigou's position is:-

(3) The Method of Analysing Commodities. In (Debreu 1961), the final chapter is devoted to the problem of uncertainty. There Debreu defines commodities so as to be different, not only at different times, but also when different states of the world exist at the same time. We do not know which of the many possible states of the world will in fact happen at any future date: what can be produced, and what men will want, will be different according to the particular state of affairs that will have come about at this future date. Therefore we label cabbage as "cabbage $_{t,1}$" at time t if the weather is good, as "cabbage $_{t,2}$" if the weather is bad, and so on, including all possible states of the world. Then preferences must state which consumption possibilities would be preferred to what, when the consumption possibilities are defined in terms of cabbage$_{t,1}$ at t if there is good weather, cabbage$_{t,2}$ at t if there is bad weather, and so on. An individual's preferences between these different possibilities will reflect his estimation of how likely the various possibilities are. The whole formalism of the new welfare economics (mathematical style) is applied, and it is shown that there will be a Pareto-optimum if each of the commodities (so defined) has a price such that each producer gets zero profit (adding up the value of all commodities produced and subtracting the value of all commodities (including services) used in production - it is rather like the expected discounted profit, but we do not need to say so, and no consumer would prefer different consumption at the given prices with his initial wealth. This is formally admirable, though leaving out the usual complicating factors so that welfare functions need not be mentioned. But the complexity has been increased unbearably. Even with a finite time-horizon, the consumers have become strange creatures who can conceive precisely the future

of the economy (so far as it affects them): their omniscience lacks but one thing - they do not know which of the possibilities (supposedly finite) in number will actually occur.

In fact the possible states of the world are not finite in number. Even if they were, it is in principle impossible to list them precisely, and practically impossible to list them with any precision worthy of the analysis. Hence the idea cannot be of use for a theory to back practical decision-making.

A particularly serious difficulty about this attempt to apply the traditional framework directly to an economy of uncertainty is the way in which the theory is based upon consumer preferences among uncertain prospects. It may be that we could make good guesses about what people would prefer in the way of known and certain prospects. But in extending the notion of defined preferencesto the set of uncertain prospects, the consumer's guess about the relative likelihood of,say, good or bad weather during July, or of contracting or not contracting tuberculosis, is being allowed the same sovereignty as his preference for milk chocolate over toffee. Scientific enquiry can increase knowledge about the relative likelihood of different events, and in general even the consumer himself would presumably prefer that his choices should be based on the best knowledge, supposing he were competent to judge what that was, which he is not. Somehow, it seems, individual tastes have to be combined with such information as can be obtained about the possibilities the future holds. For the welfare economist to define the possibilities precisely, but to claim complete agnosticism about the probabilities (or at least to leave all judgment about them to the economic agents) seems an unnatural treatment of the problem.

Finally, the critique fastens on the demand side of the analysis. The theory of preference orderings is, I think, an important and illuminating part of economic theory. One is reluctant to align oneself with those who want to jettison it in order to leave the difficult problems of demand out of their economics. But from the point of view of practical planning, the treatment of preferences in the formal theory of welfare economics certainly raises some awkward problems.

So far as the logic of the theory goes, preferences are just consistent orderings of the possibilities open to each consumer. To speak of preferences at all implies a certain consistency in the behaviour - or at least the wishes - of the person in question, in that they must continue so long as the person's circumstances remain exactly the same. Preferences, we may say, are what is revealed by deliberate, mature choices. We have to say "mature" because a man's choices depend onthe point he has reached in his personal history; and even deliberate choices may vary in the degree of farsightedness or self-understanding they are based upon. Certainly the preferences in welfare theory have to well-informed and far-sighted. The preferences must be preferences recollected, or foreseen, in tranquility. It must be assumed, furthermore, that mature preferences are stable: if the preferences were observed to change, that could be explained either on the grounds that they were not in fact mature (yet), or on the grounds that the individual is moving from maturity to senescence, neurosis, or perversity.

It may be objected that people do not know what they (maturely) prefer, even in terms of the atomic elements of demand (which the "apparent" commodities and services consumed are meant to satisfy); that practicable observations (based on aggregative behaviour) cannot reveal the mature preferences, because of poor understanding of the goods and services actually bought, large differences between individuals, and the prevalence of non-mature actions; and that an enormous group, namely future generations, have no means of getting any sort of preference observed. The first two arguments seem to me plausible, but difficult to establish: the last argument incontrovertible.

Quite apart from the problem of using the formalism of preferences, there must be serious doubt about the adequacy of the picture of consumption provided by the welfare economics model. The model requires that the impact of the productive system on people should be analysable into processes of consuming atomic services produced by the provision of services from people and the non-human world (whether made or found). Perhaps a programme of analysing the impact of the economic system in these terms could be carried out. I am doubtful. Man has spent considerable effort during the modern age in seeking to analyse the world into atomic parts. He has not been conspicuously successful, except in the physical sciences. It is my belief that a more practical model of the economic impact could be built if beside the concepts of goods and services we put the concept of a facility.

A facility is anything that it may be appropriate to value because of its existence, rather than in terms of services it may provide to, or require from, individuals. It might be appropriate so to regard some entity in the economic world when it is unhelpful, too difficult, or impossible, to analyse its impact into individual services. This is a large topic, into which I do not want to enter here; two examples may help to indicate the use to which it might be put. Consider education. The approach of welfare economics (and, indeed, of standard economic theory) is to attempt to analyse the effect of education into the effect its equivalent services have on production, and - if we are lucky - the direct value of its services to the recipient. Any school, however, is a complex experience, and it is none too easy to measure the myriad services it provides by means of an economic yardstick. Even if that could be done, it is impossible to evaluate the importance to an individual of the kind of education (the complex of "educational services") received by others: this is not only a matter of their productivity, but of the quality of life that is produced for each person by the kind of people others are. A completely analytic approach to the problem of evaluating education is unlikely to be illuminating, and extremely unlikely to produce a satisfactory method of solving the problem.

My second example is agricultural land. Besides the obvious importance for the production of food (which is, at least in part, amenable to the traditional approach) we have the impact on the kind of life lived by the agricultural community - which is highly resistant to any attempt at analysis into services given and received - and the subtle effects of the regularity or lack of regularity of agricultural supplies, and of the nutritional content of the food provided. (1) In this example, as in the previous one, it is difficult to avoid the idea that various aspects of the facility can be described as <u>Gestalts</u>, that is, as some sort of organic whole, which we are more capable of comprehending as a whole than as a collection of analysed parts.

It may appear that in talking of facilities, one is simply saying that many things cannot be measured. The assertion that they are important is closely akin to Pigou's assertion that economic welfare is only a part of

(1) For any particular country it might appear that international trade minimises this sort of effect; but transportation costs make a big difference. Even if we look at the world as a whole, it is surely clear that changes in agricultural land, which are certainly possible economic actions, have widespread effects.

total welfare. This becomes a particularly important proposition, however, when we want a theory of detailed planning decisions; for it has become very important that economic planners should not neglect the consequences of their actions that fail to provide immediately quantified results (1), or ignore those fields of investment that the analytic approach is prone to undervalue. It is not easy to fit the notion of facility into the traditional welfare analysis, precisely because there is no means within its framework of solving the problem of measurement. The approach that I favour is at least not reluctant to pose the problem.

No one doubts that practical welfare economics is an approximate science. One can conclude from the criticisms levelled at the traditional welfare economics that the subject would do well to acknowledge this in its very structure (2). It needs to be able to deal with change and uncertainty. And, above all, a theory of planning must, rather than formalise the problem of values in terms of an inaccessible Social Welfare Function, come to grips with it in a way that allows discussion and decision. For the traditional

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(1) Statistics of rice production are a measure of performance. Statistics of the number of schools opened are merely a record of administrative actions. It is all too common for facilities such as education to suffer from being measured in this way rather than in terms of actual results. Sometimes quantity <u>is</u> the enemy of quality.

(2) It is a feature of the Marshallian tradition that it does so.

welfare economics, the end was the justification or rejection of a form of economic organisation.[1] This programme has rendered it unhelpful for the taking of practical decisions, above all those quantitative investment decisions that are an important part of a theory of economic planning.

\- 3 - In response to the critique of welfare economics, there have been suggested a number of escape routes.

It is worth outlining some of the possible positions, the more clearly to distinguish my own from them. In accordance with the belief in certain primitive societies that to name a man is to destroy his power, I have provided convenient, if not entirely accurate, labels.

(1) The fundamentalist believes, hopes, or pretends that the critique did not really happen. It is not impossible to find economists who have their noses so close to the ground around some particular problem that they can blindly use welfare propositions where a rational view of welfare economics would not encourage it.[2]

A much more sophisticated form of this position is that the propositions of welfare economics are the best we have, and perhaps the best we can get, and may therefore be taken as a fair approximation. This seems to be Little's position, at least as regards parts of the recognised theory. Since most of the results that are relevant to planning problems depend on a social

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(1) Wicksell quotes Walras as starting economic theory from the belief that "Il faudrait prouver que la libre concurrence procure le maximum d'utilité." ((Wicksell 1911),p.74).

(2) Some of the articles in (Munby 1960) seem to be open to this criticism e.g. Munby's own paper, and are in fact subjected to it elsewhere in the volume. Another example is (Jewkes 1960),p.3, where it seems to be argued that because divergence between social and private gain, and poor judgment about the future, are difficult to prove or measure, therefore they are unimportant and can be neglected.

welfare function of formidable complexity, this position is not very helpful for a theory of planning.

There is also a group who, because their moral sense or instinct commends one of the conclusions of the anlysis, swallow it all. Believing in any case in a free economy, they are led to favour the perfectly competitive form, through some interpretation of welfare economics. It is the automatic nature of the world portrayed by the traditional welfare economics that appeals, allied to the proposition that, whoever knows best, neither the (other) economist nor the statesman has any right to claim it is himself.(1) I do not think that perfectly competitive economies can be created, nor do I think that the majority of people want perfectly free economies: there would be no experts if there were not a desire to use expert opinion. Once the possibility of interference with economic agents is accepted - that is, once the legitimacy of the agent, and his political context, become live issues - the question of the kind and magnitude of interference does arise. The standard critique shows, I think, that the perfectly competitive solution to the welfare problem cannot properly be applied to those cases - be it the police force, or investment in roads - where interference is accepted.

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(1) I regard (Bauer and Yamey 1957) as a well-reasoned example of this view.

(2) The Idealist attempts to avoid the pitfalls of abstract calculation by constructing an Ideal Economy. This seems to be a fair way to describe the attempts of a number of mathematical economists to transform welfare economics into the study of certain very sophisticated economic models.(1) Once one has them, it is not entirely clear what one does with them (but as one stage in the creation of a scientific theory that will some day be realistic, there might be something to be said for them. I have been arguing that, at least for welfare questions, they are going the wrong way: but they may be very illuminating pictures of the way economies work.) Another exponent, who sticks to the English language, is Mrs. Robinson (Robinson 1960):

> A large part of economic teaching consists of setting up an idealised image of a free private-enterprise market economy, working in conditions of perfect competition, and then throwing various bricks at it by way of exceptions and objections. The structure is now so battered that it is easier to make a fresh start and transfer the image to an idealised planned economy ..... In a discussion of this kind it is impossible to avoid an element of personal judgment; the argument is set out dogmatically and is intended to serve as a basis for disputation. (p. 200)

Probably Mrs. Robinson regards this exercise as worth doing in itself - almost an "economy-game". If that were the only significance of an Ideal Economy, it would form no part of welfare economics, since it would provide us with no prescriptions. Can an Ideal Economy be used as a judgment on any particular economic system, and provide us with a beacon guiding us whither we ought to aim? There seems to be two difficulties: how can we know that the economy we picture is ideal? And how can we know when any actual economy

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(1) Cf. (Arrow 1951b), (Debreu 1961).

gets nearer to it? This new programme - which has never, so far as I know, been presented explicitly - would assume too much of common sense, for it would assume that intuition trained by logic could compare the Ideal with the Actual in sufficient detail, and with sufficient accuracy, to provide a guide to economic reform. Indeed one can see few grounds, a priori, for assigning the construction of the Ideal Economy to educated intuition, although this is the only method really offered, since the mathematical economists, for instance, have to restrict the field of possibilities very severely, and scarcely argue in favour of the restrictions.

The programme of constructing an Ideal Economy is particularly important because the administrative structure of planned economies effectively encourages such an approach. Although in Marxian theory, the path of development may be inevitable to the trained eye unclouded by anti-proletarian interests and properly educated in dialectical materialism, it certainly looks as if the only ultimate guide for Soviet planners is their conception of the ideal communist state. The very lack of connection between the image of the Ideal, and the right action now, allows plenty of room for the fight between rival interests to determine actual policy.

(3) The Nihilists, who are all around us, may be represented by Graaff's brilliant advocacy.[1] They accept the critique wholeheartedly, and deduce that welfare economics is a spurious subject:

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(1) I have heard that Graaff no longer holds the extreme position argued in his book and discussed here. No doubt a second edition will be modified.

> If positive economics can provide people with an understanding of the various far-reaching indirect effects of particular policies, it will probably also provide them with a basis for drawing welfare conclusions, for themselves and according to their own lights. In my view the job of the economist is not to try to reach welfare conclusions for others, but rather to make available the positive knowledge - the information and the understanding - on the basis of which laymen (and economists themselves, out of office hours) can pass judgment. ((Graaff 1957), p.170)

The position is based on the thesis of the perfect democracy of values: that all welfare (i.e. ethical) questions can be competently discussed with everyone. If this were true it would distinguish ethical statements very sharply from ordinary factual or analytic statements, which are certainly not equally open to discussion by everyone. In what Graaff calls positive (1) matters, the importance of competence is recognised. Now Graaff's position will certainly be false if it is impossible to reduce ethical questions about economic matters (which clearly are involved with special knowledge and argumentative experience) to ethical questions that make no reference to any matters outside what we may call the general competence. Thus the economist starts with the question "How much should Britain invest in blast furnaces in 1965?", analyses it carefully, and comes out with a series of "ordinary" ethical questions, from the answers to which the answer to the original question can be deduced. The ordinary questions are then answered by the layman.

Quite apart from my doubts whether any economist is such a perfectly logical machine that - if it were possible - he could transform the economic question into questions of value within the general competence without allowing any other values to creep in or out, I have strong logical doubts about such a

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(1) The terminology goes back to Comte. (Friedman 1953) quotes J.N.Keynes as giving expression to it in the context of economics.

position. It may be thought either that the economist is going to churn out general ethical principles (which the layman is asked whether or not he assents to), or that he will churn out many particular ethical questions, such as "Do you think it would be a good thing to have saucepans cheaper, but soap-powder more expensive in a proportion of three to one, etc.......?"

Consider the first possibility. General ethical principles are either like "You ought not to tell lies" in that their full understanding involves the addition of many particular exceptions, which one discovers from experience; or like "Love your neighbour" which one has to learn to interpret, since loving and being a neighbour are not simple, easily-learned, notions. In both cases - and I think this is true of all the general ethical principles that might arise in this context (1) - the understanding of the principle depends on knowledge of the situations to which they are relevant.

As with all general principles, general ethical principles are to be applied to new situations. But this application is not automatic. Onehhas to learn about the new situation before one can gain confidence in the application; and even time spent on learning about new situations does not ensure right application - the criteria for that would clearly be very complex to set out. What one cannot do is assent - commit oneself - to the application of the principle in an unfamiliar context without understanding the context. A person who understands an economic context is an economist.

This proves that if the reduction in question were to general principles, the question of assenting to these principles in the economic context would not

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(1) Some ethical principles, such as "If you can do what is right, you ought to do it" are probably analytic. But as with analytic statements and synthetic statements, we cannot deduce a synthetic value-judgment (such as "we ought to build two new rolling mills next year.") from synthetic statements and analytic value-judgments alone.

usually be within the general competence.

Consider the second possibility - that the economic question is reduced to detailed particular ethical questions within the general competence. It is clear enough that the form of the questions would be enormously complicated (unless the economist introduced some simplifications - judgments of approximate irrelevance, for instance - which would be equivalent to the surreptitious introduction of value judgments). If we consider our particular case: "How much should be invested in blast furnaces in 1965?" - it would be necessary to do something equivalent to portraying the whole state of the economy for a large number of possible values of the (continuous) variable "investment in blast furnaces 1965", and then asking - which of these ought we to arrange for? Thus, in order to pose the question at all, it would be necessary to expound to our layman all that was involved in different rates of investment in the steel industry. If we finally understood the exposition, he would have become - at least as far as a part of the steel industry and its connections with the rest of the world economy are concerned - an economist himself. In order to deal with the question, even its "reduced to ordinariness" form, his competence would have to be extended beyond the general competence.

This argument shows, I think, that - even if one accepts the much too sharp distinction between positive and normative questions - the notion of technical competence is as relevant to normative questions as it is to positive ones; and also that competence in dealing with positive - i.e. empirical and logical-definitional - questions is necessary (though presumably not sufficient) for competence in dealing with normative questions in the same field. Hence a science of economics that eschews value-terms will fail to answer or discuss certain questions that people want answered and discussed, which, indeed, ought to be answered and discussed, and which will not otherwise be competently discussed.[1]

This argument is sufficient to rebutt Graaff's position; but one can, I think, go further. A sharp disjunction of economic discussion into two parts, positive and normative, is highly misleading. Those who have recommended such a division (e.g.(Friedman 1953), ch.1) have, at least in part, been influenced by a desire to see economists more aware of the logical status of what they are doing and less prone to insinuate objectionable values into their work without proper acknowledgment or discussion. With that we need not disagree. But compartmentalized discussion is dangerous in the social sciences, and so, in particular, is an artificial attempt to arrange the discussion under positive and normative heads. The choice of the questions to be studied, the definition of terms, the selection of causes and factors worth consideration, can all have value implications. An infinite number of positive propositions in economics could be asserted. We must choose which to propound in the light of the questions that it seems important to ask, and of the considerations that seem to deserve most attention in seeking an answer: for the economist, these choices almost always raise moral issues.

(4) The empiricists want more facts. At an earlier stage of the argument, I remarked that very stringent rules would have to be imposed upon an economy if the Pareto-optimum state were to be unique. Certainly the problems might become much more tractable if the states of the economy that are admissible in the model are considerably restricted, so as to reflect some understanding of what is possible in actual economies - or what one chooses to regard as possible. Many

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(1) On these questions of moral language, see (Hare 1952), esp. Ch.4 on principles.

economists, for example, believe that practically speaking the proper policy in regard to the rate of investment for most economies is to maximise it, because in any case political difficulties will be such that clear economic benefits would result from increasing the rate of investment beyond any politically possible level. (1) The most immediate reply to such an argument is that precisely this point has to be proved. One might deduce as a practical corollary from a theory of planning that the greatest practicable rate of investment is the right one: to assume that it is so does not constitute a theory.

However, partly because hard facts are respectable in economics, partly because we might be able to escape the nagging worry that preferences change greatly with changes in economic organisation (which might be implied by changes in the pattern of production), it may seem possible to clear up the mess by making much more detailed assumptions about the economy. Mishan seems to take this view: "What this subject badly needs is a strong infusion of empiricism to end its unchecked wanderings in the empyrean and to bring it down to earth feet first." ((Mishan 1960), p.251)

Such a programme amounts to tackling prescriptive questions in a hierarchy. At the top are those awkward questions about social structure and economic organisation - what culture, which major institutions are best, or better. We

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(1) The argument that the rate of investment should be maximized has been put, in the context of the optimum rate of saving discussion, in (Horvat 1958). There, however, the argument rests on the assumption that the rate of growth cannot be increased beyond a certain point, and that this is achieved when the rate of investment is some reasonable fraction of the rate of output. The argument that, on this assumption alone, the rate of growth should be maximised is, to say the least, obscure.

assume these solved or somehow ignore them, and come to the lower levels where such homely matters as investment in supersonic airliners, and the proper price for parking meters, are to be discussed. Surely such a programme is not convincing. Is not one of the most troubling features of economic analysis that we never know what can reasonably be taken as given? Pricing policy on the railways may not affect social mobility or the education system today; but the cumulative effect of a number of minor decisions all carefully taken on the supposition that the social institutions further up the hierarchy remain unchanged could be large changes in style of life, location of power, the form of important institutions, the temper of culture. It would be convenient if the consequences of economic decisions became less important as the years since they were made increased. But what are we to say about decisions to found great cities, or to develop new sources of power?

Of course something like this hierarchy will be necessary no matter how we set about dealing with welfare problems. We must hope that a determination not to forget wider issues will guard against some of the dangers. What is not justified is to accept without question a certain framework within which the small questions will be studied: that would be to assume that the special competence of the economist has nothing to say about the wider issues.

\- 4 - Since I do not find any of the reactions outlined in the last section either convincing or particularly useful for the task in hand - namely the analysis of typical problems of economic planning - I must suggest an alternative way of thinking about these questions. The theory I shall outline is itself open to a number of objections, but I think it does provide some hope of approaching many important questions, which might otherwise be left undiscussed, although they are not matters of mere opinion or prejudice. What I am going to suggest is not so much a new theory as a more or less new justification for the way that some economists have in fact felt it convenient to discuss questions of the type I am interested in. Those who would disagree with such a method will no doubt continue to disagree, and those who would naturally employ it will probably feel that it is unnecessary to cloak it in any semblance of philosophical grandeur; but I think it is worth explaining why I use the methods I do.

I begin by making and discussing a number of assertions about ethics. Our task is, of course, to discuss questions that are at once moral and technical; in particular, questions of the form "What should be the value of the economic variable X?" I take it that if such questions are of practical interest, they raise questions of value that are definitely moral.

(1) I hold that rational discussion of moral questions, and therefore also of questions involving moral considerations, is possible. Some believe that value-statements are arbitrary, conventional, or a matter of prejudice, and that a study of a moral question, as to which one is puzzled what opinion to hold, can only attempt to show how the answer would be related to one's other opinions and prejudices (which are not subject to discussion and criticism because they are arbitrary, conventional, or a matter of prejudice, and can be nothing else.) This position is not a thesis about the meaning of value-statements, I suppose; for we do not mean by such a statement as "It is right to give up all one's wealth" that we <u>think</u> we should give up all our wealth, or that we want to give up our wealth, or that we hold the arbitrary position of denying wealth; and in any meaning of "prejudice" that I can find useful, there are statements of this form that are not prejudiced -

indeed it is possible for any value-proposition to be unprejudiced, since prejudice is a property of statement and context, not of the proposition alone. The thesis must therefore constitute a value-judgment upon value-judgments, which consistency demands that it should avoid making.

It seems to me that value-statements fulfil a very important function in allowing us to ask for and give advice, and to discuss and criticise decisions. Presumably they would not be used in a society where the members all pursued wildly divergent aims - supposing such a society could exist - but questions of economic development and planning would be unlikely to come under discussion, if discussion there were in such a society. I shall therefore proceed on the assumption that moral terms are worth using, and moral questions worth asking and discussing.

It is particularly worth emphasising the familiar fact that when a man makes a moral statement - or any statement involving value-words - it is always natural to ask for reasons: the man would display his misunderstanding of the words if he took the position that no reasons could be given.(1) This implies that another man's value-statements are always open to question (logically, that is) - and therefore that rational discussion is possible.

This does not mean that such discussion would be easy. For - to judge but roughly - the man in the street is not nearly so ready, nor, consequently, so able to discuss rationally moral issues, as he is to discuss football form,

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(1) Recent works emphasising this include (Hare 1952) and (Nowell-Smith 1954). Of course a man may find himself unable to give reasons, either through incompetence, or because he has not in fact given the matter thorough thought. In that case he ought logically - at the best - to retreat to saying "I think that X is right", or "I believe that X is right (sc. even although I cannot at this moment give any reasons that either of us would accept as valid.)"

the best route for getting from Edinburgh to Exeter, or the relative merits of different holiday resorts. Moral discussion, like medieval theology, may atrophy for lack of practice - though perhaps because of too personal an interest, rather than for the lack of any interest at all.

(2) A particular difficulty for rational moral discourse - not unfamiliar in the history of the natural sciences - is that its resources have to be brought to bear particularly when the problem in question has some novel characteristic, or has some aspects which have to be considered for the first time. At least one of the functions that language is called upon to fulfil is that of articulating, reporting, and discussing new situations and new experience. This notion of newness is quite easily demonstrated by referring to a very simple society with a very simple language which is completely adapted to the tasks for which the members of society are accustomed to use it in dealing with one another and the furniture of the world they know. We may then imagine what happens if some entirely unfamiliar circumstance arises, or some entirely unfamiliar object appears. Definition in a more complicated world would be more difficult, but I shall assume that the analysis could be carried out.

The importance of this point for us - if it is a fair point - is that the problems of economic judgment and planning seem in many ways different from other moral problems with which human beings have been accustomed to deal. Of course there are many resemblances too, which is just as well, since otherwise I suppose we could scarecely hope to say anything about them at all; and indeed it is not at all clear why we should care about them if they were totally unrelated to the world and concerns we know.

But even the differences there are mean that the language used for dealing with moral questions must be extended in some measure if we are to have a discipline that can be applied in the field of economics.

To some extent, the extension will be performed by using general principles derived from other more familiar questions of value and morality. For example, our judgment of what factors are to be considered in studying a question of economic planning will be derived from our experience with more restricted questions, such as social work, medical research, and government administration; and also with the factual knowledge and substantive generalisations developed in, say, psychology, sociology, and certain parts of economics. But the novelty of the questions we have to deal with consists not so much in the wide range of presumably relevant factors, as in the nature of the actions that are contemplated (or of the imaginary choices considered, supposing no action is directly intended.) These raise problems more within the competence of political science and the study of administration than within the traditional bounds of economics; but they also raise questions that we are accustomed to find treated by economists in the economic journals, and their novelty consists ~~in~~ largely in their being quantitative and mathematical in form. The questions planners ask have become, not "shall we concentrate on heavy industry at the expense of agriculture?", but "how much of the available resources shall we devote to heavy industry and how much to agriculture?" We might try - although I think the attempt would be unsatisfactory in many important respects - to compare the imagined economies resulting from concentrating on the two extreme policies, just as we compare two paintings of recent vintage, wondering which to buy, and trusting to the taste that has been trained on the old masters to guide our choice. But if we want a numerical answer, this will not do; no process of trusting to the unconscious operation of our educated moral taste will allow us to extend our competence from the familiar and qualitative to the complex quantitative problems of economic planning, and that is probably just as well, since few faculties are so worthy of careful formalisation and conscious ratiocination as the way in which we value different social arrangements.

(3) Whatever principles we use, and however we use them, their application is not to be mechanical. We can expect to learn by using, just as, when driving a car, the general principles we accepted when first we learned to drive are modified in the light of accumulating experience with varied driving conditions. If a particular shape of analysis is developed, it will no doubt be better suited to some economies than to others; it will change out of recognition as the centuries pass. In developing an economic theory, however, we should want to arrange it so that some parts - the less general - are more readily to be modified than its most general features, even although the whole theory is in some sense to be likened to a general moral principle.

This question of discussion, correction, and development of any theory designed to deal with typical planning and evaluating problems is clearly important. Three propositions particularly deserve emphasis; I hope they will be accepted:-

(a) It will not be possible to develop an adequate theory that can be shown to follow logically - that is according to the rules of deductive logic - from moral and empirical propositions, such that the moral propositions refer implicitly to the already known situations, other than the novel quantitative problems we have to deal with. For the questions we have to deal with are not logically equivalent to questions of any better understood kind. (This is familiar in empirical sciences, where essentially new data have to be encompassed by new laws.)

(b) How to discuss, criticise, and modify a particular model and theory for a particular situation has to be learned. Clearly the usual rules for logical consistency, consistency with empirical evidence and with well-substantiated natural laws, will apply, as will certain rules of ordinary moral discourse. But to some extent they are supplemented out of the growing understanding of the relation between problems, consequences, and methods in the new field. (This is analogous to what we may suppose happens when two reasonably liberal-minded Christian missionaries go to live in a society with customs in many ways unlike those they are familiar with. In their mutual discourse they will develop ways of talking about the society that will involve moral judgment of situations they had not previously known.)

(c) But however the canons of discourse are learned, the appropriate <u>criticism</u> of a particular plan, a particular planning method, or a general theory of some planning problem, would be that it leads to prescriptions for actions that would not be the best possible, or that the prescriptions could be much improved. Just as a theory in natural science is to be contradicted, if at all, by a fact contrary to one of its implications, so a theory dealing with moral issues is to be criticised for being morally bad (and not, e.g. because it fails to follow from "commonsense" propositions.)

-5- In this section I want to describe a particular formalism that can be applied to planning problems.

I think that it may be logically the most natural, but I must mention at once a possible objection to it, which cannot be answered in this thesis: that, if applied to actual planning problems in such a way as to take account of all the main features by which an economy should rightly be judged, it would be too complicated for results to be calculated with sufficient efficiency (e.g. in time to be used), and too complex for any ready agreement on the details until the particular situations had long passed. I must wait and see; in any case I know of no theory that promises to deal with the important factors in a simpler way, and to provide us with a conceptual framework within which the statements of economists and the activities of planners can adequately be judged.

I consider how to set up a model for a particular situation, by which I understand a particular economy at a particular time. Since we are planning for the future, all future time must be included; and uncertainty about the future must also be dealt with. Details about these aspects will accumulate as the thesis goes on. At the moment it may be simpler to think in terms of an economy whose development would be broadly known if it were known what decisions the planning authority were going to take. In setting up the model, three sets of decisions are taken:

(1) It is determined which variables will be the subject of decision, i.e. what the planners are going to do, or at least what variables the planners hope to influence directly. (For example, the allocation of foreign exchange to fertilizers and foreign advisers.) These are the <u>planning variables.</u>

(2) It is determined which variables are to be considered valuable in themselves - i.e, which it is that the economy is supposed to be providing. (For example, the part of agricultural production available for consumption.) These variables will be called <u>significant variables.</u>

In both these stages of setting up the model, problems of defining the variables crop us. For example, however detailed the decisions of a central planning authority, it is in practice inevitable that the decisions will be instructions (e.g. level of output in the steel industry) which must be further subdivided in the taking of economic action. And the variables defined under (2) must also be aggregative - for example, the level of consumption of foodgrains by designated social groupings, rather than the detailed consumption of individuals. Also many of the variables would be representative of, say, the facility involved - clearly 'education' is not a numerical variable.[1] It may be objected that such a procedure is arbitrary, since we have no means of aggregating except conventionally, and one convention is as good as another - or is it? Certainly value-judgments are involved in our definitions (if we measure soap by number of bars rather than by current market price, we are going to be slanting things against the more luxurious brands or forms). But it is only reasonable that part of the ethics should be borne by the procedure of defining the variables. In practice one's definitions would be a compromise between available or obtainable statistics and one's judgment of the best

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(1) It seems to me that the social sciences have an important task in establishing good indexes for facilities. An analogy from another field is the IQ index, and other less familiar calibrations of psychological tests.

variables to use.[1] In the final stage,

(3) it is determined how the defined variables will be balanced against one another. These variables have been defined so as to be numerical: we describe the process of balancing them by saying that we attempt to maximise a function of them. I shall call this function the valuation function.

For example, we might decide that the planning variables are the additions to fixed capital of various sorts in various places now and at all future times. (This decision has moral implications, in that it determines the spheres of society that will not be subject to direct planning instructions.) And we decide that the valued variables are the levels of consumption (in broad categories) at all future times, collectively denoted by C, and indexes of the provisions of facilities at all times, collectively denoted by F, (including within it working conditions). The valuation function would then be $V(C,F)$, and the recommendation would be that the country ought to choose the additions to fixed capital so as to maximise $V(C,F)$.

On the face of it, this appears to have at least a formal resemblance to a kind of utility function that economists have occasionally used in their less rigorous moments, and which was used in particular - under a different name - by Frank Ramsey (Ramsey 1928) with the élan more natural to a mathematician. Originally the utility function was a measure of the pleasure-minus-pain obtained by the individual from particular collocations of consumption goods, etc., and played in welfare economics precisely the role fulfilled in more recent

(1) Certainly the first approximations would be price-measured statistical aggregates.

expositions by preferences, indifference maps, and the like. Thus the rigorous economist will be reluctant to see it extended cavalierly to aggregative variables, so as to be formally the same as the valuation function I have just defined. Nevertheless, some have felt that the approximation involved in so doing was not serious.(1) It may then be used in an explanatory way, as suggesting the way in which people are likely to act; or there may also be some measure of prescription involved. For whatever purpose it is used, the direct connection with the preferences of the individual consumer remains.

One way of distinguishing the valuation function from the utility function is to say that this direct, yet ill-defined, relation with individual preferences is severed. I am defining it by the use to which it will be put, the role that it will play in the derivation of ethical prescriptions from selected empirical propositions. A particular form of the valuation function is a particular expression of general moral principles, and could indeed be called a general moral principle (though it is logically not a statement, but a term.) Whatever knowledge is available about individual preferences may be allowed to affect the particular choice of the valuation function, or not, according to whether it ought to be taken into account or not. We may think of the valuation function as a bridge-term, providing a link between empirical propositions and judgments. Language is full of bridge-terms - the everyday

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(1) Cf. (Tinbergen 1960), ~~(Harrod 1960)~~, where estimates (by Frisch) of the marginal utility of aggregate consumption are used; and (Harrod 1960).

moral words, for example - and we could scarcely act without them (since there is no logical connection between what is the case and what we ought to do). The valuation function is not, however, like words in everyday language, for it is a mathematical term, working in a way that words do not: it is a variable, the particular form being provided in particular cases.

Because of its historical roots in the utility function, some will be tempted to think that a valuation function must belong to some-one. "Whose valuation function is it?" they will ask. It must be emphasised that this is a mistake. Valuation functions belong to no-one, although anyone may use them. If a man says, "These are my principles," it is more sensible to regard him as belonging to the principles than the principles as belonging to him. We do not establish our beliefs as worthy principles by asserting that they are ours, but only by arguing that they are right. To speak as though values must belong to someone is to imply that moral language has no function, and this I reject.

The particular form the valuation function ought to have in particulars cases is something that would have to be learned, although in fact a good deal of economic discussion is already relevant to the question. I am not at all suggesting that any substantive problems of morality are solved simply by defining the role of the function - it merely provides us with a means of expression. I am sure that different forms will be relevant to different situations: something will be said in the next chapter about the forms that may be appropriate in a broad class of situations, if not to the living man, at least to the theorising economist.

But it is worth outlining now how we may hope the approach outlined above will allow a satisfactory treatment for the many points in a theory of planning at which the traditional welfare economics was found wanting. The theory is now avowedly approximate from the beginning, so the first objection to welfare economics, that it attempts to be rigorously precise where neither the nature of the questions nor the complexity of the economic world will allow precision, does not apply. The other sources of difficulty were found to be (1) the notion of preferences and the impossibly complicated manner in which they had to be combined; (2) external effects, facilities, and so on, which could only be dealt with when the combination of extremely detailed preferences was specified by something like the social welfare function; (3) uncertainty, in all its manifestations.

(1) The implicit assumption of the traditional welfare economics, that each individual's preferences should be allowed to affect the optimum in a pre-arranged manner, has been abandoned. A totalitarian disposition is not necessary to assent to this rejection - it is quite sufficient to find the standard preference theory unhelpful for problems of planning, for the reasons suggested above. I do not exclude - quite the opposite - the possibility of allowing <u>such knowledge as we actually have</u> about people's preferences to influence the choice of valuation function (and, for that matter, the choice of planning and valued variables as well), and the possibility of deliberately leaving parameters in the function to be determined by referendum, consultation, parliamentary vote and the like. Indeed I believe that the method of analysis being put forward makes it easier to think of how, in practical terms, deserving preferences may be given effective voice, and provides a better weapon for showing where preferences or values that are widely held are less effective than they should be.

It seems to me that economists, insofar as they have been concerned with problems of economic welfare have, on the whole, created more problems than they have solved, and indeed distorted many important issues, by their attempt to pass the moral buck on to an economic man, of whom we should object, not that he is too concerned with money and material objects (though this was a true enough objection to many of the arguments), but that he is endowed with powers of superhuman foresight and understanding, (which none of us would wish to be cursed with.) The introduction of the valuation function forces the discussion to be carried on on the ethical plane where it properly belongs, and prevents us from believing that all the problems of valuation are rightly to be solved in the same way.

(2) Distribution of income, external effects, facilities, and so on, are to be dealt with, as I have already suggested, largely by the proper choice of significant variables; and also to some extent by the proper choice of planning variables. To take one example, that of distribution: the first question must be, how much confidence a planning authority would have in the right working of the economy in distributing the goods once produced. If it is confident, then the questions to be answered by analysis are, how much of the goods to be produced, given these existing distributional arrangements. Some aggregate variables will be used to represent total production of the various goods that will be distributed (rather than available to those who care to use them), and the valuation function will include them among its arguments. In any case, we may note, if there is a problem of wide discrepancies in the distribution of spendable income, the rich will usually spend their income on more expensive versions of the goods and services

consumed by the poor, as well as on goods and services fulfilling wants which the poor cannot: hence up to a point, there will be effective redistribution if the right amounts of the different goods and services are produced, and the rich consequently have to pay more for those that they want. Clearly other measures are possible, such as rationing, free distribution of basic commodities, and so on. When it comes to unequal distribution of access to facilities, however, it is clear that more variables will have to be defined, and investment policy will as a result be modified so as to deal directly with the problem.

Finding representative variables for facilities raises many difficult questions: it is a subject of constant concern to sociologists, although it does not seem to be given as careful consideration as one would wish, despite the large amount of survey work being done. Even such a comparatively simple matter as housing (1) is not easily dealt with: the number of houses is not an adequate measure. Variables like average distance from work, average number of rooms for different social groups, and the geographical distribution of these variables, would all be relevant; and much more besides. But at least we have acknowledged that our science is approximate. It will always be open to improvement, but at any time it may be possible to make do with quite rough and ready tools. Beyond suggesting the wide and fascinating field that opens up if we try to define few enough illuminating and significant variables to reflect the facilities in the economy, I want to say very little more about facilities in this thesis, important though they are. I believe it is important to be

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(1) The home, of course, is an extremely complex facility, but I take it that we do not want the planning variables to be directly relevant to it - in most economies at any rate. Housing, insofar as it comes within the sphere of economic judgment and planning, is relatively simple.

aware of the penumbra of further problems that cluster around any particular problem; but, at least in the present state of the subject, it is necessary to restrict oneself to very specialised cases. My chief concern in this thesis is(3) the various dynamic factors that the traditional economic theory had to neglect, especially technical change and uncertainty. In later chapters I shall construct and analyse a simple model that seems to me to throw up in sharp detail some of the most important features that the acknowledgement of change and uncertainty will introduce into economic evaluation and planning. In so doing, I shall be showing - and this seems to me extremely important - that it is possible, working within the theoretical framework I have outlined - to study change and uncertainty in a fruitful way, and at the same time to remove the worst puzzles about time-horizons and terminal capital.

- 6 -

The alternative programme to the traditional welfare economics is, then:

(1) To study and understand the economy in question;

(2) To decide what classes of actions can and should be taken;

(3) To construct significant variables and a valuation function;

(4) To analyse the resulting model so as to discover the optimum decisions, or at least to find reasonably good decisions.

The last part of the programme must inevitably be highly mathematical; and it must form the main part of any general theory. I shall concentrate on it in the rest of the thesis. It may be possible when particular cases are fully worked out to understand better the earlier stages in the programme and improve them. In any case, I think that detailed study of the first three parts of the programme requires first-hand knowledge of the particular economy; and is consequently a very different branch of economics.

The reader might well want to know more about how I would envisage actual planners using the programmes I have outlined, or its results; in particular how I think they could intelligently arrive at decisions about the definition of variables and the construction of valuation functions (which are not within the general competence to handle.) But I will be wise if I remain on a more theoretical plane, and simply ask my ethical questions: what ought to be done? I am, indeed, concerned lest those concerned in economic planning should acquire a solely technical morality, and implicitly deny their responsibility for the wider significance of their decisions. It is clear enough that all professional groups do acquire a technical morality, concerned with the efficient execution of their particular technical problems (the honesty of the bank clerk, the secrecy of the atomic scientist, the careful reading of the don). But, as with other professions, though in a more important degree than most, it is important that the decisions taken by economic planners should be taken in the light of wider issues, taken indeed with moral responsibility and imagination. There is a further danger that those responsible for economic planning will, in the absence of a theoretical discipline, wed their moral energy to superficial programmes rather than to the fundamental principles on which they ought to be based.

If there is a conflict between two cultures, it is between imagination and technique. Doubtless any coalition will be uneasy at best. Obviously such a coalition is precisely what planned economies need. I am anxious to emphasise the importance of moral intelligence and imagination now, for this thesis is overweighted with technique; nevertheless I believe that the programme I have outlined begins to give imagination its proper place, without at all denying the importance of technique.

CHAPTER II MACROECONOMIC PROJECTIONS

- 1 -

A programme for examining planning problems is one thing; its concrete manifestation in the treatment of particular problems is quite another. Even if something like the programme urged in the first chapter is accepted, it still remains to consider how it is to be applied when we come to consider actual economies or - as in this thesis - simplified features of actual economies that are of sufficient interest to merit more generalized treatment. I have already indicated that I regard the quantitative nature of most planning decisions as determining the language appropriate to discussing them. Mathematical techniques have to be used. The choice of these techniques, and their justification, is the theme of this chapter.

It will be necessary to touch on certain difficult and contentious matters: among them, the use of production functions, the nature of statistical inference, and rational decision-making under uncertainty. A full justification of the methods I shall be using would require a full discussion of these and other matters, and the definitive solution of questions that better minds have found intractable. I can only hope to suggest that my standpoint is a possible one, elucidate the meaning of what I hope to do, and discuss some of my special assumptions. For that, superficial notice of the wider questions will have to suffice.

The central problem may be briefly stated. Planners [1] have certain information. On this basis, they must value the economy, or - more generally - the state-of-things. Part of the information is the actions they may perform: what is sought is an evaluation of these actions, to be achieved by valuing the resulting state-of-things. The planner needs a method whereby a value can be associated to any possible collection of information. He can then apply it to his particular knowledge in order to value the state-of-things as best he can.

There seem to be two major difficulties in the way of producing such a method. In the first place, the kinds of possible information are so diverse, and so liable to change and expand, that it is not possible to give a general scheme for describing the information. Therefore the method cannot be expressed in a completely formal manner. Secondly, it is not possible to lay down firm criteria against which any suggested method can be judged. The best method is itself a matter of value-judgment; and ethical argument cannot be precise enough to be expressed in terms of firm criteria for such very general matters.

My procedure is to describe, somewhat roughly, the method that I would recommend; and then to say something about its justification. The method is not original (except in some minor details). It is rather easier in this field to suggest an off-beat method than to appreciate the merits of the more familiar.

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(1) The word "planner" is used to make the discussion a little more vivid. I have already remarked on the danger of assuming that values must <u>belong</u> to some one; equally, the rightness of an action does not depend on its being actually done. The existence of entities called "planners" is not necessary to the validity of the discussion. However, awkward circumlocutions are avoided if one speaks as if the right actions are performed by someone.

- 2 - I have already suggested that information should be used largely to formulate relations between certain <u>significant variables</u> and the <u>planning variables</u>. The problem of defining a valuation function on the body of available information is thus divided into two. Ethical argument is involved mainly in defining the significant variables and specifying a valuation function. Statistical arguments, and the like, are used mainly to produce production relations; that is, the relations between significant variables and planning variables.

This division of the problem may be justified by the following argument. If planning actions were valuable in themselves, there would be little need for a theory of economic planning. It is in the nature of economic actions that their benefits are in some degree, and often to a considerable extent, separated from the actions. On the one hand we have the organizational, mechanical, biological form of the productive actions; on the other the human appreciation of the results. The introduction of significant variables amounts to an acknowledgement that economies are not worthy in themselves, but only in terms of what they provide.

The planning variables and the significant variables both have a time-structure. It is difficult to think of ~~these~~ cases where a planning variable would not refer to a moment or period of time. Actions, after all, have dates. The case of significant variables is slightly different, for it is possible to think of significant variables that might be defined independently of time, and indeed they are quite popular. A fashionable example is the asymptotic rate of growth of the economy (as measured by some statistical feature, such as net national product, or private consumption.) Some speak as if it were plausible to make this the one overriding significant variable.[1] Again it has been suggested that one ought to minimise the time to some stated objective. The objection to such as these is, I think, serious: they are not linked to our experience, and we therefore hardly know what to make of them. One can appreciate the need for time-minimization in the context of a journey to the bedside of a dying parent, or, indeed, the end of a painful last illness. Such situations do not strike me as adequate analogies to the development of economies. The same is true of an attempt at a world speed record (even one taking an infinitely long time): nothing is more remote from those features of an organized economy that make its existence worth while. Time is, as Kant wrote, "the form of the internal sense", and it seems unwise to ignore the flow of experience as an appropriate model for the time-structure of the significant variables.

It seems to me, indeed, that similar significant variables should be defined for each moment (or period) of time. If the consumption of high-grade protein is of significance in 1963, so it should be in 1993. (The possibility that it will not be is something that present knowledge forces us to neglect.) Practical considerations may very well modify this rule. In practical planning at the present day, it seems to be desirable to have a more detailed structure of significant variables for nearer than for more

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(1) Cf. (Robinson 1962), p.224, where it is suggested, albeit with some reservation, that a disparity between the rate of profit and the rate of growth may be adduced in criticism of an economy (when certain other assumptions are satisfied.)

distant years. Clearly this involves a modification, not an abandonment, of the principle.

The time-structure of significant variables may conveniently be called a "time-stream" - of kinds of consumption, facilities, or whatever. I shall say more about the valuation of a time-stream of significant variables in the next section. What is proposed - and used in the central chapters of the thesis - is a time-additive valuation function, constructed by summing valuation functions for different periods (or integrating an "instantaneous" valuation function referring to a single moment of time). The instantaneous, or period, valuation function, is to have the same form, as a function of the significant variables, at different times (subject to the necessary modifications when the detail of the structure of significant variables changes with time); but it may, nevertheless, depend on time. For example, we have the simplest, and therefore most important case, in which the valuation of the economy is

$$\int_o^{\infty} v(c_t)e^{-rt}dt \quad \text{or} \quad \sum_{t=o}^{\infty} v(c_t)(1 + r)^{-t},$$

c_t being a statistical measurement of "consumption" in the year t. These are scarcely unfamiliar expressions; but I am, after all, trying to justify the familiar, as well as showing how it should be generalized.

The most important aspect of the relation between planning and significant variables is the relation between the time-structures of the two. Economics is a sufficiently old subject to be able to give sensible guidance. It is characteristic of production relations that they are not only somewhat complicated, and imperfectly known, but also that they involve time-lags.

The effects of investment decisions reverberate through time, and never vanish altogether, for even though machines may rust, and buildings rot, their past existence has made other acts possible, whose consequences outlive them; and so on. It is unlikely that it will ever prove possible to obtain really precise information about investment lags, and the persistence of investment effects. At any rate, methods of approximation have been developed. The best known, shall I say the most notorious, is the macro-economic production function. It is not as old as the epithet "neo-classical", usually applied to it suggests: for the possibilities of using statistical aggregates were only gradually realised as they began to be measured. Now, the linked notions of net investment and the stock of capital provide almost the only serious technique for predicting future production.

The neo-classical model is really rather unsatisfactory, not only because the subtle dynamics of technical change can be only roughly reflected in it, but also - probably this is more important - because the deterioration, replacement, and co-operation of capital goods raise many problems that the straightforward neo-classical formulation assumes away. However, both these classes of problem are so difficult to deal with, that we must be grateful for a method that allows us some broad sense of the process of development. Uncertainty is another feature that the ordinary capital models ignore; but this can to some extent be incorporated, as I shall show.

In its simplest form, the neo-classical production relations are:

$$c_t + \dot{k}_t = f(k_t, t),$$

where $\dot{k}_t$ is the planning variable, "net investment", being the rate of change of k_t; and c_t is, as before, the rate of consumption. More will be said about its use and limitations in later chapters, especially in chapter V. Meanwhile, I want to discuss its characteristics as a means of <u>prediction</u>.

A distinction that seems to be demanded by the possible production models that planners can use is that between models employing probability theory and those which do not. I shall call production relations that express the significant variables as sure variables (that is, they assign a single value or estimate to each) <u>predictions</u>. When, <u>per contra</u>, the production relations assign to some significant variable a probability distribution, I shall say that that variable is projected, and call the production relation a <u>projection</u>. It is sensible to regard a prediction as a special case of a projection. Whether there should be any other sort of production relation is an awkward question, which I shall take up shortly. Broadly speaking, predictions should only be used when (a) they are as good a probabilistic model as any - a rather rare situation, or (b) ignoring the probability distribution seems unlikely to make a significant difference to the final result. Presumably (b) is quite a common situation; but obviously it requires justification, which it seldom gets. These dogmatic assertions will be discussed a little more in the section on the use of probability theory.

A projection relates planning variables to a random variable. This will have to be done for corresponding random variables at each moment of time. A natural way to do it for the neo-classical type of model - and this technique is easily generalized - is to transform a sure production function $f(k)$ into a random production function $F(k,t)$ at t. The transformation $f \longrightarrow F(.,t)$ can be called a projection transformation. For example, one very simple form is

$$F(k,t) = f(k)e^{\alpha t + \mathcal{E}_t},$$

where $\mathcal{E}_t$ is a random variable. Furthermore it is convenient to make $(\mathcal{E}_t)$ a Brownian process; that is, to regard $\mathcal{E}_t$ as the sum of a large number of small normal random variables[1]. $\mathcal{E}_{t+\delta t}$ is obtained from $\mathcal{E}_t$ by adding a random variable $\mathcal{E}_{\delta t}$, independent of $\mathcal{E}_t$, that is normal with variance $\sigma^2\delta t$, and mean $-\frac{1}{2}\sigma^2\delta t$. $\mathcal{E}_t$ has mean $-\frac{1}{2}\sigma^2 t$ and variance $\sigma^2 t$. It is easily verified that with these assumptions, $E(e^{\mathcal{E}_t}) = 1$.

These assumptions give the simplest projection transformation that makes sense in the economic context. They do not seem markedly unreasonable, except that the increments of the $\mathcal{E}_t$'s ought not to be entirely independent. Economic time-series seem to have a "self-correcting" element - leading to periodicities - which may be expressed by saying that some years are "abnormally good", some "abnormally bad", and we can tell at the time. In the simple case, this element is ignored. Something will be said about it at a later stage.

Some other extensions of this simple case will be mentioned later. One point is so important that it deserves to be mentioned now. Economists have naturally spent their effort on production functions for at least two factors, since it is inevitable in the real world that labour should be included among the factors of production. In the previous discussion, it has not been

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(1) Normality is a very natural restriction. Cf. (Doob 1953) p.420. The Brownian movement process is discussed in (Doob 1953), pp.392ff.

mentioned. I am going to leave aside questions connected with labour and population in this thesis, because they require extended treatment, and are not as simple as those here treated. Some of the problems do, however, deserve to be mentioned. Insofar as a growing labour force simply reacts on production through the dependence of production on the factor, labour, no extra decisions are involved, and only the dependence of the projection transformation on time is affected. It is quite possible to regard the equation for the production relation as including this. Quite apart from population changes, however, production may be affected by the consumption of labour, or any of the significant variables. We might have, to take a simple example,

$$c + \dot{k} = f(k,c,t).$$

This could be solved for c: c then depends on $\dot{k}$ in a more complicated way than in the simple neo-classical model. It will be seen that this point causes no theoretical difficulties. The problem of estimating the production function from available information is of course enormously complicated.

One of the reasons for leaving questions of labour and population aside is that they cannot be adequately treated unless certain possibilities of choice are allowed, above all policies of population control, and the choice of less than full employment. The first possibility makes nonsense of the popular exponential population growth assumption. The only good treatment I know is in (Meade 1955). The possibility of deliberately allowing unemployment appears in underdeveloped countries for a number of possible reasons, mainly concerned with the limited possibilities for manipulating the labour market, (1)

(1) Cf. (Sen 1960), esp. Ch.5, (Eckaus 1955). The corrective of (Kahn 1958) is necessary to some arguments.

but also in theory tied up with the productivity of consumption just mentioned. It is important to know how readily the theory can be applied in the poorer countries where it is needed. The answer is that the additional problems are not serious: the necessary modifications will be indicated in a later chapter.

- 3 -

I have remarked that the valuation function is defined by its use. Similarly, the instantaneous valuation function is defined by the role it plays in the construction of the valuation function proper. In the first place, it provides the value-weighting of the significant variables relevant at a particular time. These, it should be noted, may include significant variables that are defined statistically for times later or earlier than that with which the instantaneous valuation is concerned. For example, one might want to include last year's consumption as well as this year's, on the grounds that the value of this year's consumption to the community is relative to the previous rate of consumption. In a continuous-time model, we might represent such a sense of connection more elegantly by including the rate of growth of a significant variable as well as the variable itself. It might even be argued - in the spirit of the new Growth Gospels - that the instantaneous valuation ought to be a function of the rate of growth of, say, consumption, not of the consumption variable itself. While admitting that these are possibilities, I think that there is much to be said for the view that the rate of change of a significant variable <u>ought not</u> to be regarded as valuable for its own sake; and that anticipations and memories, while important for the individual, are so various in their significance as to be impossible to value on behalf of a community. Accordingly I shall regard the instantaneous valuation function as a function of significant variables referring to its own moment or period.

Defined in this way, the valuation function is simply a preference ordering. Students of consumer demand [1] regard such orderings as definitely ordinal. However one instantaneous valuation function has to play its role alongside the others, with which it is in an additive relation. It must therefore be specified in a cardinal manner. Why should we add the instantaneous valuation functions? Curiously enough, the answer seems to be: why not? There is no reason why we should not multiply their hyperbolic tangents, except that it would be inconvenient. Further argument regarding the proper criterion in the presence of uncertainty, population changes, etc. would be affected. But only consistency is required: it is easier to be consistent about addition. [2] To set down the form of the instantaneous valuation function, once has to perform a "thought-experiment" (individually and in groups). The method may be roughly described. Consider two abstract instants, described by different values of the same significant variables, and juxtapose them in the mind. The instantaneous valuation function is to be constructed so that marginal changes in the variables for the two imaginary

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(1) E.g. (Hicks 1939) and (Hicks 1956).

(2) The essential assumption being made when the total valuation is analysed into instantaneous valuation functions is that the preference orderings for the different instants are independent. This is proved for the case of a finite number of instants in (Debreu 1960).

instants that leave the sum of the two valuations constant correspond to changes in the two instants that make their new joint position on balance neither better nor worse than their first. I regard this notion of fair redistribution between abstract situations as meaningful and usable, and a natural expression, and generalization, of such ethical principles as the rightness of treating like people in like situations alike, and the possibility of judging the relative needs of people in different situations.

In this way the instantaneous preferences are to be expressed as a numerical instantaneous valuation function. In general it will be reasonable to assume that it is a continuous function, usually a highly differentiable, even analytic, function. The next difficulty is that the additive operation converting the instantaneous valuation functions into a total valuation function will not in general lead to a finite numerical function. A directed set of instantaneous valuations, one for each moment or period of time will be called a valuation-stream. Once the consumption-stream, say, has been converted into a valuation-stream, we still have a comparison problem to solve when the sum, or integral, of the valuation stream is infinite or undefined. It is quite clear that when the valuation integral fails to converge, it may still be possible to compare the valuation-streams. If, for example, all the instantaneous valuations of the first stream are greater than the corresponding valuations of the second, the situation represented by the first valuation-stream will be preferred to that represented by the second. We must enquire, at the least, how valuation-streams that are of practical interest can be compared; the minimal requirement for an actual problem is that a maximal valuation-stream should exist.[1]

Certainly some people seem to believe in models for which the comparison problem is insoluble: that is, models for which no optimal policy exists.[2] I do not think that I do. At any rate I want to restrict my attention to methods of valuation that admit finite bliss; that helps considerably. I shall say that a method of valuation admits finite bliss if the instantaneous valuation for any time is bounded above - i.e. is always less than some finite number, whatever values the significant variables may take.[3] To see that finite bliss is a reasonable assumption, let us see what would be involved in denying it. We should be asserting that it is possible for the inhabitants of the economy to be infinitely better off (in our judgment) than they are now, in respect of the goods and services represented by the significant variables we can define - i.e. as a result of having more of the kinds of goods and services we can more or less visualise. Perhaps we should say instead of "infinitely better off", "better off in an indefinitely large ratio". In any case, I am quite unable to give my assent to such propositions.

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(1) The comparison problem has been referred to obliquely by a number of authors (e.g. (Massé 1959)), particularly in the context of the choice between competing investment projects. Usually a rate of discount is the cure recommended. Sukomoy Chakravarty (Chakravarty 1962) has, in effect, criticised writings about the optimum rate of saving for neglecting to discuss the problem; his sweeping conclusion that the neglect renders the optimum saving problem with infinite horizon insoluble is too dogmatic, indeed untenable.

(2) E.g., (Tinbergen 1960). Another example will be given in chapter V.

(3) Ramsey was the first to define Bliss. Meade (Meade 1955) prefers the term Glut. As a concession to the overtones problem, I omit to capitalize the initial, except when the exigencies of punctuation demand it.

Furthermore, I suspect that those who have, implicitly, assented to them have not always been clearly aware that they were assenting to the existence of economic possibilities that would justify extreme poverty (supposing it were efficient) for the sake of the far future.(1)

A linear transformation of the valuation function has no significance: so let us subtract a suitable constant from it to arrange that the value of bliss is zero.(2) This is nothing but a formal device: it makes formulae simpler. The instantaneous valuations will always be negative; it looks odd, but is very convenient.

Let us say that a consumption-stream $(c_t : t \geq 0)$ is at least as good as a consumption-stream $(c'_t : t \geq 0)$ if

$$\liminf_{T \to \infty} \int_0^T (v_t(c_t) - v_t(c'_t))\,dt \geq 0$$

$v_t(.)$ is the instantaneous valuation function at time t. This definition need not allow us to compare all possible consumption- streams; but we do not need as much as that. I define an <u>optimal</u> policy in a class of (alternative) policies as one leading to a consumption-stream that is at least as good as any other consumption-stream produced by a policy in the relevant class.

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(1) Keynes was, in a sense, arguing for the existence of finite bliss in his essay "Economic Possibilities for our Grandchildren", (Keynes 1932), though one may doubt whether his grandchildren would in fact have been within reach of economic bliss. The view is not, of course, original to Keynes, and is, for example, a normal part of Christian belief, (although this is obscured by the word "bliss".)

(2) Essentially, Ramsey does so,

If $(c_t^* : t \geq 0)$ is an optimal consumption-stream, we have

$$\liminf_{T \longrightarrow \infty} \int_0^T (v_t(c_t^*) - v_t(c_t)).dt \geq 0$$

for any of the consumption-streams (c_t) with which it is to be compared. The left hand side is finite for at least one (c_t), e.g., (c_t^*) itself. If we redefine the instantaneous valuation functions as:

$$\bar{v}_t(c) = v_t(c) - v_t(c_t^*),$$

(c_t^*) maximizes $\int_0^\infty \bar{v}_t(c_t)dt$ in the ordinary sense.(1) This way of putting the matter is sometimes useful.

Obviously the question of the existence of an optimal policy can be difficult to settle, and will usually involve advanced mathematical tools. For this reason, I shall not go into such questions in the present thesis. The above definitions are necessary to show precisely what I am assuming when I assume that an optimal policy exists. In the most interesting cases, the optimal policy yields a finite valuation (when we make the convention that bliss has zero valuation), so that more elementary ideas are quite sufficient.

It was suggested that the instantaneous valuation function for different times should have the same form. It may, however, change with time. The discounting of future valuations (which are otherwise calculated in an identical manner) seems to be a reasonable possibility. The idea that one should discount future utilities derives, I suppose, from considering the case of an individual who can lend and borrow freely at a fixed rate of interest: then future money

(1) A comment by Professor Roy Radner in conversation suggested this version to me.

values are discounted to give the capital value of the man's prospective net incomes. Naturally this is not a reason for discounting future utilities in any way, but it suggests a means of expressing any preference there might be for present as against future benefits; it seems quite clear that some men act in a way consistent with such a time preference, although most societies seem to have succeeded in evolving a social structure that makes any such preference reasonably ineffective. The question to which we must address ourselves is whether it can be right to discount future valuations in the way that some individuals might want to discount future utilities.

It seems to me that, unless definite reasons can be given for such discounting, it is wrong to discount future valuations. The principle that one should discount valuations simply because they are future I shall call, as it has often been called, pure time-preference. My reason for rejecting it is that each moment (or each unit period) of each man's life, no matter how distant in the future, is equally worthy in itself, i.e. insofar as there are not special, and individual, reasons for giving it greater or less weight than the average.(1) Remembering that the instantaneous valuation is constructed in a particular way, leaning heavily on present knowledge and understanding, there may well be reasons to contradict the basic presumption. I think that there are two possible reasons.

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(1) Many have taken the view that pure time preference is irrational, e.g. (Knight 1921), pp. 130ff. Knight refers to Sidgwick and Jevons as taking a similar position. Also (Ramsey 1928). It has been suggested (Eckstein 1961) that only a communist would refuse to discount the future, but I do not think the remark can be taken seriously. Cf. (Pigou 1952), Ch.III.

In the first place, we are not certain that the world for which we are planning will still be there. Thermonuclear war could destroy mankind, or at least so transform the world that man's past economic activities would have no relevance to his further future. Suppose that the probability of such destruction during a short time interval t is $pt + o(t)$, and that p is independent of any of the economic actions that are being contemplated within the model. Since the momentary valuation of a time after annihilation is not relevant to the decisions to be taken, we may arbitrarily take it to be zero.(1) Then maximising the expected valuation(2) will be equivalent to maximising $\int e^{-pt} v_t dt$, where v_t is the momentary valuation for time t (e.g. the valuation of a consumption index for that moment). In other words, future valuations are to be discounted at a rate p. Whether p should or should not depend on t is more difficult to determine even than the current value of p. It is difficult to visualise political or technological situation that would justify one in assuming a varying p, so perhaps one should project present optimism or pessimism into the future. There is not much knowledge relevant to determining an estimate of p, and the way in which that knowledge should be used is not well understood; it is also extremely difficult to

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(1) Clearly, if p can be affected by the planning actions contemplated, then the valuation of the post-annihilation moments is relevant. If one may draw out the obvious, a negatively infinite valuation of these moments would imply that the minimisation of p is an overriding aim. Clearly governments do not, and certainly cannot, so regard it; and it is not clear that they are wrong.

(2) I cannot hope to justify this rule in the present thesis; something will be said about it shortly.

disentangle the individual attitude to life from the judgment. However it may be safe to say that existing evidence does not justify a probability p as high as 2%, and that one would not be flying in the face of the evidence if it were put lower.

Secondly, just as we may be uncertain as to what will happen in the future, we may be uncertain about our valuation of the future. Clearly the way that the significant variables for some future date ought to be balanced against one another depends to some extent on what these variables are supposed to represent, and on the response of those living at that time to the facts they represent; we are uncertain about both. The momentary valuation function for that future moment of time will be our best guess. The current valuation function, (i.e. the instaneous valuation function for the present moment) is the natural basis for our valuation of future times, since it is based on the most detailed understanding of the significant variables, people's response to them, and current appreciation of their possible consequences. <u>Prima facie</u>, there seems to be a case for regarding the current valuation function as the best guess for future times too, except insofar as we ought to take account of confidently expected changes in tastes (such as a progressive shift in appreciation from beer to wine), or changes in the effects of the significant variables (such as a progressive improvement in the effects on health of consumption of food, drink, and tobacco, as a result of improving scientific knowledge.) Changes in the shape of the valuation function, in recognition of such arguments as I have just cited, would in general be very difficult to specify, especially since it is seldom that one can believe that the balance of probabilities suggests one direction of change rather than another. (Increasing knowledge of the effects of current patterns of consumption on health might lead one to value them less highly in the future.) The most likely possibility is that we should want to assume a steady overall change in our valuation of the significant variables, so that, v being the current valuation function, the valuation function at a time t years hence would be $e^{-rt}v$. This is our second reason for discounting the future.

But it will be readily apparent that we could easily have reasons for making r negative; so that, other things being equal, we should value the same numerical value of a variable more highly the further into the future it is: this would happen insofar as we thought future generations would appreciate economic goods more fully than ourselves; or that they would be less harmed by whatever noxious effects might accompany the economic goods, services, facilities, and operations; or that the likely methods of calculating the significant variables would tend to underestimate such values as increasing variety, improvements in quality, etc. Equally these reasons could be turned on their head; indeed it is probably more likely that the gain from our defined significant variables will fall through time, as possibilities in the world that we had not included in the significant variables, either because we had not thought to, or because they were not clearly going to be affected by our decision variables, take an increasingly important place in the life of succeeding generations.

It may be remarked that r need not be independent of t. However, insofar as r reflects a general uncertainty about future valuations of the kind I have been outlining, it would be somewhat odd - though certainly not impossible - to suppose that planners would at future moments of time be likely to discount the following year's valuation at, say, a higher rate than we are currently doing for next year. If we do assume, as a best guess, that future planners will always discount the following year's valuation at the same rate as we are doing ourselves, then clearly r should be constant:[1] for this reason, when I allow a time-discount (I shall refer to it by this neutral term rather than such a name as time-preference), I shall assume that it is constant.

I conclude from this discussion of reasons for discounting future valuations that there may be good reason for doing so, although it is not clear that the future should always be positively discounted. My own inclination is to side with those who would put the discount rate equal to zero, on the grounds that evidence and convincing reasons for a definite non-sero value are lacking. But practising planners could easily accumulate such information through their experience with the way in which they tend to revise the valuation function from year to year.

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In this section I must deal briefly with some very obscure topics, for it is time I said something to justify my use of probabilities and the mathematical theory of probability, and gave reasons for believing that the rational response to uncertainty is to maximise the mathematical expectation of the valuation. It is perhaps surprising that one should have to justify the use of techniques that have

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(1) Cf. (Strotz 1957).

become standard in scientific investigation; science is, after all, a conventional activity little troubled by fundamental doubts. However it is generally believed that there are uncertain situations that cannot be described in terms of numerical probabilities[1]; and that such situations are particularly to be found among those studied by the social sciences. If I want to advise economic planners and those desirous of judging economic systems to use the techniques of mathematical probability theory, I must say why I do not agree with that contrary position.

The projection transformation is a precisely defined probabilistic model. The first question it is natural to raise is why one should want planners to use a precise formal model. Since probability theory is the only formal language we have for dealing with incomplete information and its relevance to what is unknown (I shall say something about why this should be so) probabilities will have to come in, if one wants to use a formal model, unless uncertainty is going to be ignored (which is a possible policy). Many economists would argue that the uncertainty with which entrepreneurs (and therefore all who take economic decisions) are faced is such that special skill is required to meet it, and that the skill cannot be perfectly communicated or formalised (cf.(Knight 1921)). There are two main reasons for this position; first, that economic situations are essentially unique, in that they cannot be grouped into homogeneous classes- indeed one would

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(1) This was Keynes's position ((Keynes 1921), ch.3, esp. §13); many references are given in (Georgescu-Roegen 1958).

be justified in saying, surely, that each economic situation contains something new; second, that economic decisions usually have an individual significance to the economic agent, in that their consequences cannot be perfectly balanced against one another - each decision is potentially crucial, since it affects the ability of the agent to take further decisions. For our enquiry, it is the first reason that is important, since it may be doubted whether actions that are crucial for the individual are crucial for the society as a whole: however it is certainly true that the collection of all economic decisions is crucial, in this sense, and that we cannot think of all uncertainty being averaged out, so that the choice of the right policy for meeting uncertainty is crucial.

The uniqueness of economic situations and decisions must be conceded. Indeed I hold that there are no significant decisions, inside or outside economics, that can be described in terms of a finite number of known characteristics. But to admit that science must always be incomplete is not to say that it is impossible, and to argue that situations cannot be perfectly formalised is not to prove that formalisation is not useful.(1)

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(1) I find the account of scientific method in (Kneale 1949), although it is only an outline, sensible. See also (Braithwaite 1953). The difficulty in rationalising the scientific process is that it cannot be described in terms of taking a fixed framework as given for ever: any part of the system is logically liable to be changed. Furthermore, new means of expression and types of theory are always being developed. To take an economic example, it would be impossible to give precise rules by which such a country as Holland, which has a continually operative econometric model, should determine when to change the form of equations rather than the values of the parameters, the theoretical framework rather than the form of a particular equation. Since neither life nor science can rationally be discredited by pointing out that they cannot be reduced to rules, I doubt if econometrics can either.

Economics is concerned with the formal description and study of what may in fact take place intuitively, habitually, even instinctively; unless we were willing to formalise, the present study would be impossible, for we should be unable to argue rationally about right and wrong decisions. Of course formalisation cannot be perfect, and that needs saying; neither are the informal methods customarily employed in the real world. Someone may say: so you admit that there is a gap between the real world and your theoretical models; how do you propose to allow this gap to affect the decisions you commend? Not at all. If we could say precisely in what the gap consisted, we could remove the gap. We must try to describe as well as may be with the language - the logical, mathematical, and scientific tools - at our disposal. Beyond that, except for admitting the imperfection, there is no more to be said, until the model is further improved.

Yet that is going too far: for in fact the economist cannot hope, nor can the economic planner, to use those models that are theoretically best. He must compromise for the sake of practicable calculation. To suppose that any method of planning can ever be free of this necessity and to criticise an approach to planning for leaving a place where technical convenience can operate (cf. (Sen 1961)) seems to be mistaken; although it is clearly valid on occasion to criticise planning methods where technical convenience conflicts with important values. The essential point remains the same: there is a gap that one cannot, or has chosen not to, know about; hence it must be ignored.

What of the view that the very complexity of economic situations implies that the experienced economic agent, whether entrepreneur or conscientious bureaucrat, will make better decisions than any mathematician with his necessarily simplified little model? If the composer can inevitably create greater works than any computer, cannot the business man create better works than the theoretical planning model? And if he, or the practical planner, can perform better, what good is the theory of planning either for criticism or for practical guidance? There seem to be convincing grounds for disputing the analogy with the great arts, but there is no need for us to enter into that here. It is sufficient to point out that intuitive decisions must be made on the basis of unconscious values, which are presumably more related to individual interests than to the values that ought to guide decisions with social consequences. Since the purpose of a theory of planning is to bring values explicitly into the business, it cannot be convincingly rejected on the grounds that experience and insight are more efficient - for that sort of efficiency may be in conflict with our values.(1)

If it is accepted that the decisions must be formalised, why must they be

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(1) In my view it is the common fate of the practical arts in human affairs - I think of engineering, medicine, town planning, education, social administration - to be superseded by the scientific enquiries they breed. The progress of logical thought and the growing explicit awareness of ethical issues (e.g. the importance of risk in engineering and medicine) eventually replace fumbling intuition with systematic method which, though still fumbling, is more sure, a better servant. I take it that economic decisions have been in danger of this supersession for two centures, and are now the subject of detailed scientific organisation in many fields. If even the business man finds operations research profitable, the economist is not interfering with a mystically effective process when he asks for different values and more information to be brought into consideration.

It is necessary to correct what I have been saying by pointing out that practitioners of a planning science would inevitably acquire relevant experience and expertise; art would, or should, not be banished - but let it grow out of a framework worthy of approval, and let it be subject to the criticism of scientific discipline.

formalised in terms of probability theory? Naturally this question raises many of the most difficult questions philosophers have had to deal with; they cannot be dealt with in this thesis. All I can do is present my position by way of rough justification for using probability theory.

The following "Law of Nature" has been found a fruitful and reliable hypothesis in the development of scientific theories: namely, that the possible states of the universe can be analysed in terms of independent possibilities which reveal themselves both through the physical symmetries of situations (e.g. the faces of a die, or equal-measure subsets of phase-space in statistical machanics) and through the occurrence of classes of events in regular frequency distributions. Cumbersome though it is, that statement needs much refining. The essential point is that the relation between possibilities, what we might call the <u>physical</u> evidence for probabilities (cf. the analysis in (Kneale 1949)), and frequencies, what we might call the <u>statistical</u> evidence for probabilities (cf. the attempted analysis in (von Mises 1957)), must be considered a contingent fact. It might not have been so. And the existence of this contingent connection between two sets of regularities that Man finds in the Universe provides the basis for the calculation and use of probabilities. That probabilities must obey the rules of the mathematical theory of probability then follows in the well-known ways that have been outlined in many works besides the two mentioned.

It must be recognised that in addition to the two forms of evidence just mentioned, the physical and the statistical, there is a third, which may be conveniently called the subjective. If one wishes to know the probability of some event, the most sensible way to go about it may be to get some person experienced in dealing with such events to suggest the value of the probability. If the language of probability were foreign to him, one could interpret his preferences for different kinds of action connected with this event (I am thinking of gambles) in terms of a subjective probability by the means first suggested by Ramsey ((Ramsey 1931)) and later developed by Savage ((Savage 1954)). Naturally whether the person in question was a competent, consistent and unbiassed adviser in such matters would be estimated (in practice informally - i.e. intuitively or unconsciously) by comparing some of his subjective probabilities with probabilities derived from physical and statistical evidence, when good evidence of these kinds was available. One must be careful not to combine with the acceptance of probability theory the assumption that evidence of this "subjective" kind is not available, or never worth using. Indeed it is very important. It is important because the human being obtains and uses much more information than it can consciously formalise, at least in many fields, of which economics is by no means the least notable. I am sure that in this way of putting the matter I am not differing markedly from Knight's treatment of uncertainty, except in the very important respect that I refuse to recognise his contrast between risk and uncertainty as a contrast between kinds of events considered: I see this contrast as a contrast between the kinds of evidence that may happen to be available to the people taking the decisions, and the ways in which the evidence is available. These kinds and ways might, in any case, easily have been different; hence the distinction does not seem to me worthy of a name.(1)

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(1) Clearly my position is quite inconsistent with any view that holds subjective response to uncertainty to be quite independent of evidence and the structure of the universe - e.g. Shackle's theory ((Shackle 1952) and later works.) It seems to me that it is the relation between information and action that is interesting to the economist; that this relation is often mediated by more or less unconscious mental processes does not seem to justify hypothetical psychologising.

This way of talking about subjective probabilities seems to be at odds with the approach favoured by Savage.[1] A Ramseyan analysis of a man's perfectly consistent and well-considered preferences between the elements of a large class of possible acts (an act being something that associates with every possible state-of-things a defined consequence) makes the man look as if his preferences are determined by his always preferring a higher expected utility to a smaller one, for some utility function and some probability measure on the space of states-of-things. Consistency and well-consideredness are defined by certain postulates.[2] The cardinal utility function and the subjective probability distribution produced by this analysis are not really on a par. The first is "really subjective", the second not entirely. Indeed it can be shown - and it is surely not surprising - that the subjective probabilities will ("ultimately") have to conform to observed frequencies. Thus the Savage Theory is a justification of the usual ways of calculating probabilities. If, however, it is regarded as a method of interpreting actual behaviour, it is not helpful from my point of view. The evidence available to any individual is limited; and it is notorious that people will form opinions regarding probabilities on the minimum of evidence. It may be true that any logical man's response to uncertainty can be described in terms of probabilities; but that in itself is not of much interest to science. Ramsey was careful to deny that he suggested it was.

The Savage school puzzles me. As an attempted justification of probability theory, it somehow avoids considering the real problems. It is not puzzling

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(1) (Savage 1954)

(2) (Luce and Raiffa 1957), pp.299-304 gives an admirably clear and concise account.

that classes of events that exhibit frequency distributions can be described by means of probabilities. To want to explain it by showing how *a priori* probabilities are affected by frequency evidence is to be wedded to an out-of-date epistemology, in which the form of thought is determined independently of all experience. Kant long ago taught us that concepts and percepts are more intimately related than that.

To my mind the root puzzle of probability theory is enshrined in the simple fact that a symmetrical coin tends to fall heads about fifty per cent of the time. Symmetry and frequency seem to be quite different things, so how can they be so closely connected? I have already outlined my own position: they just are. This accepted, it is much easier to *define* probabilities in terms of physical symmetries, but the process of formal definition is a secondary matter: the connection of the two forms of evidence is the root of it all. Of course the Savage theory emphasises the connection between probabilities and action; which is clearly very important, for we would not bother to calculate probabilities if we were not going to act upon them. I think that it does not elucidate the connection in a practically helpful way, for it does not assert enough, as we shall see. Before coming to that point, I must mention another puzzle.

Even if one holds that the formalism of probability theory is concordant with the basic structure of the universe, one still has to ask how the incomplete evidence with which the statistician or economist has to deal is to be used: whatever probabilities our planners use, they will not reflect precisely the basic structure of the universe, so what is the best they can do? Again this question raises many controversies; again I can do no more than state my position - about which I feel somewhat more dubious than in the case of probability proper:

(1) Before the formal - statistical - information can be use, one must have a logical structure within which it is to be incorporated: in other words, a model. Typically this will be a set of assumptions that the variables one is interested in are random variables having probability distributions related to other variables and depending on certain parameters that are unknown. In principle, no doubt, the model should be constructed in the light of any physical evidence one may have (as when one says that the number of times a coin comes down heads in 50 throws is a random variable with a binomial distribution). In practice the construction of the model will depend at many points on informal knowledge and experience (with their attendant unreliability). A model that is found to be consistently unsatisfactory will be changed. As to when one changes the model rather than the "estimated" parameters, very little can be said. It is a more or less incomprehensible fact of life that sometimes one changes the form of the rules one lives by, sometimes only their content.

(2) At each stage, one must assume that whatever models, estimates, etc. one is working with are true - that is, one must act as if they were true, while bending one's efforts to proving them false. One must do this because

there is no other way to proceed.[1]

(3) One should then choose the parameters, with any given model, so as to make the probability (density) of what actually happens - on the assumptions of the model - as large as possible. This is the maximum likelihood rule. Kneale's argument for doing so, seems to me substantially valid (see the last footnote); but there is one serious objection, which will be taken up briefly at the end of the next section.

- 5 - We now suppose that the model has been set up, the parameters estimated: the planners have their projection transformation. They know the valuation to be put upon the future described according to the agreed significant variables. It seems to me, and in this I may very well be wrong, that men can more readily assign value to what they imagine as certain, or to what they have known as certain, than to a range of possibilities, an uncertain prospect. I would not

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(1) Cf. (Kneale 1949), p.234. Kneale is discussing the justification of the maximum likelihood rule for estimating parameters (in the case he is discussing for simplicity the parameter is a probability, but it is perhaps best to refer to an estimated probability as a parameter). In his very interesting discussion, he seems to rest the justification on two grounds: first, that the policy of maximising the likelihood is the one that makes most use of the evidence and therefore invites contrary evidence in the greatest number of possible cases; second, that the policy is the only one that can be used. The first reason rests on assuming that the aim of estimation is to arrive at the correct estimate ultimately, and it is not clear how this ties up with the aims of the modern statistical school of minimaxers, etc., who are concerned with the immediate value of various possible estimating policies (cf. (Luce and Raiffa 1957)), in the various possible states of affairs that may be actually obtaining. But the argument that we *must* make maximum use of the evidence and then act as if the estimate were true seems to me convincing.

deny that men are accustomed to act in the face of uncertainty, and that their experience of action is therefore an experience not of certainty but of uncertainty[1]; but, perhaps because men must value in the light of what they have known, projecting the certain into the uncertain future, it does seem to me that ordinarily men do not value uncertain situations, but rather the separate actual possibilities they may care to contemplate. Most men are, indeed - if put to it - puzzled to know how the fact of uncertainty should affect their valuations of the future. It seems to me that if the value of future situations has been expressed numerically, and the correct probabilities are known, that the value given to the uncertain future should be the mathematical expectation of the valuation of the possible sure futures.[2] I hold this because disagreement is only possible on the grounds that uncertainty should be either sought or avoided for its own sake; and I do not think it ought to be. But this is an obscure and difficult question, which deserves much more discussion than it has ever been given, despite the long history of the criterion.

Let us be clear what the problem is. I have suggested that probabilities are used, and form a respectable language, because of certain basic features

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(1) Dr. C. Lewy suggested this important point to me.

(2) This principle is often known as the Bernoulli Principle. It is discussed in (Keynes 1921), Chapter 26, but inconclusively.

of the universe; yet they would not in fact be used unless they had some use in action, forming a bridge between the information the world presents us with and the value judgments, imperatives, etc. that we use to guide our actions. The problem of justifying the use of bridge terms carrying us from information to action is, as I have already suggested, peculiarly intractable, because we cannot deal in terms of pure logic; nor on the other hand is it sufficient merely to describe the processes of thought whereby "people" in fact bridge the logical gap between information and action. In discussing economic planning - and a host of other important questions - one is faced with the necessity of building a formalised language with more precise rules as part of the general effort of man to use the available information more intelligently to guide his actions. We are - to use a brilliant metaphor of Wittgenstein's - seeking to build regular, planned, properly mapped, streets.(1) Thus the maximising of valuation is a recommended policy which cannot be given a logical justification, nor yet an empirical one; I believe that arguments <u>can</u> be given, in the sense of illuminating what is involved in accepting or rejecting the policy.

Moral arguments are the most difficult of all to present. In the present case, the full argument could only be given at very considerable length; and

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(1) "Our language can be seen as an ancient city: a maze of little streets and squares, or old and new houses, and of houses with additions from various periods; and this surrounded by a multitude of new boroughs with straight regular streets and uniform houses." (Wittgenstein 1951) §18. The regular streets are supposed to provide easier travelling between the older quarters too.

I very much doubt my own ability to do it. The first point to be established is that the forms of argument <u>against</u> the Bernoulli principle must, as I have suggested, involve an explicit or implicit valuing of either certainty or uncertainty for its own sake. Consider a single moment. The ethically relevant characteristics are described by the significant variables. Now these significant variables are random variables. The valuation function was so defined that the valuation of two configurations considered jointly is the sum of the valuations of the two considered separately. Suppose that in the new set-up, these two configurations are equally likely; that is, have equal probabilities (or probability densities), say, equal to one half. Since the two possibilities have equal status, the natural way to value the two possibilities together is to take the average valuation: unless the difference between the two single valuations (which is the degree of uncertainty involved) is thought to influence the matter.

Can there be valid grounds for biassing the valuation in favour of more uncertain or less uncertain situations? Naturally, in our individual lives, anticipations and uncertainties play an important part, affecting the quality of our existence in any extremely important way. Some characters thrive in exceptionally uncertain conditions; others, it seems, have a great psychological need for certainty. But in the case of these judgments on behalf of the community, what is at stake is not a general sense of security or uncertainty, but the degree of uncertainty of the planners. It is difficult to see how that could be morally relevant to the optimal path of development. All this may been seen in a more common-sense way if one reflects on the reasons for castigating political policies as unadventurous on the one hand or rash on the other.

I am saying that, unless (moral) reasons can be given to the contrary, configurations of equal status - whether they refer to similar groups of people, identical periods of time, or equally probable possibilities, should be given equal value. Like any general moral principle, this can be given no final justification. But to fly in the face of such principles, on the grounds that "anything is allowed" argues a staggering belief in the irrelevance of human actions. For some of us, it is worth grasping at any rule one can distill from the experience of living together.

Another problem remains: I have suggested the familiar policy in terms of correctly known probabilities. Surely that does not help us in action at all, for we never know precise and correct probabilities.[1] As part of a policy for getting as close as possible to the correct probabilities after the accession of a vast amount of information, it seemed sensible to act <u>as if</u> the estimated parameters gave correct probabilities. Does this rule remain sensible when values and the particular action are involved - for here one is certainly acting only on provisional information and estimates?

This is, I take it, the sense behind the implicit criticism of the present approach that could be read into the work of Wald and his successors in the field of statistical decision theory. (cf. (Wald 1950), (Savage 1954), and many others). These theorists would list all the possible states of the

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(1) No die is perfect, nor all the balls in an urn of precisely similar weight, shape, etc. We can never know all influences acting on an electron.

world, and all the possible policies for action - assigning to each possible pattern of information a particular act. They would then calculate the expected utility, given each state of affairs, of each policy: if the state of affairs is given, then a probability distribution for the possible patterns of information can be found, and so also can the utility of taking the action prescribed by the policy for any particular pattern of information - in this way with each policy and each state of affairs an expected utility is associated. If an a priori probability distribution for the states of affairs can be assumed (as Savage would want to), all these expected utilities can be combined into a single expected utility, and the policy that yielded the largest expected utility would be chosen. However, in my view, if a priori probabilities can be assigned, that must be because some information has been neglected in the earlier stages of setting up the problem - so that the policies have been defined too narrowly: the a priori probabilities are estimates, not correct probabilities, and so the justification for maximising the expected utility has gone. For Savage, of course - despite the occasional lapse from consistency - this last maximising of expected utility is only an expression for applying one's given preferences to the uncertain prospects, described in terms of a certain utility $u(s)$, for each state of affairs s. If we can assume that our values are well enough developed to require no guidance on comparing uncertain prospects, this approach seems satisfactory, but I do not believe we can. In any case one cannot in practice list all possible states of affairs, so the precision and correctness which this approach pretends is illusory. One is going to have to use a model and estimated parameters. Even if the method of estimation of the parameters is made to depend on valuation of prospects via some minimax procedure, the model is going to have to be taken as given for the time being, so that one will be working with estimated probabilities, not actual ones. Hence this method does require the application of the maximum valuation rule to estimated probabilities which are known to diverge from the correct ones.(1)

It seems to me, then, that one must be prepared to apply the maximum expected valuation rule where one is using estimated probabilities. In other words one should proceed as when one is simply trying to estimate accurately; namely, assume - for the purposes of action - that the currently estimated probability distributions are correct. As I have already argued, it must be impossible to disagree with this principle on purely logical grounds, since we are concerned to bridge a gap that logic inevitably leaves. It is a great benefit to be able to divide the planning procedure into stages - first estimating the probability distributions, the projection transformations, then applying the valuations of certain prospects to the independently estimated probabilities. If the Wald type of statistical decision theory could have been shown to avoid the awkward jump whereby estimates are treated as correct probabilities, the increased complexity of that approach might have been justified by its more satisfactory logic, despite the difficulties in agreeing to the final criterion. But for the reasons given above I do not believe that that is so. In any case the procedure that I am suggesting involves an attempt

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(1) As I have already indicated, I believe that it is in principle possible to define states of affairs (or, rather, a phase space) that are equally likely (in the sense that the whole apparatus of probability theory can be applied on the basis of this assumption, and also that they display a basic metric similarity. Thus the principle of indifference can in principle be applied if the states of affairs are properly defined.

to reflect the basic phase-space of the world in the particular problem being tackled. We always treat provisional guesses as truth for the purposes of action; we may be wrong to do so in some cases, and should perhaps demote the pursuit of truth from its position as the guiding rule of science, preferring to see a more explicit recognition of our immediate values; but I have found no convincing argument that in any particular case the natural division of human problems into the guess at truth, and the act on the assumption that the guess is true, has been bad. Hence it seems good to follow the same policy in planning and statistical decision-making.

Finally, I must justify my remarks that projections should be preferred to predictions if they field significantly different results (unless the unvarnished prediction is as good a probabilistic model as any other.) It will be clear from the preceding discussion that the choice of the proper probabilistic model is distinctly awkward, depending on much informal knowledge, which is distilled into particular probability distributions with a few free parameters. Every statistician knows that for every published probability distribution, there is an infinity of others that could have given as correct (or incorrect) a picture. The different distributions differ in their plausibility. No numbers can or need be given to the degree of plausibility: there is no question of a cloud of subjective probabilities lying behind all that has gone before. The statistician tries to choose the most plausible model, subject - perhaps too subject - to the general guiding wisdom of his profession. In a sense the particular choice, provided it is sensible, does not matter much. But it does matter that an implausible model should not be chosen. And in general, as far as economics is concerned, the plain prediction is a highly implausible model. However, the planner playing his economy is not worried about a semi-tone or two between what he does and the absolute best, not yet. It is quite possible that no plausible model will make a difference to optimal policy large enough to worry him. The striking fact is that we do not know whether this is ever the case; for no-one has ever tried to find out. A substantial part of this thesis is devoted to trying to find out for the simplest planning problem, namely the optimal investment problem. Intuition is of no service in such matters.

We must also remind ourselves that it is not clear when formal methods are superior to the informality of intelligent intuition. If the calculated decision is highly sensitive to variations in assumptions between which we cannot decide, we are in an even more serious position: we do not know what is best. But a policy of avoiding interference when agreement on what is best is impossible is quite different from a policy of avoiding decision where there is a great deal of uncertainty about the results, for it may be possible to use quite adequate probabilistic models, and obtain reasonable agreement on aims and methods. It is precisely where uncertainty is great that informal methods, and the decentralization involved in a mixed economy, are most likely to fail. In highly uncertain economies, of course, the temptation for the planners to try to make what they said would happen happen is greatest: it must not be assumed that a concentration on reducing uncertainty is the optimal policy.

\- 6 - As a postscript to this chapter, I shall survey very briefly, the work that has been done in seeking optimal development policies in macro-economic terms.

The seminal work is (Ramsey 1928), to which I have already referred. Ramsey provided a formula for the optimum rate of saving, and the optimum rate of labour employment, for an economy with the neo-classical production function $y = f(k,l)$ (y being output, l labour.) In the next chapter I shall give an analysis of this problem (by a different method from Ramsey's) as a prelude to my detailed study of the generalized neo-classical problem. Ramsey's work is frequently criticized for making unrealistic assumptions. He expressed it in terms of an aggregative utility function (which Ramsey called a satisfaction function). It was found that the utility function that people seemed to have suggested extremely high rates of investment, an unpalatable, and implausible, conclusion. Others, at a later date, found the cardinal utility function impossible to swallow.

Ramsey's work has continued to fascinate, and exasperate because the economist is unable to apply it, despite the real nature of the problem. It has not bred many generalizations. The most important work following it is that of Meade on the relation with optimum population ((Meade 1955), and that of Samuelson and Solow extending the problem to a multi-product economy, (Samuelson and Solow 1956). This last study has led into the more recent discussion of turnpike theorems and paths of efficient accumulation. (See, *e.g.*, (Samuelson 1960)(Radner 1961)(Furaya & Inada 1962)). The mathematical form of the problem encourages the use of the calculus of variations: in the case of many products, the Euler-Lagrange equations cannot be solved explicitly; hence the concentration upon the behaviour of the solution as time tends to infinity. For example, in any economy for which a steady growth state exists, there being no consumption,[1] a path for which the final stock of one of the goods is a maximum at some distant time remains close to the maximal growth path, called the von Neumann ray, for most of the time. This is a rough statement of one form of the turnpike theorem, first stated in (Dorfman, Samuelson, and Solow 1958). Most of us like to have consumption in our economies; the hint as to the importance of steady growth has been taken up in the maximal consumption theorems ((Robinson 1962), (Champernowne 1962), (Black 1962), (Solow 1962), (Samuelson 1962), (Phelps 1961), (Meade 1962).) These assert that under certain assumptions (constant returns to scale is the important one) the steady growth state for which consumption per head at each time is a maximum is the one for which the marginal product of capital is equal to the rate of growth. I shall bring argument later to show that this result is less relevant to finding optimal development paths than it seems. For example, when there is no growth of the labour force, nor of technical knowledge (i.e. the Ramsey case) the result tells us only that the greatest level of consumption maintainable from now on is that for which net investment is zero: not a very helpful result.

Because of the apparent intractability of the Ramsey model, most subsequent papers have simplified the assumptions drastically. The popular device is to assume a constant capital-output ratio. ((Tinbergen 1956), (Tinbergen 1960), (Goodwin 1961), (Chakravarty 1962). There is an interesting note on the first Tinbergen paper, (Sen 1957).) Goodwin's paper is perhaps the most interesting, for he assumes a lag between investment and production, a refinement that others have found too awkward to handle. Both Goodwin and

(1) Other than that implied by a fixed wage rate.

Chakravarty favour a finite time-horizon. (It was also suggested in (Sen 1957)) Goodwin calculates some numerical examples, in which the common feature is very low levels of consumption initially, probably as a consequence of the constant capital-output ratio assumption. Goodwin's terminal condition is a specification of the rate of growth of capital: his results look a little odd because the terminal rate of growth is much smaller than could theoretically be achieved when the capital-output ratio is constant. Chakravarty's terminal condition is a specification of the ratio of final capital to initial capital. He argues that the result is insensitive to this ratio, but highly sensitive to the time-horizon (because the valuation function does not admit bliss.) Tinbergen assumes positive time-preference, no bliss, and a constant saving-ratio, which turns out to be rather large when he uses Frisch's estimates of the elasticity of marginal utility.[1]

The school of disgust at utility functions is represented by (Horvat 1958), already referred to; and (Sen 1961), wherein more Marxian production assumptions, and a more political treatment of the choice between present and future is commended. These latter ideas develop those in his book (Sen 1960), where the planning problem for a two-sector economy, with capital made by one sector and used by the other, is discussed, along with some generalizations. The discussion draws out the contrast between different policies in terms of the contrast between present consumption and employment on the one hand, and the rate of growth and future employment on the other. This class of model, also discussed in (Dobb 1960), is particularly appropriate to underdeveloped economies; those who have studied it have unfortunately avoided the Ramsey-type utility function. Optimal policies for this kind of economy are

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(1) In addition to these modifications or extensions of Ramsey's work, there is an attempt to fit his results, for appropriate forms of the utility and production functions, to the behaviour of actual countries in (Stone 1955).

considered in (Srinivasan 1962), but the valuation functions are not readily justified: the criteria considered are the minimization of the time taken to the maximal consumption path, and the maximization of discounted consumption.

The Ramsey type of argument is applied to the choice of technique problem proper in an interesting paper (Eckstein 1957), where a formula for the rate of interest, which goes back to the Ramsey paper, is expounded. Further developments of the argument are possible, but the topic is not taken up in this thesis.

The same formula is used in (Harrod 1960), which is concerned, essentially, with the optimum saving problem. It is assumed that the utility function is $u(c) = B - c^{-n}$.(1) If there is no time preference, the valuation for m people sharing consumption C is $mu(C/m)$, and the rate of growth of consumption per head is g, then the "rate of interest", r, meaning the marginal product of capital at constant time, is given by:

$$r = (n+1)g,$$

on the optimum development path. Harrod then assumes that the ratio of the saving ratio to the rate of growth of output (which has become known as the incremental capital-output ratio) is a function of the rate of interest (but presumably not of time.) He further assumes that g is a function of r (or rather that the rate of growth of output is a function of r). The above equation determines r, and the optimum rate of saving is supposed to be found by means of the "incremental capital-output ratio".

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(1) This is not admitted in the paper, but it is clearly assumed that the elasticity of marginal utility does not change with time.

All this is easy when the production function simply makes output proportional to capital and there is a constant capital-output ratio. If this is not the case, the Harrod method only shifts the problem to determining the dependence of the "incremental capital-output ratio" upon the "rate of interest". Observation is not any direct help: existing interest rates are not easily related to the marginal social product of capital; in any case the optimum situation is not likely to bear much resemblance to the present one. I conclude that Harrod has only transformed the problem into a less familiar, but equally difficult, form.(1)

My reading of the literature is that no-one has succeeded in analysing a production model that is at all realistic with a plausible valuation function; nor has anyone even attempted to treat the problem of uncertainty. Recently the emphasis has turned away from analysing present policy to discovering theoretical elegancies in asymptotic behaviour. On the face of it, this turning is an unprofitable one for the theory of planning. It will appear in chapter V that it is a more fruitful search than at first appears.

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(1) A recent exposition of Ramsey, (Black 1962), effectively assumes a constant "rate of interest", which is as good as assuming $y = a.k.$

CHAPTER III: A DYNAMIC MODEL

- 1 -

In this and the following three chapters I expound and study a model of economic development that generalizes Ramsey's "neo-classical" macro-economic model. My purpose is to discover, if I can, what difference would be made to our recommendations if we assumed a steady improvement in technological possibilities and a growing uncertainty about future production (and also, as a by-product, what effect pure time-preference has on the results.) Logically, these problems are mathematical questions of some difficulty. It is not surprising that even tolerably good approximations to the optimum investment policy are hard to obtain. However, we can discover a great deal about the qualitative features of the solutions, and even give some numerical estimates. I shall be able to prove enough to answer most of the more obvious economic questions that might be asked about the optimum policy for the model.

This chapter is devoted to defining the model, the next to the fundamental equations for the optimum policy and a number of fairly general, but elementary, theorems. These are followed by two chapters in which the model is first analysed when there is no uncertainty - that is, when we are entirely concerned with technical progress - and then when there is uncertainty as well. Chapter VII is devoted to a number of generalizations. In the final chapter, I discuss the conclusions - and their limitations. A few of the more awkward mathematical manipulations are consigned to an appendix.

There are two ways of describing an economic model. The first - which is usual in the sciences - defines the interrelations of the variables and indicates the features of the world to which they are supposed to correspond. This correspondence is only approximate; the degree of approximation to any particular economy must be guessed. In order that the approximation should be as useful as possible, the precise definition of the variables can be postponed until a particular application demands definition. We have a precise logical model, and a rough guide to its relationship with the economic world. This way of defining a model has the advantages that it admits honestly that we want an approximation to the real world, and that it leaves the interpretation of the model as free as possible; on the other hand, it fails to emphasize how a model, which is only one step in the development of a general theory, is to be improved. Nevertheless, it seems to me the best way of setting up the model, and I shall describe it in these terms first.

A second method has been popular in some recent work. One describes an imaginary and very simple economy, with all the variables precisely defined, as well as the relations between them.(1) The advantages of this approach are that the limitations of the model are dramatically set

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(1) This procedure has been described as that of making "scarecrow" assumptions (v.(Swan 1956)). Clear examples of the genre can be found in (Robinson 1961) and (Bensusan-Butt 1960), as well as many other books and papers. Perhaps surprisingly, many mathematical economists, following von Neumann (von Neumann 1948), work this way. The "scientific style" is exemplified by many econometric works (e.g., Solow's work on production functions and technical progress (Solow 1956, 1957, 1960)), and, indeed, in Keynes' exposition of his basic model in the General Theory (Keynes 1936).

out - really too dramatically - and that non-mathematical economists seem to find it easier to think in terms of such precisely defined concrete assumptions. Although, in this style, the exercise appears less valuable than it is, and obscures the extent to which economic theory is an accumulating scientific theory, the approach does form a useful complement to, or exposition of, the basic definition of the model according to the first method.

A. The assumptions of the model are:-

(1) GENERAL - The economy is closed (or at least its relations with the rest of the world are approximately constant). A unit of measurement for stocks and flows of goods is agreed, which is of effectively identical meaning at different moments of time. The rate of (net) output at any time, which I shall denote by y, can be divided arbitrarily between current consumption and additions to capital stock, or investment; which two variables I shall denote by c and i. All three variables are measured in terms of the agreed unit. Our first relation is:

$$y = c + i. \tag{1}$$

COMMENT 1: I am assuming that production is exclusively divided between something that is valuable in itself, but of no productive use, and something that will increase the level of future production, but is of no value in itself.

COMMENT 2: The assumption that output can be arbitrally allocated to the two uses is much stronger than necessary. It is quite sufficient if we can choose the unit of goods and services so that the allocation can be arbitrarily varied in the neighbourhood of the optimum allocation - whatever that may turn out to be. Cases where the rate of substitution between consumption and net capital formation varies along the optimum path can be dealt with.

(2) PRODUCTION - The value of capital stock, k, is accumulated net investment, so that:

$$i = \frac{dk}{dt}. \tag{2}$$

Current production depends on the current level of capital stock. The dependence is precisely known at time 0, which we may call the planning origin. (This assumption will be relaxed later.) Using a o-suffix to relate the variables to the planning origin, we write:

$$y_o = f(k_o). \tag{3}$$

The function $f(.)$ is called the production function. It is assumed that:

(i) $f(0) = 0$;

(ii) $f(k)$ is an increasing function of k ;

(iii) f is as continuous and differentiable as may be required for any part of the argument.

This last assumption will be made of any functions that may be defined in the course of the thesis.

The manner of dependence of output at future dates on the capital stock at future dates is uncertain. A projection of future production is made by means of the random function:

$$y_t = Z_t f(k_t). \tag{4}$$

Z_t is a random variable; the relation (4) makes a random variable (for output at time t) correspond to each possible value of the future capital stock.[1]

I shall picture the idea that the planners are more uncertain about future production possibilities the further distant in time they are, and allow for the expectation of general improvement in production possibilities, by using the assumption that

$$Z_t = e^{\alpha t + \varepsilon_t}, \tag{5}$$

where ε_t is a normal random variable with mean $-\beta t$ and variance $2\beta t$. It is easy to check that $E(Z_t) = e^{\alpha t}$. Furthermore, I shall assume that (ε_t) is a stochastic process with independent and identically distributed increments - in fact a Brownian stochastic process with mean $-\beta$ per unit time and variance 2β per unit time.[2]

This assumption is capable of generalization, and indeed must be generalized by relaxing the assumption that the influences of successive periods of time on the initial production function are independent. As it seems to be rather difficult to make this extension, it will not be attempted in the present thesis.[3] A modification that can be more easily made is to allow α and β to depend on the stock of capital or the rate of investment. These and similar possibilities will be examined later.

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(1) A more complete theory, in which the projection is more general, and in particular related to the changing data available, is not attempted in the present thesis.

(2) This was defined in Chapter II above.

(3) A particular case is examined in Chapter VII below.

An investment strategy will tell us how to divide current production between consumption and investment in any of the possible circumstances that can arise (described by the stock of capital and the production function). Since the future form of the production function depends on a random variable, even with a given investment strategy future investment is a random variable; hence future capital stock is a random variable. To distinguish the random variable from any particular value it may take, I shall denote the capital random variable by K_t, and any particular value of capital by k_t.

(3) VALUATION.(1) The total valuation of a programme of future consumption $(c_t : t \geqq 0)$ is:

$$\int_0^\infty v(c_t)e^{-rt}dt.^{(1)} \tag{6}$$

The instantaneous valuation function (usually referred to where confusion is impossible simply as the valuation function), $v(.)$ has the following properties:

(i) $v(c)$ is a strictly increasing function of c;

(ii) $v(c) \longrightarrow 0$ as $c \longrightarrow \infty$;

(iii) $v(c) \longrightarrow -\infty$ as $c \longrightarrow 0$;

(iv) $v(c)$ is a concave function (2); $v''(c) \leqq 0$.

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(1) The form of the valuation function was discussed in chapter II.

(2) cf. (Eggleston 1958), p.45.

This valuation applies to a sure consumption programme. In the general case of the model, consumption is a randon variable; then, for the reasons discussed in the previous chapter, the valuation ~~of~~ is the expectation of the expression (6). The problem is to find the investment strategy that will "maximize" (1)

$$E\left(\int_0^{\infty} v(c_t)e^{-rt}dt\right). \tag{7}$$

The expectation operator applies to the random variables (ε_t).

An investment strategy is a rule that associates with each possible (current) production function, and each possible value of the stock of capital, a rate of investment. This class of investment strategies must be restricted by excluding certain unrealistic possibilities. For example, if it were possible to run down capital until it reached negative infinity, there would be no optimum policy at all. This is clearly ridiculous. We must restrict the class of admissible strategies by requiring that they should never render the stock of capital negative. This is ensured by requiring that <u>any investment strategy must, to be admissible, give rates of investment greater than zero when the stock of capital is zero</u>.

Realism suggests that we might further restrict the class by saying that capital cannot be run down at faster than a certain rate. However it is generally very easy to deduce the optimal policy in such a case from the less restricted case, as we shall see, so that there is no need to make a further restriction.

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(1) The word is enclosed in inverted commas to indicate that maximization may have to be understood in the extended sense expounded in Chapter II.

B. Having expounded the model once, I do so again, this time in terms of "scarecrow assumptions".

(1) The economy is closed.

(2) A single infinitely divisible and homogeneous good is produced, accumulated, and consumed.

(3) The population of the economy is constant in all respects: in numbers, composition, and tastes.

(4) When accumulated as capital, the good never deteriorates. The number of workers that can be employed with any amount of it is freely variable.

(5) Total production at any time is divided exclusively between consumption and additions to capital:

$$y = c + \dot{k}$$

(6) The division of that part of the national product allocated for consumption between the different members of the community is rigidly determined by law, custom, or overriding moral considerations. So are the hours and intensity of work. Thus, the varying economic situations possible at any time can be described by the aggregate rate of consumption: that is the only "significant variable", the only one that has to enter the valuation function.

(7) The present state of knowledge regarding production at times present and future is given by:

$$Y_t = Z_t f(k_t),$$

where Z_t and $f(.)$ have the properties already discussed in the first statement of the model.

(8) The question we want to ask is - how much of the national product ought the country to consume at each moment of time? Since the appropriate valuation must inevitably be a matter for dispute, and will, in any case, be different in different countries with differing tastes, customs, etc., we want to be able to answer the question for any instaneous valuation function $v(c)$ satisfying the conditions listed above. The objective is to maximise:

$$V = E\left(\int_{o}^{\infty} e^{-rt} v(c_t)\, dt\right)$$

If the reader wants an institutional background to this question - although the question is, of course, logically independent of any such background - he may imagine a central authority or government charged with, or adopting the responsibility of, arranging the rate of investment in the economy. The means are not relevant here, although very important: monetary policy, fiscal policy, statutory regulation of investment, or government marketing can all be considered. It must be noted that although the ethical question is without interest unless somebody is or could be charged with attempting to implement the answer, we - the economists - are not committed to accepting the values that happen to be currently held by the implementing group.

It will appear that the exposition of the model and its workings is greatly simplified if the following assumption about $f(.)$ is made:

(U) $\qquad f(k) \longrightarrow \infty \quad \text{as} \quad k \longrightarrow \infty.$

It is not clear how one could decide whether this is realistic. I am inclined to think it is. However it is not necessary to make the assumption, and the problem can be solved without it.

Occasionally I shall indicate the consequences of assuming instead that the limit of $f(k)$ as k tends to ∞ is finite. It is to be borne in mind that the choice of the form of $f(.)$ is the choice of a good way of predicting future production. If a finite limit is assumed, we are in effect estimating certain maximum levels of production for all future times (random fluctuations apart). This is, to say the least, difficult to do, and not very convincing. So long as $\alpha > 0$, we may take it that (U) is not a very serious assumption.

I make the further assumptions:

$$(P) \qquad \alpha \geqq 0; \qquad r \geqq 0 .$$

The former scarcely requires comment. In the present state of the world, we do not have to deal with a steady running down of economic possibilities, and there is therefore no point in considering that part of a theory of planning that would tell us what to do about it.

At one point in a later chapter, I shall relax the second assumption in effect, when I say something about the consequences of assuming an exponential population growth. Otherwise, despite the argument of the previous chapter, it seems to me that a negative value of r is very implausible. As I have already remarked, I think the sensible assumption is that $r = 0$.

It is to be expected that the problem will be more easily solved when the functions f and v have particular forms, and this is so. I shall therefore have frequent occasions to assume that $f(k) = ak^b$ and that $v(c) = -c^{-n}$. Both forms are familiar, the first so familiar as scarcely to require comment. The meaning of the form $v = -c^{-n}$

requires some elucidation, however.

The meaning of a valuation function can be made most striking, and familiar, I think, by interpreting it in terms of interpersonal redistribution of consumption. The whole problem of planning is one of determining the right distribution between people, both contemporaneous and of different generations. The valuation function interprets our values in terms of distribution between people with different consumption levels. The metaphor of the mythical cake whose size varies with the proportions in which it is divided is familiar enough. Given two people, with rates of consumption c and c', we may ask how much we should like to see the first rate of consumption reduced in order to have the second rate of consumption increased by a fixed (small) amount. This - admittedly difficult - thought-experiment seems to me a very natural way of expressing our views on equality. Obviously a great many considerations besides our ideas of fair distribution of material consumption ~~is~~ are involved in the highly complex debate about equality; but central to actual policy issues, such as taxation, is the question of the abstractly just distribution when the size of the cake varies with the slices. It is this that is to be expressed by the function $v(.)$. The maximum amount to be taken from the c - man in order to provide an increase in the c' - man's consumption is $\frac{v'(c)}{v'(c')}$ times this (small) increase. When $v = -c^{-n}$, this factor is $(c'/c)^{n+1}$, and thus depends only on the ratio of the levels of living of the two beings, not on their absolute levels. Is it plausible that only the ratio of the levels of consumption considered should be relevant to the ratio in which one would be willing to redistribute?

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I think it is as plausible as any alternative. There is one argument that suggests an improvement, however. It has been usual for nutritional scientists and others to speak of threshold levels of consumption for various classes of food, and it is very common to find even highly intelligent people thinking in terms of minimum standards of clothing or housing. Actually there is no convincing evidence for this picture of a set of sharp minima for the various parts of consumption, but the notion of basic subsistence consumption has caught on. It is argued that there must at least be a minimum below which life cannot be continued. Even for this there is no evidence. No doubt below a certain level of consumption, any reduction reduces life expectancy as well as general happiness: but to fix a certain level below which this phenomenon is extremely important and above which it can be ignored is more picturesque than plausible. One should not, clearly, introduce a fixed level of "subsistence consumption" into the valuation: yet perhaps there is enough in nutritional and similar arguments to argue that redistribution towards the extremely poor is much more important than towards the "poor" of a society such as the American, so much more important that the elasticity of marginal valuation should be much greater at these levels. Viewed in the context of actual lives, I think this argument is too vague to make the case that the comparison between threepence and sixpence a day is radically different, quantitatively, from the comparison between one pound and two pounds a day. The rate at which life becomes intolerable as we move down the income scale is too gradual for it to be obvious to one who has seen extreme poverty <u>and</u> not-quite-extreme poverty to think it obvious that the assumption $v = - c^{-n}$ is wrong.

The greater is n, the greater is the amount one would take from the rich to give one penny to the poor. n is an index of the strength of egalitarianism. How can one decide the particular value that n should take? It is a discouragingly difficult question. Yet it is surely relevant to all the questions of planning; unless one has a view on it, one cannot honestly pronounce on the right policies to adopt. If it is impossible honestly to fix upon a particular value of n, we must conclude that economic planning is quite arbitrary, and devote our lives to the contemplation of beautiful numbers and the creation of a satisfying personal life. Most of us, I think, will be willing, however, hesitantly, to take a position. Some would find $n = 0$ perfectly plausible (which would not be consistent with my assumptions). I cannot do so myself. Many would regard $n = 1$ as rather large, implying as it does that it is right to take four pounds from a man with a thousand pounds in order to give one pound to a man with only five hundred. Others will say: why stop short at any finite n? Equality is an overriding end. I doubt whether anyone really believes that it is. $n = 3$ already implies that considerable importance is given to equality. It is a value that I think I am prepared to accept: at any rate something between one and three seems plausible. I shall not try to argue the question more closely here.

It is important not to confuse the question of estimating $\bar{V}$ with estimates of the utility of income that have been made. No doubt many would want the valuation function to be measured by the cardinal utility of consumption. However one is not required to agree with them; and for myself, I think that revealed utility of consumption is determined by considerations that have no place in constructing the valuation function, such as prospects of increased consumption <u>for oneself</u>, and at the best presents a very self-interested view of equality. Frisch produced estimates for the elasticity of marginal utility from both French and American data (for the latter, see (Frisch 1932)). n was found to decrease as income increased, remaining greater than 1 for French workers, but falling to about 1/4 for the Americans. I shall assume that those who believe in such estimates, and want to reflect people's preferences on such matters in the valuation function, would want n to be about 1.

This, then, is the basic model. It will be generalized in a number of ways in chapter VII; but most of the thesis will be devoted to analysing it as completely as I can. Even this simplest of planning models cannot be fully solved, but I think we can go a long way towards solving it.

- 2 -

I have remarked that the problem generalizes the problem in Ramsey's ~~seminal~~ paper (Ramsey 1928). His model represents the static case, when α and β are zero, (and also r.) It is essential to understand clearly the static case before turning to the dynamic case (by which I shall refer to cases where r, α or β is not zero,) as it plays an important part in the more general analysis, and provides a point of comparison. By way of variety, and also because the method is extremely useful when applying Ramseyan methods to other economic models, I shall solve the maximization problem in a different way from his.

We want

$$\int_0^{\infty} v(c_t)\, dt = \max. \qquad (8)$$

with $c_t = f(k_t) - \dot{k}_t$ and k_0 given.

$f(k)$ is an increasing function of k: define $\bar{k}$ $(\leqq \infty)$ as the smallest $\bar{k}$ making $f(\bar{k})$ a maximum. ($\bar{k} = \infty$ if f is strictly increasing for all k).

Clearly the case where $f(.)$ has a finite maximum must be distinguished from the case where $f(.)$ is unbounded: for in the former case, $v(c_t)$ cannot tend to zero. Suppose first that the supremum of $f(.)$ is $\bar{y}$. Then optimally c_t must ultimately tend to $\bar{y}$, since otherwise v would tend to a lower value, in which case the valuation integral could be dominated (in the sense defined earlier) by one for a path with $c_t \longrightarrow \bar{y}$. We want to maximize

$$\int_0^\infty (v(c_t) - v(\bar{y}))\, dt.$$

It will always be right to invest at a positive rate so long as $k < \bar{k}$: hence, if we denote the optimum path of capital accumulation by $\hat{k}(t)$, $\hat{k}(t)$ is an increasing function of t, strictly increasing until $\hat{k}$ reaches the value $\bar{k}$, say at $T \leqq \infty$. We can change the variable of integration to k in (o,T); for the range (T, ∞), the integral is zero. Hence the valuation integral is

$$\int_{k_0}^{\bar{k}} (v(f(\hat{k}) - \dot{\hat{k}}) - v(\hat{y}))\, \frac{d\hat{k}}{\dot{\hat{k}}} \tag{9}$$

For any particular value of $\hat{k}$, the integrand is maximum when (differentiating with respect to $\dot{\hat{k}}$):

$$\dot{\hat{k}}\, v'(f(\hat{k}) - \dot{\hat{k}}) + v(f(\hat{k}) - \dot{\hat{k}}) = v(\bar{y}). \tag{10}$$

If $\dot{k}$ is given as a function of k by (11), $\dfrac{v(c) - v(\bar{y})}{\dot{k}}$ is a maximum for each k, and so the integral (10) is a maximum.(1)

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(1) It is easy to check that the second order conditions give a maximum, not a minimum or anything else.

This line of reasoning is not clearly valid if the k-path so calculated fails to make the integral (10) finite. But then we have certainly shown that

$$\int_{k_0}^{\bar{k}} \left\{ (v(\hat{c}_t) - v(\bar{y}))\,\dot{\hat{k}}^{-1} - (v(c_t) - v(\bar{y}))\dot{k}^{-1} \right\} dk \geqq 0$$

for any other k-path: which means that $\hat{k}(t)$ is optimal according to our extended definition.

Turning to the case where $f(.)$ is unbounded, we see that $\bar{y} = \infty$, and that the same reasoning applies to the integral in (9) without modification. Instead of equation (11), we have the following to define the optimum path:

$$\dot{k}\, v'(f(k) - \dot{k}) + v(f(k) - \dot{k}) = 0. \qquad (11)$$

As before, $\hat{k}(t)$ so defined is optimal whether it renders the valuation integral finite or not.

It is clear from both equations (10) and (11) that $\dot{\hat{k}}$ depends only on $f(\hat{k})$: in other words to relate investment to output, it is only necessary to know the form of the valuation function and the upper limit of $f(k)$. If, for example, $f(k) = ak^b$ and $v(c) = - c^{-n}$,

$$\dot{\hat{k}} = \frac{f(k)}{n + 1}.$$

Thus the greater is n, the greater is the proportion of consumption to output. For those who believe that n should be 1 or less, this formula is a distinct problem, since it suggests a 50% investment/output ratio (which I shall call the investment ratio), which is quite unacceptable. Many reasons can be given for rejecting the recommendation, the most popular being that the assumptions are too unrealistic. Unrealistic they are, but whether they are <u>too</u> unrealistic remains to be proved.

It will be shown later that in fact they are, and that the static case does not in general give a good guide to action. I find this a surprising result, for the dynamic parameters $(r, \alpha, \text{and } \beta)$ have to take rather small values.

As it will be used frequently in the following pages, I want a special notation for the dependence of $\dot{\hat{k}}$ on y in the static case with $\bar{y} = \infty$. I shall denote it by

$$\dot{\hat{k}} = \theta(y) \tag{12}$$

In the next chapter, I begin the study of the dynamic case.

CHAPTER IV THE FUNDAMENTAL EQUATION

- 1 -

In my basic model, expounded in the previous chapter, the form of the production function is fixed: production possibilities at any time are a multiple of some function $f(.)$. If at some time this factor is a, and the stock of capital is k, the optimum rate of investment must be a function of a and k alone. We have to choose the optimum strategy from among investment strategies $i(k,a)$. Let us denote the optimal strategy by $s(k,a)$.

In order to avoid a complicated discussion, I shall assume without proof that an optimal investment strategy $s(k,a)$ exists, and that both s and the corresponding total valuation (which will be denoted by $V(k,a)$,) are twice differentiable functions of k and a. A rigorous derivation of the fundamental equation, which is the key to the solution of our problem, is given in the appendix. Here I shall prove in a rather informal way that the optimal policy satisfies the fundamental equation. It will be easier to see how the fundamental equation arises from this non-rigorous argument. The argument is essentially a dynamic programming argument, depending on what is usually called the principle of optimality.(1)

If, starting at a time when the capital stock is k, and the production function is $af(.)$, the optimal investment policy is employed,

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(1) The method was first developed by P.Massé (see (Massé 1959)), and has been applied by Bellman to a wide range of problems. (Bellman 1957), (Bellman 1961), and various papers.) The general contours of the argument presented here owe something to the problems studied in (Bellman 1957.)

a certain expected total valuation is generated[1], and is denoted by $V(k,a)$. Suppose that after a short time θ [2] the optimal strategy has taken the economy to the position k', a'. From that point, the total valuation will be $V(k',a')$. Because of the discount factor, the valuation now of the state of the economy from time θ on is $e^{-r\theta}V(k',a')$. a' is the value of a random variable $Z_\theta a$; and $k' = k + s(k,a).\theta$ to first order in θ. Hence - again to first order in θ - $V(k,a)$ must be the maximum value of

$$E\left\{\int_0^\theta v(c_t)e^{-rt}dt + e^{-r\theta}V(k+s(k,a)\theta,\ Z_\theta a)\right\},$$

for the different admissible functions s; and s itself is the function at which this expression attains its maximum. To first order in θ, the integral can be written as $v(c_0).\theta$. Therefore,

$$\begin{aligned} V(k,a) &= \operatorname*{Max}_s \left\{E(v(af(k)-s).\theta + (1 - r\theta).V(k + s\theta,\ Z_\theta a))\right\} \\ &= \operatorname{Max}\left\{v(af(k) - s).\theta + E(V(k + s\theta, Z_\theta a)).(1 - r\theta)\right\}, \quad (2) \end{aligned}$$

since the expectation operator does not affect the first term.

Now we must find the expression for $E(V(k + s\,\theta,\ Z_\theta a))$ up to first order in θ. Ultimately I shall let θ tend to zero; but that is not a straightforward operation when we are dealing with a random variable like Z_θ. It is easier to see what is going on if we behave somewhat

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(1) For the present, I shall assume that the total valuation is finite. The fundamental equation holds without this assumption, as may be seen by methods similar to those used for the static case.

(2) This use of θ, which is temporary, should not be confused with the function defined by equation III.(12).

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less than rigorously. As a first step, we can see that

$$V(k + s\theta,\ Z_\theta a) = V(k, Z_\theta a) + s.\theta. \frac{\partial V(k, Z_\theta a)}{\partial k} + O(\theta^2); \quad (3)$$

we may take it, too, that

$$\frac{\partial V(k, aZ_\theta)}{\partial k} = \frac{\partial V(k, a)}{\partial k} + O(\theta). \quad (4)$$

Hence the term that we really have to worry about is $E(V(k, a.Z_\theta))$.

$Z_\theta = e^N$, where N is normally distributed with mean $(\alpha - \beta)\theta$ and variance $2\beta\theta$. Hence:

$$Z_\theta = 1 + N + \tfrac{1}{2} N^2 + \ldots \quad (5)$$

I am going to expand $V(k, aZ_\theta)$ in powers of N. Since moments of N higher than the second are of smaller order than θ, we can neglect them, getting:

$$V(k, aZ_\theta) = V(k, a) + a\left(N + \tfrac{1}{2} N^2\right) \frac{\partial V(k, a)}{\partial a} + \frac{a}{2} N^2 \frac{\partial^2 V(k, a)}{\partial a^2} + O(N^3). \quad (6)$$

If we apply the expectation operator to both sides of (6), we get:

$$E(V(k, aZ_\theta)) = V(k, a) + \alpha a \frac{\partial V}{\partial a}\theta + \beta a^2 \frac{\partial^2 V}{\partial a^2}\theta + O(\theta^2) \quad (7)$$

If we bring equations (2), (3), (4), and (7) together, we can subtract $V(k, a)$ from both sides of the resulting equation, and divide by θ. Then letting θ tend to zero, we have:

$$0 = \underset{s}{\text{Max}}\left(v(af(k) - s) + s\frac{\partial V}{\partial k} - rV + \alpha a \frac{\partial V}{\partial a} + \beta a^2 \frac{\partial^2 V}{\partial a^2}\right) \quad (8)$$

Given a and k, the maximum will be attained when

$$v'(af(k) - s) = \frac{\partial V}{\partial k}, \quad (9)$$

on differentiating the expression in square brackets with respect to s.

less than rigorously. As a first step, we can see that

$$V(k + s\theta,\ Z_\theta a) = V(k, Z_\theta a) + s.\theta.\frac{\partial V(k, Z_\theta a)}{\partial k} + O(\theta^2); \qquad (3)$$

we may take it, too, that

$$\frac{\partial V(k, aZ_\theta)}{\partial k} = \frac{\partial V(k, a)}{\partial k} + O(\theta). \qquad (4)$$

Hence the term that we really have to worry about is $E(V(k, a.Z_\theta))$.

$Z_\theta = e^N$, where N is normally distributed with mean $(\alpha - \beta)\theta$ and variance $2\beta\theta$. Hence:

$$Z_\theta = 1 + N + \tfrac{1}{2} N^2 + \ldots \qquad (5)$$

I am going to expand $V(k, aZ_\theta)$ in powers of N. Since moments of N higher than the second are of smaller order than θ, we can neglect them, getting:

$$V(k, aZ_\theta) = V(k, a) + a\left(N + \tfrac{1}{2} N^2\right) \frac{\partial V(k, a)}{\partial a} + \frac{a}{2} N^2 \frac{\partial^2 V(k, a)}{\partial a^2} + O(N^3). \qquad (6)$$

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Given a and k, the maximum will be attained when

$$v'(af(k) - s) = \frac{\partial V}{\partial k}, \qquad (9)$$

on differentiating the expression in square brackets with respect to s.

Also, for this (optimal) function s,

$$v(af(k) - s) + s\frac{\partial V}{\partial k} = rV - \alpha a\frac{\partial V}{\partial a} - \beta a^2\frac{\partial^2 V}{\partial a^2},$$

which we may write, if we define an operator

$$D \equiv -r + \alpha a\frac{\partial}{\partial a} + \beta a^2\frac{\partial^2}{\partial a^2},$$

as

$$v(af(k) - s) + s\frac{\partial V}{\partial k} = -DV. \qquad (10)$$

Our optimal s and V satisfy equations (9) and (10). The significance of the operator D is that it contains the dynamic parameters - α, representing the rate of technical change, β representing the uncertainty about future production, and r representing uncertainty about either the value of future goods or the possibility of there being a future at all.

By eliminating V from equations (9) and (10), we may derive various equations for s : the two that are most interesting are, first, one resembling the Ramsey equation (III - 11) ; and, second, a purely differential equation that is valid even when V is not finite. Let us assume for the present that V is finite.

Let us notice that, on the assumption (U), that production possibilities are unbounded, the expected valuation for the optimum path must tend to zero (i.e. become as large as possible), as $k \longrightarrow \infty$, provided that $a \neq 0$. Using this boundary condition, we can integrate (9) with respect to k, getting:

$$V = -\int_k^{\infty} v'(af(x) - s(x,a)).\,dx. \qquad (11)$$

Inserting (11) in (10):

$$v(af(k) - s) + sv'(af(k) - s) = D(\int_k^{\infty} v'(af(x) - s(x,a)).\,dx) \qquad (12)$$

In the static case, D is zero; we get Ramsey's formula (III - 11): our problem is to solve this fundamental equation in the more general case when D is not zero.

The integro-differential equation (12) can be changed into a differential equation. Going back to equations (9) and (10), we write u for $v'(af(k) - s)$, and differentiate (10) with respect to <u>k</u>:

$$u\, af'(k) + s \frac{\partial u}{\partial k} + Du = 0$$

$v'(x)$ is a decreasing function of x, so that it has an inverse function, g. Then $g(u) = af(k) - s$. Hence we have the following partial differential equation for u: (1)

$$u\, af'(k) + (af(k) - g(u)) \frac{\partial u}{\partial k} + Du = 0 \tag{13}$$

As befits a partial differential equation, (13) has many solutions: we must find a way of characterizing the particular solution that will give the optimum rate of investment for our model. I do so by means of a <u>Lemma</u>.

<u>Let u(k,a) be a solution of (13). Then the investment strategy defined by u (i.e., $i = af - g(u)$) yields a total expected valuation:</u>

$$- \int_k^{\infty} u(x, a)\,.\, dx \tag{14}$$

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(1) To show that this holds even when V is not finite, we should have to go through the whole calculation replacing $V = E \int_0^\infty v\,dt$ by $V = - E(\int_0^\infty \{\hat{v}(t) - v(af-s)\} dt)$, where $\hat{v}(t) = E\,(v(C_t))$ for the optimal policy starting from some particular (a_0, k_0). V now depends on <u>t</u> as well as on <u>a</u> and <u>k</u>. By this means we derive equation (13) in the neighborhood of (a_0, k_0), which is arbitrary.

A proof is given in the appendix. The optimum strategy gives a solution of (13); and any solution of (13) that is strictly positive when $k = 0$ yields a possible strategy. Hence an optimum strategy is given by any such solution of (13) that makes (14) a maximum. If all admissible strategies render (14) infinite - i.e., if the total valuation is infinite - the phrase "(14) is maximum" must be understood in the extended sense described in Chapter II above.[1]

Let us now relax the assumption that production possibilities at a given moment are unbounded. Instead, let $f(k)$ have a finite upper bound, A. Then as $k \longrightarrow \infty$, the valuation for the optimum path must tend to

$$E\left(\int_0^\infty v(a.A.Z_t)\, e^{-rt} dt\right)$$

since the optimum policy when capital is infinite, but production finite, must be to invest at a rate zero. This function of a may be written $W(a)$. I shall assume that it is finite: the contrary case can be dealt with in the usual way.

On integrating equation (9) with respect to k, we get:

$$V = W(a) - \int_k^\infty v'(af(x) - s(x,a)).dx. \tag{15}$$

The first form of the fundamental equation, (12), becomes:

$$v(af(k)-s) + sv'(af(k)-s) = D\left(\int_k^\infty v'(af(x)-s(x,a)).dx\right) - DW(a) \tag{16}$$

On differentiating with respect to k, the last term, being a function

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(1) Among the simple corollaries of this characterization, it is easy to see that u must tend to zero as k tends to infinity $(a \neq 0)$.

of a alone, drops out. Thus the differential form of the fundamental equation remains unchanged.

From the definition of $W(a)$ it is easy to see that its first derivative is positive and its second negative. $W(a)$ itself is negative, like the total valuation.

This is an appropriate point at which to show that under certain conditions we can tell in advance whether the total valuation is finite or not. The total valuation for the investment strategy that is identically zero is

$$E\left[\int_0^\infty v(af(k_0)e^{\alpha t+\varepsilon_t}).\ e^{-rt}dt.\right]$$

If this is finite, then the maximal valuation (corresponding to the optimal strategy) must be finite, for it is greater than this and less than zero. Consider the example the case $v(c) = -c^{-n}$. Then the valuation for the zero strategy is

$$-(af(k_0))^{-n}\int_0^\infty e^{-rt-n\alpha t+n(n+1)\beta t}dt$$

since $E(e^{-n\varepsilon_t}) = e^{n(n+1)\beta t}$. Therefore the total valuation is finite if

$$r + n\alpha > n(n+1)\beta.$$

It will be seen later, when the numerical values of the parameters are discussed, that this condition is sufficient to ensure that the maximal valuation is finite in general.

\- 2 - <u>General Results.</u> Before entering upon the analytical details of attempted solutions of the fundamental equation, we can prove some elementary propositions, first about V, then about s.

PROPOSITION 1. V is an increasing function of k

- for, by (14), the derivative of V with respect to k is u, which is > 0.

PROPOSITION 2. V is an increasing function of a

- to prove this, I ask what happens to V if the value of a is increased from a_o to a_1. Suppose that we first hold the investment plans at precisely the same level - so that, for each t, and each value z_t of the random variable Z_t, the rate of investment is to be what it would have been optimally if the initial value of a were still a_o. Since

$$v(a_o z_t f(k_t) - s_t) < v(a_1 z_t f(k_t) - s_t)$$

for any particular values z_t and k_t of the random variables Z_t and K_t, we still have the same inequality after multiplying by the discount factor, integrating over time and applying the expectation operator. In other words, the total valuation has been increased while keeping investment plans constant - i.e. changing the investment strategy from $s(k,a)$ to $s(k, \frac{a_o}{a_1} a)$. Hence, a fortiori, the total valuation is larger when the optimal strategy $s(k, a)$ is used starting from $a = a_1$. This proves the result.

PROPOSITION 3. V is an increasing function of r and an increasing function of α.

I shall prove the second result; the first is proved in a similar way.

We can write $Z_t = e^{\alpha t} X_t$, where $E(X_t) = 1$. Consider a particular time path of X_t, $(x_t: t > 0)$. This would yield a particular valuation with the optimum strategy,

$$V(\underline{x}\,;\alpha) \equiv \int_0^\infty v(ae^{\alpha t}x_t f(k_t) - s(k_t, ae^{\alpha t}x_t))e^{-rt}dt,$$

starting from some given k and a. If α were increased, but the rate of investment at each moment maintained at the same level (i.e. no longer the optimum rate of investment), clearly the integral would be increased. Thus if $\alpha_1 > \alpha_2$,

$$\begin{aligned} V(\underline{x}\,;\alpha_2) &< V(\underline{x}\,;\alpha_1\,;\; s(\alpha_2)) \\ &\leq V(\underline{x}\,;\alpha_1\,;\; s(\alpha_1)) \end{aligned}$$

where $s(\alpha_1)$ is the optimum investment strategy when $\alpha = \alpha_1$. Taking expectations we can see that V is an increasing function of α, which was to be proved.

We cannot, it seems, prove a similar theorem about the dependence of V on β.

These theorems for the total valuation are quite easily proved, and by similar methods; they are not unexpected. But we are more interested in the characteristics of the optimum investment strategy itself. It seems to be difficult to prove similar general propositions about the investment strategy. I shall shortly introduce methods for finding approximate solutions to the problem, which will provide some information about the dependence of the strategy on the parameters when these parameters are small. In addition we shall get rather more information about the special cases. We can, however, say for the general case what will be the effect of introducing some of the parameters separately into the static case.

Our question is: if we compare an economy in which one of the valuation with the optimum strategy,

$$V(\underline{x}\,;\alpha) \equiv \int_0^\infty v(ae^{\alpha t}x_t f(k_t) - s(k_t, ae^{\alpha t}x_t))e^{-rt}dt,$$

starting from some given k and a. If α were increased, but the rate of investment at each moment maintained at the same level (i.e. no longer the optimum rate of investment), clearly the integral would be increased. Thus if $\alpha_1 > \alpha_2$,

$$\begin{aligned} V(\underline{x}\,;\alpha_2) &< V(\underline{x}\,;\alpha_1\,;\, s(\alpha_2)) \\ &\leq V(\underline{x}\,;\alpha_1\,;\, s(\alpha_1)) \end{aligned}$$

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Our question is: if we compare an economy in which one of the parameters r, α, β is non-zero, with a completely static economy, how do the optimal investment strategies for the two economies compare with one another? For convenience, since it will be referred to frequently, we write the optimum strategy for the static case as:

$$s(k,a) = \theta(af(k)) \tag{17}$$

We remember that θ is given by the equation:

$$v(af(k) - \theta) + \theta v'(af(k) - \theta) = 0 \tag{18}$$

PROPOSITION 4. If α and β are zero, but r is positive, the optimal strategy, s, is less than θ. (And if r is negative, it is greater than θ).

- For $v(af - s) + s\,v'(af - s) = rV, < 0$; and the derivative with respect to s of the left hand side of this inequality, viz. $- sv''(af - s), > 0.$ [1]

COROLLARY We can prove more. V is negative, and a decreasing function of r; Therefore, the larger is r, the smaller is the optimal rate of investment (when α and β are zero.)

PROPOSITION 5. If r and β are zero, but α is positive, the optimal strategy, s, is less than θ, (and greater than θ when α is negative.)

- For $\partial V/\partial a$ is positive (by (2)). [1]

We might expect some similar result for the dependence of the rate of investment on β. However, as in the case of propositions about

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(1) If $V = \infty$, the equation (13) can be used in the proof instead of (12).

V, we cannot. It will be seen later that <u>for small enough</u> β, the optimum rate of investment is an increasing function of β, so that up to a point greater uncertainty will require greater investment (a rather surprising result); but it does not seem to be possible to obtain readily a straightforward proof of the fact.

I can prove one other general result about the optimum rate of investment:

<u>PROPOSITION 6</u> $af'(k) > \frac{\partial s}{\partial k}$: i.e., optimal consumption increases with increasing <u>k</u>; <u>provided that</u> f(.) is a concave function.

- This is proved in the same way as proposition (3). If $f''(k) < 0$, the argument proves that $\frac{\partial^2 V}{\partial k^2} < 0$ (actually strict equality might hold for a set of measure zero, but that is not an interesting possibility.) Now $\frac{\partial V}{\partial k} = v'(c)$, and $v''(c) < 0$; hence $\frac{\partial c}{\partial k} = \frac{1}{v''} \frac{\partial}{\partial k} v'(c), > 0.$

It is time that I summarized the economic meaning of these propositions.

The parameter α represents investment-independent technical progress. Proposition 5 suggests that a nation ought to invest a smaller part of the national product when part of future economic growth can be expected to arise from investment-independent changes than when no such improvements are expected. This is common sense. The significance of proposition 3 is less than might appear at first sight. It tells us that the valuation is greater when, with the same function f(.), α is greater. When the dynamic parameters are zero, the optimum rate of investment is a function of the national output, independently of the form of the production function. Thus it makes sense to say that less should be invested in an economy with investment-independent technical progress, even although we shall naturally choose a different form for the production function when we believe there is technical progress. However the total valuation is certainly affected by the form chosen for the production function, and so we can draw virtually no interesting conclusions from what proposition 3 tells us about the dependence of V on α.

The fact that V is an increasing function of r is a little more interesting, because it does not seem to make sense: is an economy really better off when nuclear annihilation is more likely? In fact, what happens to V when r changes is not significant, because not invariant when constants are added to the instantaneous valuation functions. (The proposition is used in proving the corollary to proposition 4, of course.)

If we could show that V was a decreasing function of β, that would be significant. That we cannot, suggests the interesting possibility that under some circumstances greater uncertainty about the future may actually be desirable. At first sight it seems unlikely: yet increased uncertainty might mean a greater likelihood of reaching regions of the production possibility curve where greatly increasing returns are available. I cannot offer an example, but the possibility should be borne in mind, as it illustrates the sort of uncertainty that my model allows for.

The discount rate, r, represents valuation uncertainty - uncertainty about the future tastes of people, and their ability to appreciate the fruits of economic growth. When r is positive, we may say that valuation uncertainty is "pessimistic", for it is assumed that future generations will gain less from the investment in the intervening years than would our present selves (for example, because they may not be there at all.) It has been shown that the optimum rate of investment is smaller when valuation uncertainty is pessimistic; this corresponds to the frequently held notion that uncertainty ought to reduce the optimum rate of investment, since it acts like a pure time discount. On the other hand, when valuation uncertainty is optimistic (the contrary case), the optimum rate of investment is greater than when it is neutral (i.e. when the rate of time-preference is zero). It may be that when α and β are non-zero these calculations might have to be modified.

No general proposition has been proved about the effect of production uncertainty on the optimum rate of investment. We shall come to this most interesting question shortly.

It was shown in proposition 6 that, as the stock of capital increases, the rate of consumption should increase too - provided that increasing returns do not prevail. If for some range of possible capital-stocks there are increasing returns to additional capital (this is a very strong type of increasing returns), it seems possible that consumption ought to fall at some point.(1) If the economy reaches a stage where the next dose of capital yields a more than proportional increase in output, it might be worth depressing consumption in order to take advantage of the increasing returns at once.(2)

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(1) I am assuming at this point that no negative valuation is attached to a falling rate of consumption, *per se*. It is certainly tempting to suppose that one should be, on the grounds that habits are difficult to break. My puritan instincts suggest that this is a political constraint, not a moral one, in which case it means only that the optimum policy will not be followed.

(2) Perhaps *some* moral justification for certain stages of the industrial revolution could be given along these lines.

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(2) Perhaps some moral justification for certain stages of the industrial revolution could be given along these lines.

- 3 - A number of interesting questions remain unanswered: as to how the optimum policy will change when all three parameters are non-zero, in particular. It may be that not much can be said about these qualitative questions in full generality. In any case, what is important is to know something about the detailed properties of the optimum policy when the values of the dynamic parameters are small, and in particular cases that are likely - or ought - to be of practical interest. The special cases that are most tractable are pointed to by the last proposition of this section, which demonstrates the relatively simple functional form of the optimum investment strategy in a class of cases. It is preceded by another proposition showing a simple homogeneity property of the solutions, when the valuation function has constant elasticity.

PROPOSITION 7. Suppose that $v(c) = -c^{-n}$. The optimum rate of investment is a homogeneous function of degree 1 in the variables a, r, α, β, I.e.

$$\frac{s}{a} = h\left(k\,,\ \frac{r}{a}\ \frac{\alpha}{a}\ \frac{\beta}{a}\right) \tag{19}$$

for some function h .

This states, essentially, that a change in the unit of measurement of time cannot affect the amount of investment to be done in any fixed interval of time. If we begin with the year as a unit, and then consider what happens when we change to m years as the unit of time, we see that r , α, β will become mr, $m\alpha$, and $m\beta$, while a and s will become ma and ms.

The effect of a change in time-scale on the instantaneous valuation function has to be rather carefully elucidated: in general it is not independent of the time scale. If the unit of time in a new time-scale is T times the unit of time in the previous scale, the new instantaneous valuation function should be $Tv(c/T)$, $v(c)$ being the old one. These two functions are proportional, and therefore effectively the same, only when $v(c) = - Ac^{-n}$. Thus only in this case do we have the desired homogeneity.

It is natural also to ask what happens when the unit of product is changed. This leads us to

<u>PROPOSITION 8.</u> <u>Suppose the production function and the valuation function have constant elasticities:</u>

$$f(k) = k^b \; ; \qquad\qquad v(c) = - c^{-n}$$

<u>Then, as a function of a and k, the optimum strategy is given by:</u>

$$\frac{s(k,a)}{af(k)} = \sigma\left\{ \frac{k}{af(k)} \right\} \tag{20}$$

<u>for some function σ.</u>

The form (20) is suggested by dimensional considerations: it turns out that only in the constant elasticity case does it work.

Consider any investment strategy $i(k,a)$. Suppose that a, k, and i are changed in such a way that the ratios of k and i to $af(k)$ remain constant. Since the ratio i/k remains constant, k_t is also changed in the same propostion as k for all t. Hence $af(k_t)$ changes in the same proportion, since $f(k)$ has constant elasticity. Thus $C_t = aZ_t f(k_t) - i_t$ changes in the same proportion for all t. As the valuation function has constant elasticity, the total valuation is multiplied by a factor depending only on this proportion, and not on the particular investment strategy $i(k,a)$. Thus the optimum rate of investment, when a and k are changed in such a way that the ratio of k to $af(k)$ remains constant, is $s(a,k)$ increased in the same ratio as k. This proves the result.

COROLLARY

$$s(k,a) = af(k).\,\sigma\left(\frac{ak}{af(k)}, \frac{r}{\alpha}, \frac{\beta}{\alpha}\right),$$

when the production and valuation functions have constant elasticity.

This is a combination of the two propositions.

The great advantage of proposition 8 is that it allows us to reduce the fundamental equation for the problem to an ordinary differential equation, which is much easier to discuss than a partial differential equation. It turns out that only in one special case can the equation be solved explicitly. However it would be somewhat easier to do numerical computation for the ordinary differential equation, especially as the desired solution is defined by the awkward extremal property (14). In addition, it is possible to sketch the general character of the solution more easily when one has an ordinary equation to work with.

The resulting ordinary differential equation is not beautiful; but it looks a little better if we choose our variables carefully. Let us take as our independent variable

$$\eta = \log \frac{af(k)}{k} \qquad (21)$$

When $v(c) = -c^{-n}$, $u = nc^{-n-1} = n(af(k))^{-n-1}\left(1 - \frac{s}{af(k)}\right)^{-n-1}$. Let the dependent variable be

$$z = \left(1 - \frac{s}{af(k)}\right)^{-n-1}. \qquad (22)$$

Proposition 8 shows that this is a function of η. The rest is routine calculation.

$$a\frac{\partial z}{\partial a} = (n+1)z + (af)^{n+1}\frac{\partial u}{\partial a}\ ; \quad \text{and also} \ = \ z'(\eta).$$

$$a^2\frac{\partial^2 z}{\partial a^2} = n(n+1)z + 2(n+1)(af)^{n+1}a\frac{\partial u}{\partial a} + (af)^{n+1}a^2\frac{\partial^2 u}{\partial a^2}\ ;$$

$$\text{and also} \ = \ z''(\eta) - z'(\eta).$$

$$\frac{\partial z}{\partial k} = (n+1)b\frac{z}{k} + (af)^{n+1}\frac{\partial u}{\partial k}\ ; \quad \text{and also} \ = \ -\frac{1-b}{k}z'(\eta).$$

(In calculating this last expression, the fact that $f'(k) = b\frac{f}{k}$ is used.) Substituting in equation (13) after multiplying the whole equation by $(af)^{n+1}$, we find that:

$$\beta\frac{d^2 z}{d\eta^2} + (\alpha - (2n+3)\beta)\frac{dz}{d\eta} + ((n+1)(n+2)\beta - (n+1)\alpha - r)z$$
$$= e^{\eta}\left[(1-b)(1 - z^{-1/(n+1)})\frac{dz}{d\eta} + (nz - (n+1)z^{n/(n+1)})b\right] \qquad (23)$$
$$= e^{\eta}\left[(1-b)\frac{d}{d\eta}H(z) + nbH(z)\right],$$

where $H(z) = z - \frac{n+1}{n}z^{n/(n+1)}$. (24)

We can convert this into an equation for $\sigma = \frac{s}{af(k)}$:

$$\beta(n+1)(1-\sigma)\sigma'' + \beta(n+1)(n+2)(\sigma')^2 + (n+1)(\alpha-(2n+3)\beta) - (1-b)e^{\eta}\sigma)(1-\sigma)\sigma'$$
$$+ \left[(n+1)(n+2)\beta - (n+1)\alpha - r - be^{\eta}((n+1)\sigma - 1)\right](1-\sigma)^2 = 0. \qquad (25)$$

This is not a very helpful form when $\beta \neq 0$. However, when $\beta = 0$, and we take $\xi = e^{\eta} = af(k)/k$ as the dependent variable, we have the rather useful equation:

$$(\alpha - (1-b)\sigma\xi)\xi\frac{d\sigma}{d\xi} = (1-\sigma)\left(\alpha + \frac{r}{n+1} + b\xi\left(\sigma - \frac{1}{n+1}\right)\right). \qquad (26)$$

I shall return to these equations in due course.

Before ending this chapter I shall prove the basic result for the theory of approximation to the solution of the fundamental equation, which I shall want to use in later sections. I want to prove that when the dynamic parameters are small enough, the optimum rate of investment is relatively close to the optimum rate of investment for the static case. We shall see later that the effect of at least one of these parameters is likely to be numerically significant; but it is important to know whether the introduction of any of them leads to some discontinuity. It would be rather worrying if a small change in the assumptions - say from zero technical progress to half a per cent - made a very large difference to the result, especially if an infinitesimal change had a finite effect on the optimal policy. If that were the case, the range of policies that might be optimal (as far as one knew from the evidence) would be so great that the economist might not hope to give any answer to the policy maker in search of advice. Fortunately it is not so. In proving that it is not, we shall also see how to derive a first approximation to the solutions we seek.

Most of the work is done by a lemma, which is proved in the appendix. It states that, provided the valuation for the optimal strategy is finite for some non-zero β, with r and α zero, the total valuation tends, as the dynamic parameters tend to zero, to the valuation for the static case; and that this convergence is uniform for a in any closed interval not containing $a = 0$. This is a highly plausible result, and its proof is essentially routine, if a little tortuous.

Let us denote the valuation for the optimum strategy (i.e. the maximal valuation) by $V(k,a;\ r,\alpha,\beta)$. When r, , and are zero, we can write it $V_0(k,a)$. Since the convergence to V_0 is uniform, in some neighbourhood of any non-zero a, the derivatives of V also tend to V_o. Hence we can write:

$$DV(k,a;\ r,\alpha,\beta) = DV_o(k,a) + o(\sqrt{r^2+\alpha^2+\beta^2})$$

where $o(\delta)$ is the standard notation for a function tending to zero faster than δ. We can write $\sqrt{r^2+\alpha^2+\beta^2} = \delta$.

Returning to equation (10), we see that

$$v(af(k) - s) + sv'(af(k) - s) = - DV_o + o(\delta). \tag{27}$$

V_o and its derivatives are finite, so that the right hand side of this equation is "small". It follows that s is close to the solution when the right hand side is zero (since the left hand side is a continuous function of s), that is, to the optimal strategy for the static case, $s = \theta(af(k))$. More precisely, it is clear that the left hand side of equation (27) is a strictly increasing function of s (the derivative with respect to s is $- sv''(af-s)$, which is positive.) It is a differentiable function of s too, so that we can solve the equation for s to give

$$s = S(af(k),\ - DV_o + o(\delta)),$$

S being differentiable (at least twice) with respect to the second argument. Now we can expand by Taylor's theorem:

$$s = S(af(k),0) - S'(af(k),0).\ DV_o + o(\delta),$$

where S' is the derivative with respect to the second argument. $S(af(k),0)$ is just $\theta(af(k))$. $S'(af(k),0) = - [\theta(af)v''(af-\theta)]^{-1}$. Finally $V_o(k,a) = - \int_k^\infty v'[af(x)-\theta(af(x))].dx$. Thus we have a first approximation to the optimal policy:

$$s(k,a) = \theta(af(k) - \frac{1}{\theta(af)v''(af-\theta(af))} D\int_k^\infty v'[af\ (x)- \theta(af(x))]dx + o(\delta). \tag{28}$$

This is exactly the result that we should get if we set out to solve equation (12) on the assumption that $s(k,a) - \theta(af)$ must be of the same order of smallness as r, α, and β. It turns out that this is a valid assumption only then the integral (i.e. V_o) is finite. When V_o is not finite, it seems that the optimal strategy still tends to the strategy for the static case when the parameters tend to zero, although I do not have a general proof. I shall examine the question for some important special cases in the next two chapters.

The approximation given by (28) is not usually as good as one might hope. It will appear as we study it in greater detail that it is seldom sufficiently accurate when the correction that has to be made to the static case is significantly large. The problem of convergence is serious. It seems sensible to compute further approximations to the solution by substituting the last approximation to s in the right hand side of equation (12), then solving to obtain a further approximation - in other words repeating the process that leads formally to equation (28). At least in the interesting cases, this process does not converge, and may not converge sufficiently in the first few approximations to give a satisfactory degree of accuracy. It is possible to get round this difficulty to some extent in the important special cases, but in general the problem of convergence presents a challenge that I have not been wholly able to meet.

CHAPTER V TECHNICAL PROGRESS

- 1 -

Before introducing uncertainty, we shall find out what we can about the influence of technical progress alone. We consider what the optimum investment policy is when the production function is

$$y = af(k)e^{\alpha t}. \tag{29}$$

This is a familiar production function, especially in its "Cobb-Douglas" version:

$$y = ak^{b}e^{\alpha t}. \tag{30}$$

Economists, realising that the theory of long-run equilibrium under fixed production possibilities is not directly relevant to problems of long-run growth, have reacted in a number of ways. The more mathematically inclined stick time into the production function, usually in such a form as (29). (As I am keeping the labour force constant, there is only one "factor of production.") In particular the form (30) has proved attractive to the econometrician. There is no doubt that the careless use of such extended production functions can lead to some strange economics. In particular there is not much to be said for estimating the relative effects of capital investment and "education + organisation" by fitting a form like (30) to national time-series. Yet, since long-range prediction of output is something we are not very good at, fitting something like (30) <u>might</u> tell us more about future production than we should discover by simply projecting a trend.

Much *a priori* argument has been spent on showing that this last hope is, at best, highly optimistic. Such argument is not entirely irrelevant to planning problems. It is true that good performance as a predictor is not a good guide. Checking predictions against the facts is usually done for countries where no forced change in the proportion of output devoted to investment is being made in the long-run; while it is precisely such an acceleration of the rate of investment that interests us most. At the present stage of empirical enquiry, we cannot, I think, commit ourselves to any particular values of the parameters b and α; but we can indicate how much difference various possible estimates would make to the optimum rate of investment. It seems to me that such an exercise is worthwhile, for, despite all the serious limitations of using (29) or (30) macro-economically - some of which I shall come to, and emphasise, later - they do provide a plausible form for predicting the future, a form which is also mathematically usable. Even if it is a form that economists may hope to grow out of, we shall do well to elaborate its implications while it is the practicable method of prediction.

\- 2 -

We can calculate the first approximation to the optimum strategy from (28):

$$s(k,a) = \theta(af) - \frac{1}{\theta(af)v''(af-\theta(af))}\left(-r\int_k^\infty v'(af(x) - \theta(af))dx + \alpha a\int_k^\infty v''.(f(x) - \theta'(af)f).dx\right).$$

It is convenient to write y for $af(k)$. θ is defined by the equation:

$$v(y - \theta) + \theta v'(y - \theta) = 0$$

If we differentiate this with respect to y, we find that

$$(1 - \theta'(y))\theta v''(y - \theta) = - v'(y - \theta). \tag{31}$$

Using this result, we can write the first approximation:-

$$s(k,a) = \theta(y) - \frac{1-\theta'(y)}{v'(y-\theta)} \int_k^\infty (r + \alpha \frac{af(x)}{\theta(af(x))}) v'[af(x) - \theta(af(x))] dx \tag{32}$$

From (31) it is clear that $1 - \theta$ is positive: hence the first perturbation - the correcting term in (32) - is negative, as it should be.

When $v(c) = - c^{-n}$, we know that $\theta(y) = y/(n+1)$. Thus it is easy to calculate the first approximation:

$$s(k,a) = \frac{y}{n+1} - \frac{n}{n+1} (r + (n+1)\alpha)(f(k))^{n+1} \int_k^\infty (f(x))^{-n-1} dx. \tag{33}$$

The perturbation is zero when $r + (n+1)\alpha = 0$: not a very likely contingency. It is more illuminating to say - a little roughly - that α is $(n+1)$ times as effective as r in its effect on the optimum policy. It seems that the importance of r is not much affected by the magnitude of n : if anything it will be a little greater when n is small. The importance of α, however, grows with n.

Of course the first approximation looks simplest when $f(k) = k^b$. Then:

$$s(k,a) = \frac{y}{n+1} - \frac{n(r + (n+1)\alpha)}{(n+1)(b(n+1) - 1)} k. \tag{34}$$

We must remind ourselves that this form of the first approximation is only valid when the integral in (33) is convergent - i.e. when $b(n+1) > 1$.

It is apparent that, although the optimum investment ratio is quite independent of the production function in the static case, a larger capital-output ratio in this more general case implies a smaller rate of investment per unit of output (to this degree of approximation). To illustrate the magnitude of the perturbation, we can calculate the approximation for a few representative values of the variables. Suppose first that $r = 0$. If $b = 1$,

$$\frac{s}{y} = \frac{1}{n+1} - \alpha k/y.$$

If there were an investment-independent improvement in output of 1% per annum, and the capital-output ratio were 3, the investment ratio would be reduced by .03 from that for the static case. (The imposition of a 2% rate of time-preference would mean a reduction of another 3% if n were 1, less if n were greater.) When $b = 3/4$ and $\alpha = 1\%$ (corresponding to an equilibriem growth rate of 4%), the perturbation is

$$0.04 \frac{n}{3n-1} k/y.$$

If the capital-output ratio is 3, this is 6% when $n = 1$, 4.8% when $n = 2$, 4.5% when $n = 3$. When $b = 1/2$ and $\alpha = 2\%$, the perturbation to the investment-output ratio is 24% when $n = 2$ and 18% when $n = 3$: which yield first approximations to s/y of 9.3% and 7%, respectively! Clearly these last approximations are not very accurate.

As one might expect, the expression for the second approximation is very complicated, but it is worth going another step to see what happens. We want some notation for the successive perturbations now, so let us write:

$$s(k,a) = s_0(k,a) + s_1(k,a) + s_2(k,a) + \ldots \qquad (35)$$

s_0 is, of course, $\theta(af(k))$. The ratio of each perturbation to the previous one tends to zero as the dynamic parameters tend to zero, but none is identically zero. Let us not trouble about the rigorous details this time, although they could be provided in the same way as for the first approximation. It is quite sufficient, in order to find out what the second perturbation looks like, to substitute (35) in equation (12) and expand up to terms of second order in the parameters. Expanding $v + sv'$ gives:

$$- \theta v''(y-\theta(y)).(s_1 + s_2) + \tfrac{1}{2}(\theta v''' - v'')(s_1)^2 . \qquad (36)$$

It can be verified, from the equation $v + \theta v' = 0$, that

$$\frac{\theta v''' - v''}{-\theta v''} = \frac{2(1-\theta') - \theta\theta''}{\theta(1-\theta')^2} > 0. \qquad (37)$$

The expansion of the right hand side, $D\int_k^\infty v'[af(x) - s(x,a)]dx$, is:

$$D\int_k^\infty v'[af(x) - \theta(af(x))].dx - D\int_k^\infty s_1(x,a).\ v''.dx . \qquad (38)$$

Equating (36) and (38), and remembering the equation for the first perturbation, we have:

$$s_2 = - \frac{2(1-\theta') - \theta\theta''}{2\theta(1-\theta')^2}(s_1)^2 + \frac{1-\theta'}{v'(y-\theta)}\left(r - \alpha a\frac{\partial}{\partial a}\right)\int_k^\infty s_1(x,a)v''(af-\theta)dx$$

$$(39)$$

It is not really worth while evaluating the derivative with respect to a in general symbols: it can easily be done for each particular case that might interest us. Consider for instance the case

$v(c) = -c^{-n}$. s_1^1 is given by (33). We have:

$$s_2 = -\frac{(n+1)^2}{ny}\ \frac{n^2(r+(n+1)\alpha)^2}{(n+1)^2}(f(k))^{2n+2}\left[\int_k^\infty (f(x))^{-n-1}dx\right]^2$$

$$+ \frac{1}{a}(r+(n+1)\alpha)\frac{1}{n}\left(\frac{n}{n+1}\right)^{n+2}(f(k))^{n+1}(r+(n+2)\alpha)\int_k^\infty n(n+1)\left(\frac{n}{n+1}\right)^{-n-1}\frac{dx}{f(x)}\int_x^\infty (f(\xi))^{-n-1}d\xi.$$

$$= n(r+(n+1)\alpha)(f(k))^{n+1}a^{-1}\Big\{(r+(n+2)\alpha)\int_k^\infty\int_x^\infty [f(\xi)]^{-n-1}d\xi . \frac{dx}{f(x)}$$

$$- (r+(n+1)\alpha)(f(k))^n\left[\int_k^\infty (f(x))^{-n-1}dx\right]^2\Big\}. \qquad (40)$$

When $f(k) = k^b$, this looks much simpler:

$$s_2 = n\frac{(r + (n+1)\alpha)[(1-b)r+n\alpha]}{[b(n+1) - 1]^2\,[b(n+2)-2]}\;\frac{k^2}{y}\,. \tag{41}$$

The approximation only holds good when the integrals in (40) are convergent. In the special case - (41) - this condition means that $b(n+2)$ must be greater than 2. If b is much smaller than 1, we have a valid second approximation only for rather large n. For instance, if $b = 0.5$ (a value that some might consider rather large), we can use (41) only if n is greater than 2: and, as one would expect, the approximations are not very good. If one attempts to calculate third and higher approximations one finds that factors $b(n+3) - 3$, $b(n+4) - 4$, and so on appear in the denominators. As far as the Cobb-Douglas case goes, one cannot easily correct a bad second approximation by pushing the approximation process further. The situation can be illustrated by a table of numerical values. I take a Cobb-Douglas case with $k/y = 3$.

TABLE I.

r%	b	α%	n	θ/y	1st. perturbation	2nd. perturbation	2nd. approximation
0	1	1	1	0.500	- 0.030	0.002	0.472
			2	0.333	- 0.030	0.001	0.304
			3	0.250	- 0.030	0.001	0.221
0	3/4	1	1	0.500	- 0.060	0.029	0.469
			2	0.333	- 0.048	0.007	0.292
			3	0.250	- 0.045	0.004	0.209
0	3/4	2	1	0.500	- 0.120	0.115	(0.495)(unreliable)
			2	0.333	- 0.096	0.029	0.266
			3	0.250	- 0.090	0.018	0.178
0	1/2	2	3	0.250	- 0.180	0.259	completely unreliable
2	3/4	1	2	0.333	- 0.050	0.014	0.297

The first six entries seem to be reasonably good approximations, since the second parturbation is not large. The seventh entry is very dubious. When $b = 1/2$, it is clear that we can get no numerical information worth having for acceptable values of n, at any rate by the methods so far used. The last entry illustrates the effect of a non-zero rate of time-preference: to the degree of accuracy given by the second approximation, which seems to be reasonably high in this case, a two percent time-preference alters the optimum investment ratio by only half a percent. When the capital-output ratio is larger, the second perturbation is relatively larger, and the second approximation consequently less accurate.

\- 3 -

In our quest for more accurate means of estimating the optimum rate of investment it is instructive to examine the one case that can be solved explicitly: it will make clear the meaning of the "asymptotic series" of approximations with which we have been working. This convenient, but unfortunately not particularly useful, case arises when $b = 1$, so that we have a production function: $y = ake^{\alpha t}$. The ordinary differential equation (26) becomes, since $\xi = af(k)/k$ is now a:

$$\alpha \cancel{a} \frac{d\sigma}{da} = (1 - \sigma)\left(\alpha + \frac{r}{n+1} + a\left(\sigma - \frac{1}{n+1}\right)\right) \tag{42}$$

Notation is easier if we write γ for $1 + \frac{r/\alpha}{n+1}$. Multiply (42) by $a^{\gamma - 1}$:

$$\alpha \frac{d}{da}\left((1-\sigma)\, a^{\gamma}\right) = (1-\sigma)^2 a^{\gamma} - \frac{n}{n+1}(1-\sigma)a^{\gamma},$$

which may be written as a linear differential equation:

$$\alpha \frac{d}{da}\left[(1-\sigma)a^{\gamma}\right]^{-1} - \frac{n}{n+1}\left[(1-\sigma)a^{\gamma}\right]^{-1} = -a^{-\gamma}$$

This is solved in the routine way to give the solution:

$$1 - \sigma = \frac{\left(\frac{a}{\alpha}\right)^{-\gamma} e^{-(n/(n+1)).(a/\alpha)}}{\int_{a/\alpha}^{\infty} X^{-\gamma} e^{-(n/(n+1))X} dX} \tag{43}$$

The upper limit of the integral is infinity, because all other solutions of the equation make $1-\sigma$ become negative for large a, that is, for large enough t; which is impossible for the solution we seek. The right hand side of (43) is a function of $a/\alpha = af(k)/(\alpha k)$, as it should be. The character of the function is interesting. It is not an analytic function at $\frac{\alpha}{a} = 0$, and so cannot be expanded in a convergent series of powers of α/a. It can however be expanded in an asymptotic series. Such a series, if truncated at any term, differs from the function in question by a quantity that is of smaller order in the variable than the last term included in the series.

Write $\zeta = \frac{n}{n+1} \cdot \frac{a}{\alpha}$. Then

$$\frac{1}{1-\sigma} = \frac{n+1}{n}\int_\zeta^\infty \left(\frac{x}{\zeta}\right)^{-\gamma} e^{-(x-\zeta)}\,dx = \frac{n+1}{n}\int_0^\infty \left(1+\frac{x}{\zeta}\right)^{-\gamma} e^{-x}\,dx .$$

We are interested in what happens when ζ is large, so we expand $(1+x/\zeta)^{-\gamma}$ in powers of x/ζ. It is quite easy to show that this really does give an asymptotic expansion, provided we proceed carefully. The integral is

$$\int_0^\zeta \left[1 - \frac{x}{\zeta} + \frac{\gamma(\gamma+1)}{2}\left(\frac{x}{\zeta}\right)^2 - \dots\right] e^{-x}\,dx + \int_\zeta^\infty \left(1+\frac{x}{\zeta}\right)^{-\gamma} e^{-x}\,dx.$$

The second integral here is only of order $e^{-\zeta}$ at the most, which tends to zero faster than any negative power of ζ. Integrating by parts repeatedly shows that

$$\int_0^\zeta x^n e^{-x}\,dx = n! - O(\zeta^n e^{-\zeta}),$$

and again we can throw out the term of exponential order. The asymptotic expansion is:

$$\frac{1}{1-\sigma} = \frac{n+1}{n}\left\{\sum_{\nu=1} (-)^\nu \frac{\gamma(\gamma+1)\dots(\gamma+\nu-1)}{\zeta^\nu} + 1\right\} \qquad (44)$$

This series gives a figure for σ that is accurate at least to the nearest percentage point when ζ is ten or more. For example, if the capital-output ratio is 3 and α is 3%, r being zero, one can calculate that the optimum investment-output ratio is 27% when $n = 2$, 18% when $n = 3$. Thus the asymptotic expansion is sufficiently accurate for all conceivably practicable cases when output is proportional to capital.

- 4 -

We should like to be able to write down some such function as (43) in other cases too, for then numerical integration could give us an answer where the series solution is no help. It seems that we cannot do anything so simple, but we can hope to obtain a series of explicit and calculable functions that will - or at any rate might - converge. I turn to this possibility now. It seems that we have to take a roundabout route, or rather, we have to go almost back to the beginning again. We shall, however, obtain a technique for finding more rapidly convergent approximations, and also go a long way towards solving the case where the valuation integral for the static case fails to converge.

We know that the optimum strategy and the valuation of the economy for that strategy satisfy the equation

$$v(y - s(k,a)) + s(k,a)v'(y - s(k,a)) = (- r + \alpha a \frac{\partial}{\partial a})(- V(k,a)).$$

I have been using this equation in the form (12), where V is expressed as a functional of s without the intervention of the dynamic parameters. We did so because (12) is the simplest form of the fundamental equation, and leads directly to the asymptotic expansion for the solution in powers of the parameters. V , however, is also given by its definition:

$$V(k,a) = \int_0^\infty v(ae^{\alpha t}f(k_t) - s(k_t, ae^{\alpha t})) . e^{-rt}dt, \qquad (45)$$

where $dk_t/dt = s(k_t, ae^{\alpha t})$, and $k_t = k$ when $t = 0$. Let us denote the valuation of the economy when a strategy $i(k,a)$ is used by $V(k,a: i)$. Then $V(k,a: i) \leqq V(k,a)$. $V_o(k,a)$, remember, is the maximal valuation of the static economy, the optimal strategy being $\theta(af(k))$. It is quite easy to see that

$$V(k,a: \theta) \geqq V_o(k,a);$$

for $\theta(af(k))$ is an increasing function of a and of k , so that k_t is greater when α is not zero than when it is (for every t) ; and so y_t is greater when $\alpha \neq 0$, which means that $y_t - \theta(y_t)$ is, since $1 - \theta' > 0$.

We know already that $V(k,a) - V_o(k,a)$ tends to zero as α and r tend to zero. It follows at oncethat

$$V(k,a) - V(k,a: \theta) \longrightarrow 0$$

as α and r tend to zero. And the convergence is uniform in a since it is uniform for the former case. Thus convergence remains after differentiation with respect to a . Hence, just as with the previous method of approximation,

$$v + sv' = - DV(k,a: \theta) + o(\delta). \qquad (46)$$

We can solve this equation for s : this step will usually involve a further approximation, of any desired order.

I cannot prove that approximations so obtained are better than the previous ones. However it is plausible that they should be. We know that $V(k,a: i)$ is a maximum when $i = s$. Thus a small change in the investment strategy from s should make a "very small" difference to V. It should be a good policy to estimate V by calculating it for a strategy that we know to be relatively close to the optimum. Investigating the goodness of the approximation rigorously would be interesting, no doubt, but its usefulness for my purposes - and others, - will become apparent soon enough.

It is clear that we should gain nothing by making (48) more explicit for general functions v and f. The equation would become more complicated without any corresponding gain in comprehensibility. It is sufficient to note in passing that (48) provides us with a meaningful approximation in many cases where the valuation for the static case is infinite. We can expect it to work when the maximal valuation for the economy is finite. It was remarked earlier that this is usually the case when $\alpha > 0$, and always the case when both α and r are greater than zero.

This and other benefits are balanced on the debit side by the computational awkwardness of the method. Even the determination of the first step, which we may conveniently call the improved first approximation, would usually require numerical integration. In order to repeat the process, much more complex numerical methods would be required. First the path of capital would have to be determined by numerical integration of a differential equation specified only in numerical terms. Then further numerical integrations would have to be performed. In principle, all these calculations could be programmed for a computer. This is important, because in our previous discussion, at no point did the specification of the solution seem to lend itself to numerical techniques. The present method of approximation may well converge, if it does for some functions v and f, we have a possible means of calculation for those cases. There are other means of computation, and we shall say something about them later.

The most interesting case, for getting some impression of the character of optimal policy, is the constant-elasticity case. Let

$v(c) = -c^{-n}$ and $f(k) = k^b$ $(n > 0,\ b < 1)$. The static optimal policy is

$$\theta = \frac{ak^b}{n+1}.$$

Since $\theta = \dot{k}$, capital at time t is (when $b < 1$)

$$k_t = \left[k^{1-b} + (1-b)\frac{a}{n+1}\frac{1}{\alpha}(e^{\alpha t} - 1)\right]^{1/(1-b)}, \qquad (47)$$

k being the quantity of capital at time zero. It follows that

$$V(a,k{:}\ \theta) = -\left(\frac{n+1}{n}\right)^n a^{-n}\int_0^\infty e^{-(r+n\alpha)t}\left[k^{1-b} + \frac{1-b}{n+1}\frac{a}{\alpha}(e^{\alpha t} - 1)\right]^{\frac{-nb}{1-b}} dt$$

We know that the investment-output ratio, σ, is a function of $af(k)/k = ak^{b-1}$ (which we had denoted by ξ) and the parameters. Indeed it is a function of ak^{b-1}/α and r/α. Let us use ξ to represent ak^{b-1}/α now; and recall the definition of $\gamma = 1+r/[(n+1)\alpha]\xi$. Making the substitution $e^{\alpha t} - 1 = x$ in the integral, we find that

$$V(a,k{:}\ \theta) = -\frac{1}{\alpha}\left(\frac{n+1}{nak^b}\right)^n\int_0^\infty (1 + x)^{-(n+1)\gamma}\left(1 + \frac{1-b}{n+1}\xi x\right)^{\frac{-nb}{1-b}}.dx$$

We have to apply the operator $-r + \alpha a\frac{\partial}{\partial a}$ to this. The derivative of the right hand side with respect to a is

$$-\frac{nV}{a} + \frac{nb}{n+1}\frac{k^{b-1}}{\alpha^2}\int_0^\infty (1 + x)^{-(n+1)\gamma}\, x\left[1 + \frac{1-b}{n+1}\xi x\right]^{-\frac{nb}{1-b}-1}.dx.\left(\frac{n+1}{nak^b}\right)^n.$$

It then appears that

$$DV(a,k:\theta) = \left(\frac{n+1}{nak^b}\right)^n \left(n+1+\frac{r}{\alpha}\right) \int_0^\infty (1+x)^{-\frac{r}{\alpha}-n-2} \left(1+\frac{1-b}{n+1}\xi x\right)^{-\frac{nb}{1-b}} .dx \tag{48}$$

For calculation, this is a convenient form of the integral: we can make it a little more convenient by transforming to $X = 1/(1+x)$:

$$DV(a,k:\theta) = \left(\frac{n+1}{nak^b}\right)^n \left(n+1+\frac{r}{\alpha}\right) \int_0^1 X^{\frac{r}{\alpha}+\frac{n}{1-b}} \left[\Lambda - (\Lambda-1)X\right]^{\frac{-nb}{1-b}} .dX \tag{49}$$

where $\Lambda = \frac{1-b}{n+1}\xi$. It may be noted that when the exponents in the integral are whole numbers the integral can be evaluated exactly.

If one is interested in the asymptotic behaviour of the improved approximation, it is better to transform (48) by dividing the variable of integration by ξ. Then we have:

$$DV = \left(\frac{n+1}{nak^b}\right)^n \frac{n+1+r/\alpha}{\xi} \int_0^\infty \left(1+\frac{x}{\xi}\right)^{-r/\alpha-n-2} \left(1+\frac{1-b}{n+1}x\right)^{-\frac{nb}{1-b}} dx. \tag{50}$$

In order to calculate the approximation, we must return to equation (46). The left hand side, $v + sv'$, is in the present case

$$-(ak^b)^{-n}(1-\sigma)^{-n} + n(ak^b)^{-n}\sigma(1-\sigma)^{-n-1},$$

which we can write

$$(ak^b)^{-n}\left[\frac{n}{1-\sigma} - (n+1)\right](1-\sigma)^{-n}$$

Thus the improved first approximation is the positive solution of:

$$\frac{n}{1-\sigma} - (n+1) = -(1-\sigma)^n \left(\frac{n+1}{n}\right)^n \left(n+1+\frac{r}{\alpha}\right). I(\xi), \tag{51}$$

where $I(\xi)$ is the integral in (48) or (49), or $1/\xi$ times the integral in (50).

It is not really of any practical interest to derive the improved first approximation for $b = 1$, since we already know the exact solution.

For completeness, it may be recorded that in this case

$$I(\xi) = \int_0^\infty (1+x)^{-r/\alpha - n-2} e^{-\frac{n}{n+1}\xi x} dx \tag{52}$$

It is interesting to compare the asymptotic expansion of the improved first approximation with the asymptotic expansion obtained by the first approximation method. The asymptotic expansion of $I(\xi)$ in powers of $1/\xi$ is obtained by expanding $(1+x/\xi)^{-r/\alpha-n-2}$ in powers of x in the integral in expression (50). We cannot obtain an infinite asymptotic expansion, as the terms become divergent integrals after a certain point. The coefficient of ξ^{-m} is

$$(-)^m(\tfrac{r}{\alpha}+n+2)\dots(\tfrac{r}{\alpha}+n+m+1)\,\frac{1}{m!}\int_0^\infty x^m(1+\frac{1-b}{n+1}x)^{-\frac{nb}{1-b}} dx.$$

This integral can be evaluated by repeated partial integration. It is

$$(\frac{1-b}{n+1})^{-m-1}\frac{m!(1-b)^{m+1}}{((n+1)b-1)((n+2)b-2),,,((n+m+1)b-m-1)}. \quad ((n+m+1)b > m+1.)$$

Let us define

$$\gamma_m = \frac{r/\alpha+n+m}{(n+m)b - m} \tag{53}$$

Then the coefficient of ξ^{-m} in $\xi I(\xi)$ is

$$(-)^m(n+1)^{m+1}((n+1)b-1)^{-1}\,\gamma_2\gamma_3 \cdot\cdot\, \gamma_{m+1}\,.$$

Hence, from (51), if we write Z for $\dfrac{n}{(n+1)(1-\sigma)}$, we have asymptotically

$$Z^{n+1} - Z^n \sim -\sum_{m=1}^{M} (n+1)^{m-1}\,\gamma_1\gamma_2 \cdot\cdot\, \gamma_m \cdot \xi^{-m}\,(-1)^{m+1}\,, \tag{54}$$

so long as $(n+M)b > M$.

We can confirm that this expression gives us not only the first approximation, but the second as well. We know that the second approximation is

$$\sigma \sim \frac{1}{n+1} - \frac{n}{n+1}\gamma_1 \frac{1}{\xi} + n(\gamma_1\gamma_2 - \gamma_1^2)\cdot\frac{1}{\xi^2}$$

It follows that, for the second approximation,

$$Z \sim 1 - \frac{\gamma_1}{\xi} + [(n+1)\gamma_1\gamma_2 - n\gamma_1^2]\frac{1}{\xi^2}$$

Hence

$$Z^{n+1} - Z^n \sim -\frac{\gamma_1}{\xi} + (n+1)\frac{\gamma_1\gamma_2}{\xi^2}.$$

This proves my assertion.

The asymptotic expansions do not agree beyond this point, however. This is most easily shown by taking the particular case $b = 1$, for which we know the whole asymptotic expansion, but I shall not reproduce the calculations here. At any rate, we now know that the improved first approximation is a reasonably good estimate in cases where the asymptotic expansion can be used; and further that it can be used in cases where the former approach fails. Clearly this is the approximation to use: but unfortunately, even in the special case, it is very difficult to push the process of approximation further.

In order to find the second improved approximation, we must find the valuation when the first improved approximation to the optimal policy is employed. In principle, we know the first improved approximation in the form $\sigma(\xi)$, where $\xi = ak^{b-1}/\alpha$. If ξ_t is the value of ξ at time t,

$$\xi_t = ak_t^{b-1}e^{\alpha t}/\alpha.$$

Hence

$$\frac{d\xi_t}{dt} = \alpha\xi_t + a(b-1)k_t^{b-2}\cdot ak_t^b\sigma(\xi_t)e^{2\alpha t}\frac{1}{\alpha},$$

$$= \alpha(\xi_t - (1-b)\xi_t^2\sigma(\xi_t)). \qquad (55)$$

This equation is integrable: one can write down t as a function of ξ_t, and, in principle, find ξ as a function of t. Indeed one can go on to write the valuation down explicitly. For what it is worth, here it is:

$$V = -\int_{\xi_0}^{\xi_\infty} \exp\left[-\left(\frac{r}{\alpha} + \frac{n}{1-b}\right)\int_{\xi_0}^{\xi} \frac{dx}{x[1-(1-b)x\sigma]}\right] \xi^{\frac{nb}{1-b}} (1-\sigma(\xi))^{-n} \frac{d\xi}{(1-(1-b)\xi\sigma)} . \qquad (56)$$

The idea is to change the variable of integration from t to ξ. $\xi_0 = ak^{b-1}/\alpha$. ξ_∞ is the value that ξ tends to as t tends to infinity. This is a question that I shall look at more fully later. Meanwhile we may notice that ξ cannot tend to ∞ unless σ tends to zero along with it: which it clearly cannot do, as can be seen from (51) and the fact that $I(\xi)$ tends to zero as ξ tends to ∞. Thus, by (55),

$$(1-b)\xi_\infty \sigma_\infty = 1 , \qquad (57)$$

where σ_∞ is the limit of σ. Naturally $\sigma_\infty = \sigma(\xi_\infty)$, and is determined in this case from (51). (51) and (57) provide two simultaneous equations for ξ_∞ and σ_∞.

Having shown how the second improved approximation can be derived, I do not propose to make any more use of it in this thesis, because it is so awkward to handle, and indeed cannot be easily calculated without the use of long-winded numerical techniques that I do not want to use here. I shall content myself with illustrating the improved first approximation by calculating it for a number of special cases. It may be remarked that the improved approximation process can be used at the point in the series of straightforward approximations where divergence begins to appear; this would seem to be the best treatment for awkward cases.

As I remarked earlier, the integral in (49) can be evaluated exactly when $r/\alpha + n/(1-b)$ and $-nb/(1-b)$ are whole numbers. In this way it is relatively easy to deal with certain cases. Let us denote the two numbers by M and $-N$. Unless r is negative (a case that I am excluding) $M > N$. Then repeated integration by parts shows that

$$\int_0^1 X^M (A - X)^{-N} dX = \frac{(A-1)^{-N+1}}{N-1} - \frac{M.(A-1)^{-N+2}}{(N-1)(N-2)} + \ldots$$

$$+ (-1)^{N-2} \frac{M(M-1)..(M-N+3).(A-1)^{-1}}{(N-1)(N-2)..2.1}$$

$$+ (-1)^{N-1} \frac{M(M-1)..(M-N+2)}{(N-1)!} A^{M-N+1} \left(\log \frac{A}{A-1} - \frac{1}{A} - \frac{1}{2A^2} - \ldots - \frac{1}{(M-N+1)A^{M-N+1}}\right)$$

When $N = 1$, only this last term occurs: its coefficient is then 1. In our case, A is $\Lambda/(\Lambda-1)$; and to obtain $I(\xi)$, the above integral must be multiplied by $(\Lambda-1)^{-N}$, which is $(A-1)^N$.

In order to get a representative range of values of b, I shall consider $b = 3/4$, $1/2$, and $1/4$. As before, n will take the values 1, 2, and 3. As some of the formulae are very long to write out, I shall only give a few examples. In all of them, I take $r = 0$.

<u>$b = 3/4$, $n = 1$.</u>

$$Z - Z^2 \sim \gamma I(\xi) = \frac{(\Lambda - 1)^{-4}}{2}(\Lambda^3 - 7\Lambda^2 - 7\Lambda + 1) + \frac{6\Lambda^2}{(\Lambda-1)^5}\log\Lambda .$$

(Since $r = 0$, $\gamma = 1$.)

e.g., if $\alpha = 1\%$, $k/y = 3$, we have $\Lambda = 4\frac{1}{6}$; and $\sigma = 0.45$. (which we shall see is correct.)

<u>$b = 1/2$, $n = 1$.</u>

$$Z - Z^2 \sim (\Lambda - 1)^{-3}\left[\Lambda^2 \log\Lambda - \tfrac{1}{2}(3\Lambda - 1)(\Lambda - 1)\right].$$

e.g., if $\alpha = 2\%$, $k/y = 3$, we find that $\sigma = 0.32$ (which is a little small, in fact.) It is apparent in this example that the logarithmic term is dominant for large Λ (corresponding to large ξ). The solution for large ξ (remembering that $Z > 1/2$ when σ is positive) is:

$$\sigma \sim \tfrac{1}{2} - \frac{2}{\xi}(\log \xi - \log 4)$$

It will be recalled that in this case the valuation integral for the static case diverges.

<u>$b = 1/2$, $n = 3$.</u>

$$Z^3 - Z^4 \sim \frac{1}{4}(\Lambda - 1)^{-6}(2\Lambda^5 - 22\Lambda^4 - 57\Lambda^3 + 23\Lambda^2 - 7\Lambda + 1)$$
$$+ 15(\Lambda - 1)^{-7}\Lambda^4 \log \Lambda .$$

<u>$b = 1/4$, $n = 3$.</u>

$$Z^3 - Z^4 \sim (\Lambda - 1)^{-5}\Lambda^4 \log \Lambda - \frac{1}{12}(\Lambda - 1)^{-4}(14\Lambda^3 - 23\Lambda^2 + 13\Lambda - 3).$$

Again the logarithmic term is dominant for large ξ.

It will be clear from these examples that we cannot hope to write down the improved first approximation in finite form. These formulae allow us to calculate it for different values of ξ, so that the functions could be mapped on a graph.

What can be done when the exponents in the integral are not whole numbers? Fortunately we can derive a convergent series for the improved first approximation, so that there is no need to resort to numerical integration. Returning to (49) we expand the second factor in the integrand in powers of $\frac{\Lambda - 1}{\Lambda} X$ and integrate. This is valid because $(\Lambda - 1)/\Lambda < 1$, so that the series being integrated is uniformly convergent.

$$I(\xi) = \Lambda^{-\frac{nb}{1-b}} \sum_{m=0}^{\infty} \frac{1}{m!} \left(\frac{nb}{1-b}\right)\left(\frac{nb}{1-b} + 1\right) \cdots \left(\frac{nb}{1-b} + m - 1\right) . \left(\frac{\Lambda - 1}{\Lambda}\right)^m \int_0^1 X^{\frac{r}{\alpha} \pm \frac{n}{1-b} + m} dX$$

$$= \Lambda^{-B} \sum_{0}^{\infty} \frac{B(B+1)\,..\,(B+m-1)}{m!} \left[\frac{r}{\alpha} + \frac{n}{1-b} + m\right]^{-1} \left(\frac{\Lambda-1}{\Lambda}\right)^{m} , \qquad (58)$$

where $B = nb/(1-b)$.

The series (58) converges fastest when the approximation is least accurate - viz., when Λ is not very much greater than 1 . Nevertheless it converges fast enough to be useful, even for quits large values of Λ. When $B < 1$ (i.e., $(n+1)b < 1$) , it converges even as $\Lambda \longrightarrow \infty$. In this case, $I(\xi) \sim \Lambda^{-B} \times$ a function of $\frac{r}{\alpha}$: this gives the asymptotic form of the optimal policy when $B < 1$, as will be confirmed later.

These methods of approximation that we have been studying are sufficient to calculate tolerably good approximations to the optimal investment policy for the whole range of possible production functions $y = ak^{b}e^{\alpha t}$, at any rate when α and the capital-output ratio are fairly small. They could be used for any functions v and f . In the constant-elaxticity case, however, there is a method whereby the optimal policies can be calculated to any desired degree of approximation. To find it, we must again set off in a different direction from the beginning. We have to concern ourselves with the asymptotic form of the optimal policy as $t \longrightarrow \infty$. This will provide us with the means to calculate optimal strategies, and the results will be presented at the end of the chapter.

- 5 -

The topic of this section is the behaviour of the optimal path of development as the time from its inception becomes very large. It appears that only academic curiosity could lead us to study such a question. However, besides providing an interesting commentary on some recent work *, the discussion will also provide some further help in the calculation of the optimal strategy.

The instrument of our enquiry is the ordinary differential equation (26):

$$(1-(1-b)\sigma\xi)\xi\frac{d\sigma}{d\xi} = (1-\sigma)(\gamma + b\xi(\sigma - \frac{1}{n+1})). \qquad (26)$$

Here σ is the ratio of investment to total output; and $\xi = \frac{a}{\alpha}k^{b-1}$ is the reciprocal of the capital-output ratio multiplied by α. γ is $1 + \frac{r/\alpha}{n+1}$, which is 1 when $r = 0$.

The link with time - t does not occur in (26) - is provided by equation (55):

$$\frac{d\xi}{dt} = \alpha(\xi - (1-b)\xi^2\sigma(\xi)). \; ^{**} \qquad (55)$$

It will be remembered that the optimal strategy is provided by the positive solution of (26) that renders $\int_k^\infty [f(x)(1-\sigma)]^{-n-1}dx$ a minimum. We were in any case able to identify this solution by the property that σ tends to $1/(n+1)$ as ξ tends to ∞. As a first step now, let us portray all the solutions of (26) on a "phase diagram", diagram V.1. As we shall certainly be greatly concerned with the behaviour of solutions when ξ is large, it is best to make a transformation to $x = 1/\xi$. If we calculate the time derivatives of σ and x, we get a pair of differential equations:

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* The literature on turnpike theorems, paths of efficient accumulation, and the "neo-neo-classical theorem" was mentioned in chapter II § 5.

** Cf. p. V - 15 above.)

Opposite p. V-21

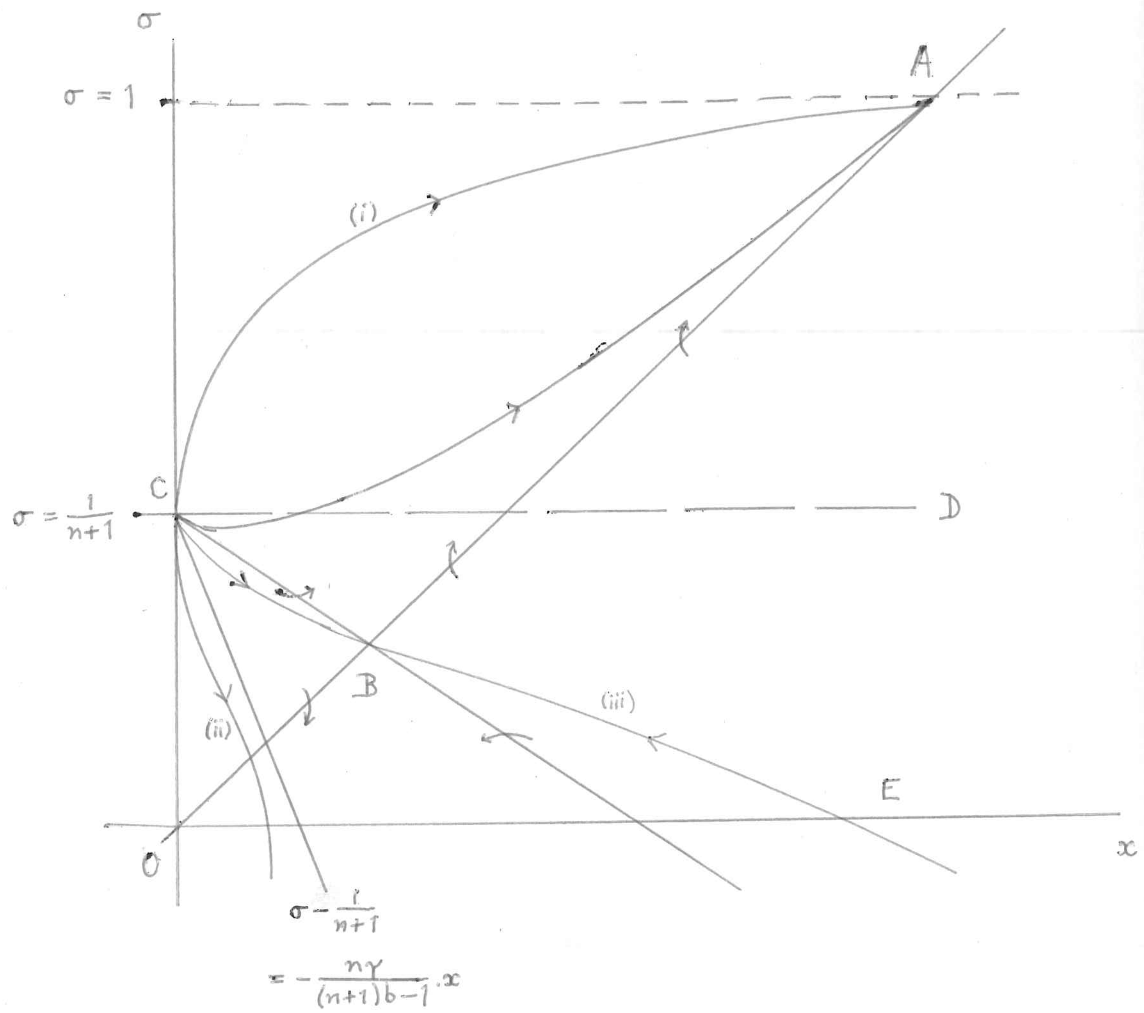

diagram 5.1

$$\left.\begin{aligned} \frac{dx}{dt} &= \alpha((1-b)\sigma - x) \\ \frac{d\sigma}{dt} &= \alpha(1-\sigma)\left(\gamma + \frac{b}{x}\left(\sigma - \frac{1}{n+1}\right)\right) \end{aligned}\right\} * \qquad (59)$$

If we plot solutions (trajectories) of these equations on a diagram with x on the horizontal axis, and σ on the vertical axis, we shall also have graphs of solutions of (26) in the form $\sigma(x)$.

The singularities of equations (56) are the points where both time-derivatives vanish. The first vanishes when

$$x = (1-b)\sigma \ ;$$

the second vanishes when

$$\sigma = 1 \qquad \underline{\text{or}} \qquad \gamma x + b = \frac{b}{n+1} \ .$$

Hence the singularities are $(1-b, 1)$ and $\left(\left[(n+1)\left(\frac{\gamma}{b}+\frac{1}{1-b}\right)\right]^{-1}, \left[(n+1)\left(\gamma\frac{1-b}{b}+1\right)\right]^{-1}\right)$.
These are plotted as A and B on the diagram. $\dot{x} = 0$ on the line AB , while $\dot{\sigma} = 0$ on the line BC , where C is the point $(0, 1/(n+1))$. $\dot{\sigma} > 0$ to the North-east of BC and below the line $\sigma = 1$.

The point C requires special attention. It is a third singularity, in that an infinity of trajectories can pass through it. It is the only point on the σ- axis between 0 and 1 at which $d\sigma/dt$ can be finite. The limit at C of $\frac{1}{x}\left(\sigma - \frac{1}{n+1}\right)$ is $d\sigma/dx$, on any trajectory through C . So, dividing the second equation (59) by the first, we have at C ,

$$\frac{d\sigma}{dx} = \frac{n}{n+1}\left(\gamma + b\frac{d\sigma}{dx}\right) \Big/ \left(\frac{1-b}{n+1}\right)$$

Hence

* Pairs of non-linear equations have been discussed by many authors; but I have not found a treatment of equations of this form. The method of analysis has been influenced by a lecture course on non-linear differential equations given at Cambridge in 1961 by Mr. H.P. Swinnerton-Dyer.

$$\frac{d\sigma}{dx} = \infty, \ -\infty, \text{ or } -\frac{n\gamma}{(n+1)b - 1} .$$

Denote the line $\sigma = \frac{1}{n+1}$, $x \geq 0$ by CD .

It is easy to verify that no trajectories go straight into the angle BCD . When $(n+1)b > 1$, the third value of the derivative corresponds to trajectories from C that start off below CB . If $(n+1)b < 1$, the corresponding trajectories begin above CD .

The next point to be established is how the trajectories cross the lines AO and CB . The directions are shown in the diagram, and are easily verified: they cross CB rightwards, CB produced leftwards, OB downwards, and BA upwards. It is particularly to be noticed that trajectories that cross the lines below B will subsequently cross the x-axis: σ can never again become positive. Hence such trajectories are disqualified, for investment must ultimately remain positive on the optimal trajectory. Indeed trajectories must either end at B or at A: they cannot cross the line $\sigma = 1$, and they cannot go rightwards to infinity, since $\dot{x} < 0$ to the right of OA . Clearly the optimal trajectory cannot end at A , since $\sigma = 1$ cannot be an optimal policy; in any case this is obvious from the following argument.

When $x = 0$, the trajectory is at C . A trajectory beginning at C can be of three kinds:

(i) It may begin by rising vertically $(d\sigma/dx = \infty)$; in which case it must turn to the right and end at A .

(ii) It may move downwards $(d\sigma/dx = -\infty)$, and then cross CB, OB, or end at B. If it crosses CB it must end at A ; if it crosses OB , it will disappear into negative regions. When $(n+1)b > 1$, we can show that it will cross OB . Consider the line $\sigma = \frac{1}{n+1} - \frac{n\gamma}{(n+1).b-1}\, x$, which passes through C and cuts OB . On it

$$\frac{d\sigma}{dx} = -\frac{(b-1)(1-\sigma)\gamma}{((1-b)\sigma - x)((n+1)b-1)} < -\frac{(n/(n+1))(1-b)\gamma}{(1-b)(1/(n+1))}\cdot\frac{b}{(n+1)b-1}$$

$$= -n\gamma/((n+1)b-1) .$$

The inequality follows because $\sigma < 1/(n+1)$ and $x > 0$. Hence trajectories can only cross CB downwards: trajectories of type (ii) cannot cross it at all.

(iii) It may begin with $d\sigma/dx = -n\gamma/((n+1)b-1)$. If $(n+1)b - 1 < 0$, it remains above CD and ends at A . If $(n+1)b > 1$, it may cross CB , OB , or end at B . In this latter case, one trajectory must end at B ; for the trajectories crossing CB and those crossing OB form the two parts of a Dedekind cut, which must define a dividing trajectory.

If $(n+1)b < 1$ one of the trajectories of type (ii) must end at B , by the same argument.

This completes the picture to the left of OA . On the right, trajectories move leftwards. $\sigma = 1$ is itself a trajectory. All the others come from below the x-axis, for $d\sigma/dx$ does not tend to zero as x tends to infinity unless $\sigma = 1$. Again we apply the standard Dedekind cut argument to show that one of them must end at B , all those cross BA being above it.

These two trajectories ending at B form a continuous curve, which is lower than all other elegible curves. Hence it must represent the optimal strategy, for $\int v'dk$ is at least on it. The trajectory crosses the x-axis at some point E . When x is greater than x_E, the economy should decumulate capital. If for some reason the economy cannot do so, then investment should be held at zero until the "natural" growth of output reduces x to the point where investment is to begin. The direction in which x changes depends on the branch of the curve on which the economy finds itself. If $x < 1/((n+1)(\frac{\gamma}{b} + \frac{1}{1-b}))$, it should increase over time, and σ decrease: in the contrary case, x and σ move the other way.

I have shown that

$$\sigma \longrightarrow \frac{b}{(n+1)(\gamma(1-b)+b)}$$

$$x \longrightarrow \frac{b(1-b)}{(n+1)(\gamma(1-b)+b)} \qquad (60)$$

as $t \longrightarrow \infty$. When $r = 0$, and consequently $\gamma = 1$, the asymptotic values are $b/(n+1)$ and $b(1-b)/(n+1)$, respectively. Let us denote the asymptotic investment ratio by S.

We would like to be able to calculate the point at which the curve cuts the x-axis - as a matter of curiosity: is it a possible state of the economy or not? It can only be estimated numerically, no simple formula being available. It would also be interesting to estimate the form of the curve near $x = 0$ when $(n+1)b \leqq 1$, as we can do when $(n+1)b > 1$. But all that local methods tell us is that $\sigma - 1/(n+1) \sim Ax^{nb/(1-b)}$, where A is some constant, when $(n+1)b < 1$. Which constant can only be determined when we know which of the curves from C passes through B - which is a much more difficult task by the present methods. However the series expansion (58) for the improved first approximation gives us the value of the constant (and may be valid for another term, although I have no means of checking it.) We have

$$\sigma \sim \frac{1}{n+1} - n\left[r+(n+1)\alpha\right]\left(\frac{n+1}{1-b}x\right)^{\frac{nb}{1-b}} \sum_{0}^{\infty} \frac{B(B+1)\ldots(B+m-1)}{m!}\left[\frac{r}{\alpha}+\frac{n}{1-b}+m\right]^{-1} \qquad (61)$$

It is now apparent why the condition $(n+1)b > 1$ played the part it did in finding the first approximation.

\- 6 -

The maximum consumption theorem for a growing economy, which has aroused considerable interest in recent years, states that when population is growing, but there is no technical progress, and when there are increasing returns to scale (the factors of production being capital and labour), the maximum sustained level of consumption per head is attained when the investment-output ratio is b, the elasticity of capital (this is often expressed picturesquely by saying that the rate of profit equals the rate of growth, or that all the earnings of capital are devoted to investment; it can even be derived without mentioning that dread word "capital".)

On the whole, I am avoiding questions of population changes in this thesis, largely because the popular and easy assumption of an exponentially growing population strikes me as neither realistic in the long run, nor desirable. Since, however, the maximum consumption theorem is concerned only with economies where the population is growing at a positive and constant exponential rate, I can scarcely compare it with my own results unless I allow population growth to rear its head, if only temporarily.

Let us assume a production function

$$y_t = ak_t^b N_t^c e^{\alpha t} \qquad (62)\ *$$

The labour force (let me not enter into questions of how to measure

* I hope that the exponent c will not be confused with my previous use of c to represent consumption.

it) is $N_t = N_o e^{\nu t}$.

The proper form for the valuation function is a real difficulty. To my knowledge only Professor Meade has discussed this question explicitly ((Meade 1955)), although many of the considerations have been implicit in discussions of optimum population for decades, and anyone who thinks seriously about the ethics of population control must face these issues. Let us ignore a number of questions and assume that the instantaneous valuation function should be of the form $v(C_t, N_t)$, where C is total consumption, and N is supposed to represent the size of the population as well as the size of the labour force. The particular form of $V(C,N)$ is a question of exactly the same kind as we were faced with in having to choose the form of $v(c)$ previously. It is rather natural to hold that both consumption per head and the number of heads deserve some weight. In the present context, those who say that the number of heads should be irrelevant are saying that larger generations deserve no more than small ones, a view that I find repugnant. I want to say only a little about the pure theory of the asymptotic form of the optimum, so I shall not enter into these very important questions, but content myself with writing down a valuation function that we can use to elucidate some of the consequences of different views on the equation. Let us put

$$v(C,N) = v(C/N)q(N). \qquad (63)$$

Professor Meade espouses the particular form $q(N) = N$, and on the whole I find this form the most satisfactory (but with many reservations). The consumption-per-headers(1) keep q constant. Keeping to the special

(1) This naive view is usually used implicitly by propagandists for population control: The best causes often have to depend upon the worst arguments.

case, I put $v(C/N) = -(C/N)^{-n}$.

When $q(N) = N$, the total valuation is

$$V = -\int_0^\infty C_t^{-n} N_t^{n+1} e^{-rt} dt = -N_0^{n+1} \int C_t^{-n} e^{-(r - (n+1)\nu)t} dt .$$

The discount rate has become $r - (n+1)\nu$. The production function has changed too. Now

$$Y_t = aN_0^c e^{(\alpha + c\nu)t}$$

The "rate of technical progress" has become $\alpha + c\nu$. The only difference when q is a constant, instead of proportional to N , is that the discount rate is $r - n\nu$. If we want to generalize a little, we can think of the discount rate being between $r - (n+1)\nu$ and $r - n\nu$. It certainly looks as if the difference between the two valuation treatments of population is very small.

Before applying our results to this new case, we would do well to see what difference a negative discount rate might make to our result, for it is clear that exponential population growth would in general lead us to posit a negative rate of valuation-discount. The results proved above depended on the point B being in the positive quadrant below the line $\sigma = 1$. That is, the lines $(1 - b)\sigma = x$ and $b\sigma + \gamma x = b/(n+1)$ must meet in the region $x > 0$, $0 < \sigma < 1$. This happens if and only if

$$\gamma > -\frac{n}{n+1} \cdot \frac{b}{1-b}$$

that is, making the substitution for the new "r" and "α" ,

$$(1 - c + n \cdot \frac{1 - b - c}{1 - b})\nu < r + (1 + \frac{n}{1 - b})\alpha \quad , \qquad (64)$$

when the valuation is proportional to N . When q is constant, the condition is

$$\left(-c + n\,\frac{1-b-c}{1-b}\right)\nu < r + \left(1 + \frac{n}{1-b}\right)\alpha \quad . \tag{65}$$

Clearly (65) is always satisfied if r and α are positive, and we do not have decreasing returns to scale (which on the whole would be a surprising assumption). (64), however, may fail to hold. In such a case no optimum policy exists, for no solution to the ordinary differential equation can be an optimum policy: the only trajectory from C that does not eventually become negative (implying eternal decumulation of capital) goes to A , a point on the line $\sigma = 1$, and so certainly is not optimal. The explanation of this is relatively simple. If we assume an exponential growth of population, we may not be able to ensure that consumption per head tends to infinity. When that happens distant generations, being enormous in each year, and covering an infinite number of years, are so deserving of our charity in the form of present investment that any amount of investment is justified.(1) As I have said I do not regard exponentially growing populations with any favour, so that the fact does not worry me: it points out, however, that with a sensible valuation function, constant returns to scale, no technical progress and no pure time preference, the maximum consumption path referred to in the maximum consumption theorem is not optimal. Only with the naive and unreasonable valuation function $v(C,N) = v(C/N)$ does the optimal path tend in fact to the situation portrayed by the maximum consumption theorem.

Let us now restrict ourselves to cases for which the inequality (64) holds. Then a simple calculation shows that

$$(n+1)S = \frac{b(\alpha + c\nu)}{\alpha + (b+c-1)\nu + [(1-b)/(n+1)]r} \tag{66}$$

---

(1) This was the situation facing Tinbergen (Tinbergen 1960)), although his trouble was a disobliging valuation function.

When there was no population growth, S was less than $1/(n+1)$, but now, of course, it may well be greater. When there are constant returns to scale and $r = 0$,

$$S = \frac{1}{n+1}\left(b + (1-b)\frac{\nu}{\alpha}\right) ,$$

which is precisely $1/(n+1)$ when $\nu = \alpha$. Indeed when $S = 1/(n+1)$, $\sigma = 1/(n+1)$ for all x . This is easily shown by the same arguments that were used to establish the shape of the optimal path when $S < 1/(n+1)$. When $S > 1/(n+1)$, σ is always greater than $1/(n+1)$, and tends to 1 as x tends to infinity. It follows that the valuation is infinite when $S \geqq 1/(n+1)$, but it will be remembered that the validity of our methods is not affected.

When the valuation function is simply $-(C/N)^{-n}$, we have

$$(n+1)S = \frac{b(\alpha + c\nu)}{\alpha + [c - (1-b)n/(n+1)]\nu + [(1-b)/(n+1)]r} \qquad (67)$$

The smaller ν is in relation to α , the less is the difference between (66) and (67), but the difference is not likely to be negligible unless n is rather large.

- 7 -

Were it not for one circumstance, the study of the asymptotic investment-output ratio would be of no practical interest. This useful feature is that it tells us the value of the investment ratio for a value of x other than zero, namely at the asymptotic point, B . So long as we only know the solution at $x = 0$, we cannot obtain the solution of the differential equation (26) that we seek in the form of a convergent series in x , for the point $x = 0$ is not a regular point of the equation. However the point

$x = (1-b)S$, $\sigma = S$ has only two solutions passing through it, and we are consequently able to solve the equation in the neighbourhood of this point in the form of a convergent series. This fact really enables us to calculate any solution we want as accurately as we wish. I shall end this chapter by calculating a few terms of this expansion, and showing how it can be used to discover the optimum rate of investment.

We want a series solution of equation (26) in the form

$$\sigma = S + S_1(x - (1-b)S) + S_2(x - (1-b)S)^2 + \ldots \qquad (68)$$

This has to be inserted in the equation, which we can most conveniently write in the form:

$$((1-b)\sigma - x)\frac{d\sigma}{dx}.x = (1-\sigma)\left[\gamma x + b\left(\sigma - \frac{1}{n+1}\right)\right] .$$

We equate the coefficients of powers of $x - (1-b)S$ on the two sides of the equation. Equating the coefficients of $x - (1-b)S$ yields a quadratic equation for S_1 :

$$((1-b)S_1)^2 - \left(\frac{b(1-S)}{(1-b)S} + 1\right)((1-b)S_1) - \gamma\frac{1-S}{S} = 0 .$$

Of the two roots, only the negative one is what is wanted: the other corresponds to a path going from B up to A in the diagram. All the other S_i follow uniquely from this one. S_2 is given by:

$$S_2\left[2 + \frac{b(1-S)}{(1-b)S} - 3(1-b)S_1\right] = \frac{(S_1 + \gamma - 1)S_1}{(1-b)S}$$

S_i is found from S_{i-1} by means of the formula:

$$\left(S_i + \frac{1}{(1-b)S}S_{i-1}\right)\left[n + \frac{b(1-S)}{(1-b)S} - (n+1)(1-b)S_1\right]$$

$$\cancel{= \frac{1}{(1-b)S}\left[1 - (1-b)S_1 + \frac{b(1-S)}{(1-b)S}\right)S_{i-1} + \frac{b}{(1-b)S}B_i + (1-b)A_i + \frac{1}{S}A_{i-1}} ,$$

$$= \frac{1}{(1-b)S}\left(1 + \gamma + \frac{b(1-S)}{(1-b)S} - (1-3b)S_1\right)S_{i-1} + \left(\frac{(n+1)(1-b)}{2} + \frac{b}{(1-b)S}\right)A_i + \frac{1}{2S}A_{i-1} ,$$

where

$$A_i = S_2 S_{i-1} + S_3 S_{i-2} + \ldots + S_{n-1} S_2 \quad (i > 2)$$
$$= 0 \quad (i = 2).$$

These formulae enable calculations to be done quite readily. In most cases, six terms of the series are quite sufficient to calculate the optimum investment ratio to the nearest percentage point, even for very small values of x.

\- 8 -

The method of the previous section has been used to prepare graphs of $\sigma(x)$ for all reasonable values of x and for representative values of n and b. Before presenting these results, it is worth saying a few words about what interest they might be expected to have.

In recent years, there has been a certain amount of discussion among economists as to the importance of capital in economic development. As I have already indicated, I do not suppose for a moment that all aspects of the importance of capital are contained in the neo-classical production function. Of the way in which the "rate of technical progress" itself might be supposed to depend on capital investment, I shall have a little to say later. Meanwhile, the point of interest is to see what effect on policy different views about the relative importance of investment-dependent growth of output and investment-independent growth, <u>as represented by the</u>

simple form of the production function we have been using, should have.

The work of Solow, Aukrust,(1) and others, who have used as their working assumptions such time-honoured, but also time-worn, assumptions as constant returns to scale and the marginal productivity theory of factor rewards, has produced values of b often not much greater than 1/4 . Shorn of some econometric sophistication, the idea is that - on the assumptions used - b is the share of the national product accruing to capital, and that is usually found to be from one half to one fifth. Accepting this value of B , it would follow that the equilibrium rate of growth of the economy's output would be $\alpha/(1 - b)$. (Here I am neglecting entirely all changes in the labour force, which of course play quite a large part in these econometric discussions.) Observation of the rate of growth over a longish period of more or less undisturbed growth, if this can be found, suffices to estimate α as well as b .

Let us denote this observed rate of growth of output in the economy by α^* . We are really interested in the potential of the economy, so that it is not unreasonable for the optimistic among us to accept a value of α^* somewhat larger than has been sustained by the economy over long periods in the past. Most of us would feel that the business cycle and the government policy cycles have some cost in terms of growth. α^* is, at any rate, some sort of observable: a stylised , and somewhat interpreted, fact. Again, it is not entirely unreasonable to accept the neo-classical production function as an interim method of prediction. But the knowledge that increasing returns to scale are rather widespread, and that investment has a great deal of indirect effect on growth, besides a suspicion about the usefulness of assumptions of perfect competition in

(1) (Solow 1957), (Aukrust 1959).

OPPOSITE p. V – 33

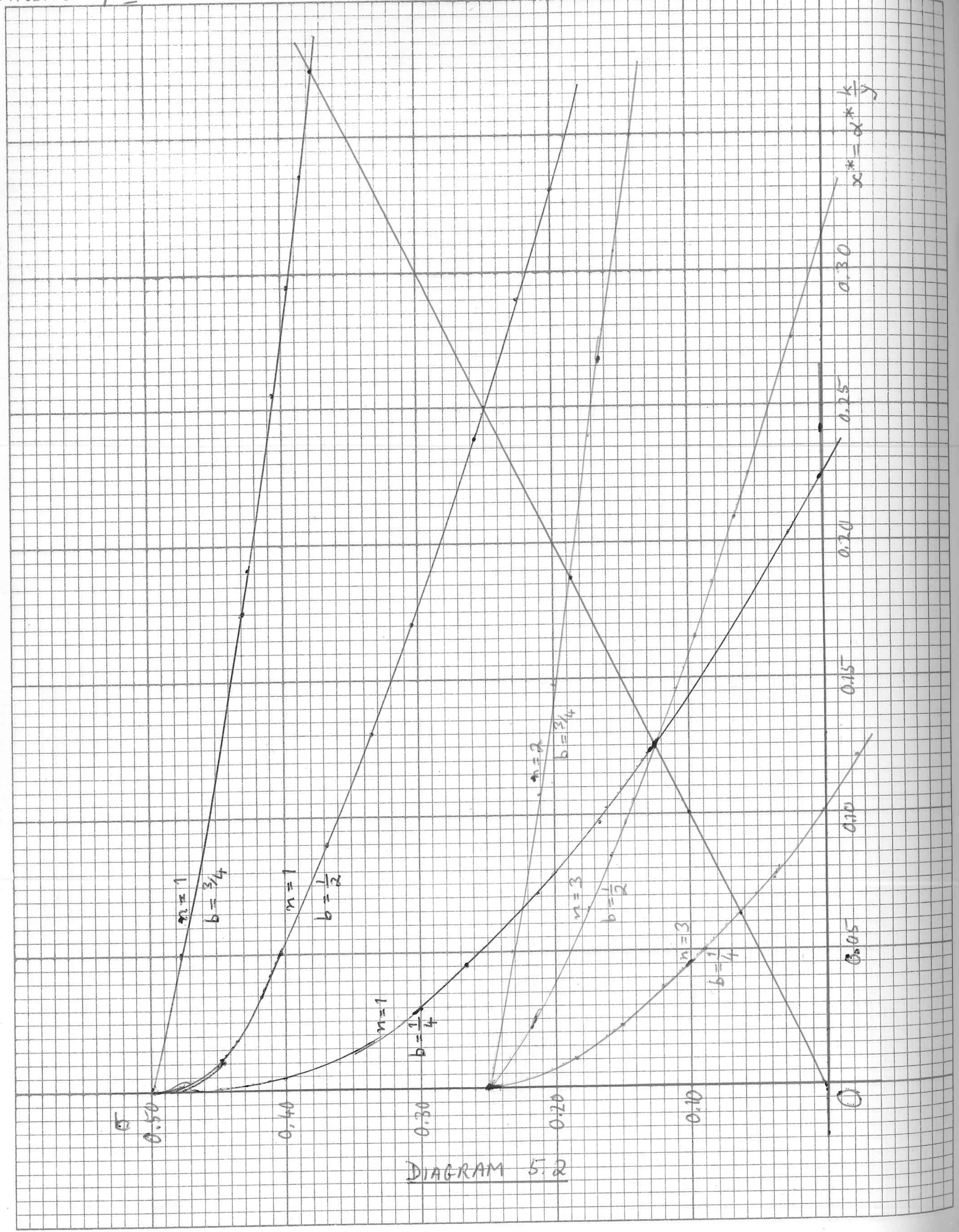

DIAGRAM 5.2

such a context as this (where, for instance, half of gross investment may be in the public sector), combine to suggest that one may well choose a larger value of b without flying in the face of the evidence. Macroeconomic evidence from the past, in the form of time-series, is really no guide: fluctuations have not been the result of guided full-employment acceleration and deceleration of growth. If in the future we can look back on such a period, we could no doubt estimate b . At any rate it is worth seeing what difference different values of b should make to our policy decisions.

The constant in all this is the observable, α^* . We want to know the consequence of changing α and b while keeping α^* constant. In the attached graphs r is zero , the values of σ have been shown against the values of $x^* = \alpha^* \frac{k}{y}$. The graphs give the value of the optimum investment ratio for n = 1 and 3 , and for b = 1/4, 1/2, and 3/4 . It may be assumed that these graphs give the investment ratio accurately to the nearest one per cent at least. A number of features of the results call for comment :-

(1) The optimum investment ratio is highly sensitive to variations in b . If we think of α^* as lying between 2% and 6%, and k/y as lying between 2 and 5, the interesting values of x^* are from 0.04 to 0.30. Even at the narrowest end of the range, when n = 3 , the optimum investment ratio for b = 1/4 is less than half the optimum investment ratio for b = 3/4 . Thus the most striking feature of Ramsey's result for the static case not only ceases to hold in general; but in fact the particular form of the production function is crucial. If, for example, α^* = 4% and k/y = 4 , we find that, if n = 1;

when $b = 1/4$, $\sigma = 7.5\%$

when $b = 1/2$, $\sigma = 31\%$

when $b = 3/4$, $\sigma = 43.5\%$

In view of our very imperfect knowledge of b , this must be regarded as unfortunate, and a due warning against rashly assuming that we know roughly what the optimum rate of investment is. Clearly it is true that investment should be much less if it can be shown that investment-independent forces are the "main engines of growth"; a proposition that has been too often assumed without proof.

From the curves given, it is possible to interpolate with reasonable accuracy for other values of b . We know that σ is $b/(n+1)$ when x^* is $b/(n+1)$ (since $r = 0$) , and we can read from the graphs the values of σ for this value of x^* when b is 1/4 , 1/2 , and 3/4 ; call them σ_1, σ_2, and σ_3 . If b lies between 1/4 and 1/2 , say, we can use the ratio of $\frac{b}{n+1} - \sigma_1$ to $\sigma_2 - \frac{b}{n+1}$ to determine the required value of σ at some other x^* . This is accurate enough for most purposes.

If, for example, b for the U.K. is estimated to be 0.4 , the capital-output ratio is 4 , and the maximum long-run rate of growth is 4% , we can find the optimum investment ratio as follows (1):

The investment ratio ought to tend ultimately to $\frac{0.4}{n+1}$. Consider first $n = 1$. Then $S = 0.2$. At $x^* = 0.2$,

$$\sigma = 0.28 \text{ if } b = 1/2 \;;$$

$$= 0.025 \text{ if } b = 1/4 \;.$$

---

(1) Need one remark that it is optimum only relative to a very imperfect model? It may be the best available for all that.

We estimate the investment ratio at $x^* = 0.16$ as:

$$\sigma(0.16) = \frac{(0.28-0.20)\ (0.16:\ b=1/4) + (0.20-0.025)\ (0.16:\ b=1/2)}{0.28 - 0.025}$$

$$= \frac{0.08 \times 0.075 + 0.175 \times 0.31}{0.255}$$

$$= 0.24$$

If $n = 3$, it turns out that $\sigma = 0.045$

These results can be compared with the corresponding values, 0.31 and 0.10 when b is only 0.1 larger.

(2) The optimum investment ratio is also very sensitive to the value of n. In other words, our values in regard to material equality have a considerable influence on the right rate of investment. Again, this is distressing, since it is difficult enough to determine in one's own mind the proper value for n, while the prospect of agreement sufficient to determine the optimum rate of investment within reasonable limits seems remote.

Interpolation by the method already suggested for interpolating b works very well in this case. If the curves are examined closely, it will be seen that curves for the same b but different n are practically parallel, except for very small values of x^* (say, less than 0.04.) For example, it can be calculated that when $b = 0.5$ $\alpha^* = 4\%$, $k/y = 4$, and $n = 2$, the optimum investment ratio is 0.17. S in this case is equal to 1/6: if these parameters described the actual situation, the economy would be rather near the long-run optimum steady growth state.

(3) The optimum investment ratio for the static case is seen to be a very bad guide to the situation in the dynamic case, except for very large values of b (that is, close to 1). I find this surprising, for,

as I have earlier remarked, realistic values of α <u>seem</u> to be rather small. Clearly this is not true in the relevant sense. The results are a warning against too facile conclusions when $\beta \neq 0$, for when there is uncertainty, a second derivative appears in the equations, and who knows what that might do? Ramsey's results worried economists because they seemed to recommend rates of investment that were implausibly large. These more general results may worry some because they can recommend rates of investment that are surprisingly low. For example, consider the "liberal syndrome" of a low value for b (because capital is not very important), and a low value for n (because that is "what people want"). Taking $b = 1/4$ and $n = 1$, the investment ratio is 8.5% when $x^* = 0.16$. Furthermore, the direction of development is such as to reduce the capital-output ratio towards the value 3.1 (assuming an equilibrium growth rate of 4%.) If higher values of n are assumed, recommendations may become very odd: it is apparent from the lowest curve that a recommended rate of investment less than zero is not entirely implausible.

(4) It is possible to express the recommended rate of investment a little more picturesquely by translating it into a rate of growth. The derivative of output, y, with respect to time is:

$$\dot{y} = b \frac{y}{k} \dot{k} + \alpha y.$$

Hence

$$\frac{\dot{y}}{y} = b \frac{y}{k} \sigma + \alpha$$

Consider, by way of example, $n = 1$, $b = 1/2$, $\alpha^* = 4\%$, $k/y = 4$. σ is 31%. Hence the optimum rate of growth of output is

$$\dot{y}/y = 6\% ,$$

which is two percent greater than the equilibrium rate of growth. This emphasises, what is in any case obvious, that it is not optimal for a country to increase its rate of growth to the equilibrium level, although it may well be a step in the right direction. The equilibrium rate of growth is only the optimum rate of growth when the process of capital-deepening has gone far enough for the capital-output ratio to have attained its equilibrium level too.

(5) I have not calculated curves for non-zero r, because I do not think it is a case of much interest. If it is desired to get an impression of the effect of non-zero r, S can be calculated, and compared with its value for $r = 0$. For example, take $n = 2$, $b = 1/2$. When $r = 0$, $S = 0.25$. When $r = 2\%$, $\alpha = 2\%$, we find that $S = 0.20$. The difference for values of x^* less than 0.25 will be less than 0.05.

Some people might want to discount future valuations at a rate of 5% and more. I have given my reasons for thinking this wrong in chapter II. Naturally the methods of calculation expounded in the previous section can be used just as easily when $r \neq 0$.

When we turn to uncertainty, as we must now do, we must leave the accurate and complete methods to which we have now come. I cannot give any similar method for the more general case. We shall see that there is some reason to believe that the lack of such a method matters less than it would in the no-uncertainty case. It may be remarked finally that the method of finding the asymptotic policy just used could presumably be applied to a wider range of cases than the constant-elasticity cases, although the fact that we cannot derive an ordinary differential equation would add to our difficulties. The nature of the asymptotic behaviour is likely to be so diverse that a general treatment seems unlikely to be very useful.

CHAPTER VI UNCERTAINTY

\- 1 -

The general ideas of the preceding section can be used to find out about uncertainty. For the present, production uncertainty is represented for us by the parameter β . We must consider the magnitude of β later, but let us first establish some information about the optimal policy for general, but small, values of β . Just as when there was no uncertainty, I shall first discuss approximations for small values of the parameters, and then turn to the general features of the solution in the special constant-elasticities case. Having considered some empirical evidence about the value of β , we shall have to examine the extent to which the simple model portrays the important features of uncertainty in the world of actual decisions. In particular, present uncertainty about production possibilities will be introduced into the model, and its importance assessed. The general question that I want to throw light upon is when, if ever, it is worth paying explicit attention to uncertainty in planning models, and what effect it should have on our decisions. Naturally we cannot give a complete discussion of this very important question in what is a rather limited macroeconomic context. However it seems essential to begin a systematic enquiry into what is an unjustly neglected topic, and right to begin it with a discussion of the simplest and most representative case.

At the end of section 2 I derived the first approximation, which gives the optimal policy up to the first order in the parameters. It is given in equation (28). If the second derivative of the integral $\int v' dx$

with respect to a is worked out more explicitly, the expression involves the second derivative of θ, the function that provides the optimal policy for the static case. At the beginning of section 3, I calculated the first derivative, θ'; the equation (31) can be differentiated again to obtain an expression for the second derivative. When this is used, it is found that the first approximation is:

$$s(k,a) = \theta(y) - \frac{1-\theta'(y)}{v'(y-\theta)} \int_k^{\infty} \left[r + \alpha \frac{af(x)}{\theta(af(x))} - \beta \left(\frac{af(x)}{\theta}\right)^2 (1+\theta'(x))\right] \times v'(af(x) - \theta(af(x))).dx. \qquad (69)^{(1)}$$

When the first approximation is written in this way, it is clear that the β - part of it is positive, so that the first perturbation is positive when r and α are zero.

It will be remembered that the expression (69) only gives a valid first approximation (at any rate for small enough β) if f and v are such that the valuation integral converges for small enough β when r and α are zero.

It can already be seen from the expression (69) that the first perturbation is small when θ' is close to 1, that is, when the elasticity of the marginal valuation is small. I have already argued that the elasticity of the marginal valuation represents the degree of egalitarianism. Thus the first perturbation is small at levels of output for which the egalitarian bias is small: that is, to my way of thinking, high levels of output, if any. In other words, the static policy is more nearly correct at high levels of output, if one believes that equality

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(1) As earlier, y denotes output, $af(k)$.

matters little when affluence is general.

It can also be seen that the effect of β is relatively greater when Θ/y is small. (I am speaking roughly, but only a rough argument is possible in the general case.) Thus uncertainty is likely to be a particularly important consideration when the investment policy required by static conditions is conservative.

For reasons that must now be apparent, conclusions are sharper in the particularly interesting case $v(c) = -c^{-n}$. Then $\Theta(y) = y/(n+1)$. The first approximation is:

$$s(k,a) = \frac{y}{n+1} - \frac{n}{n+1}\left(r + (n+1)\alpha - (n+1)(n+2)\beta\right)(f(k))^{n+1}\int_k^\infty (f(x))^{-n-1}\,dx. \tag{70}$$

Obviously that linear expression in the dynamic parameters deserves a symbol. In the last section, I used γ to denote $1 + \frac{r/\alpha}{n+1}$; now that $\beta \neq 0$, I shall define

$$\gamma = 1 + \frac{r/\alpha}{n+1} + (n+2)\beta/\alpha \tag{71}$$

This reduces to the previous definition when $\beta = 0$. (70) can be written:

$$s(k,a) = \frac{y}{n+1} - n\alpha\gamma . f^{n+1}\int_k^\infty (f(x))^{-n-1}dx.$$

We shall want to examine later to what degree of approximation we can say that the static policy is adequate when $\gamma = 0$. Meanwhile, it seems to be roughly true that production-uncertainty has $(n+2)$ times the effect on policy as the same percentage of technical progress. However α and β are not likely to be of the same magnitude, empirically, so that the actual relative importance of technical progress and uncertainty will have to be discussed later.

If we now put $f(k) = k^b$, we have - generalizing (34) - a first approximation:

$$s(k,a) = \frac{y}{n+1} - \frac{n\alpha\gamma}{b(n+1) - 1} k , \tag{72}$$

valid when $b(n+1) > 1$. The effect of uncertainty in modifying the policy that would be recommended if it were ignored would seem to be greater when b is smaller.

The second approximation, too, is derived in the same way as previously, Instead of (39) we have:

$$s_2 = -\frac{2(1 - \theta') - \theta\theta''}{20(1 - \theta')^2}(s_1)^2 + \frac{1 - \theta'}{v'(y - \theta)}\left(r - \alpha a \frac{\partial}{\partial a} - \beta a^2 \frac{\partial^2}{\partial a^2}\right) \int_k^{\infty} s_1 v'' . \; dx. \tag{73}$$

I shall not trouble to elaborate this formula by evaluating the derivatives explicitly, but rather illustrate the computation for the particular case $v(c) = -c^{-n}$. The result when $\beta = 0$ is given in equation (40). It would be tedious to go through the detailed computations, and I shall simply reproduce the result:

$$s_2 = n(n+1)\frac{(f(k))^{n+1}\alpha\gamma}{a}\Big\{(r + (n+2)\alpha - (n+2)(n+3)\beta)\int_k^{\infty}\int_x^{\infty}(f(\xi))^{-n-1}d\xi . \frac{dx}{f(x)}$$
$$- (r + (n+1)\alpha - (n+1)(n+2)\beta) . (f(k))^n\Big[\int_k^{\infty}(f(x))^{-n-1}dx\Big]^2\Big\} \tag{74}$$

When $f(k) = k^b$, the second perturbation - generalizing (41) - is:

$$s_2 = \frac{n(n+1)\alpha\gamma[(1 - b)r + n\alpha - (n+2)((n+1)b + (n - 1))\beta]}{(b(n+1) - 1)^2(b(n+2) - 2)} \frac{k^2}{y} \tag{75}$$

From (74) we see that the second perturbation, as well as the first, is zero when $\gamma = 0$. This is true of all the perturbations (but, as I remarked when considering the case $\beta = 0$, only a finite number of the perturbations are meaningful, as the integrals eventually diverge unless $b = 1$.)

Although we were able to obtain the optimal policy explicitly when

$b = 1$ in the previous section, it seems that it is no longer possible. It is therefore of particular interest to see how the second approximation looks in this case. Putting $b = 1$ in (72) and (75), we obtain:

$$s(k,a) = \frac{y}{n+1} - \alpha\gamma k + (n+1)\alpha\gamma(\alpha - 2(n+2)\beta)\frac{k^2}{y} + \ldots \tag{76}$$

When α is zero – the case when the capital-output ratio tends on average to remain constant, however fast or slow the investment – $\alpha\gamma$ must be replaced by $r/(n+1) - (n+2)\beta$, and we have

$$\frac{s}{y} = \frac{1}{n+1} - \frac{rk/y}{n+1} + (n+2)\beta\frac{k}{y} + 2(n+1)(n+2)^2\beta^2\left(\frac{k}{y}\right)^2 - 2(n+2)r\beta\left(\frac{k}{y}\right)^2 + \ldots \tag{77}$$

Certainly in this very special case, r would have to be rather a large multiple of β to counteract the effect of production uncertainty in augmenting the optimum rate of investment. We have still to see whether that is likely or not.

– 2 –

The ordinary approximation process meets difficulties no more serious than were met in the previous section: it has the advantages and disadvantages that the no-uncertainty case would lead us to expect. However, all is more difficult and obscure when we try to do better, either by seeking a better method of approximation, or by trying to utilise the ordinary differential equation to obtain information about the special cases. These further avenues must be explored nevertheless, since they are essential if we want to obtain a rounded picture of optimum policies.

Improved approximations were obtained by evaluating the total valuation for a known policy close to the optimum policy, namely the static policy.

In principle, precisely the same technique is open to us now, and it should have the same advantages. Unfortunately, it is now extremely difficult to calculate the valuation of a dynamic economy following the static policy, because it involves determining the expectation of a complicated operator on the stochastic process (ε_t). We do need to make some use of the method in order to obtain information about the optimal policy when the first method of approximation fails us, that is, when the static valuation integral diverges. I shall therefore outline the method, indicating that it is a possible technique, and illustrate it by applying it to the now familiar special case in which the valuation and production elasticities are constant.

The improved first approximation is given by the equation

$$v(c) + sv'(c) = -D\left\{E\left(\int_0^{\infty} v(af(k_t)e^{\alpha t+\varepsilon_t} - \theta(y_t))e^{-rt}dt\right)\right\} \tag{78}$$

where y_t is the output at t, viz. $af(k_t)e^{\alpha t+\varepsilon_t}$, and $\dot{k}_t = \theta(y_t)$.

ε_t, let us remind ourselves, forms, as t varies, a stochastic process having independent increments normally distributed with mean per unit time $-\beta$ and variance per unit time 2β. This means that $\varepsilon_t - \varepsilon_\tau$ is independent of ε_τ when $t > \tau$. The stochastic process k_t is then determined by the differential equation $\dot{k}_t = \theta\ (af(k_t)e^{\alpha t + \varepsilon_t})$, which, sad to say, cannot be solved in general; but it can be solved when, for instance, θ is a multiple of a power of y. Even when it cannot be solved, the solution can be approximated in various fruitful ways, but we should not gain much by following that avenue here.

There are two really awkward problems in using (78). The determination of the capital stochastic process, I have just mentioned: the other is to evaluate the expectation. It seems to be impossible in all cases to evaluate it exactly, but that is not in itself a serious drawback, especially as the solution of (78) is only an approximation. What has to be done is to expand the integrand (after applying the operator D) in such a way that each term of the expansion contains integrals, the integrands of which have factors like e^{ε_t} only in the numerator - that is, in positive (whole number) powers: for example, one term in the expansion might consist of a non-stochastic factor multiplying

$$e^{n\varepsilon_t}\left(\int_0^t e^{\alpha\tau+\varepsilon_\tau}\,d\tau\right)^2.$$

The expectation of such a term can be found with application, by making use of the fact that the random variables $\varepsilon_t - \varepsilon_\tau$ and ε_τ are independent. All this does not make for simple expressions for the right hand side of (78). The method will be seen more clearly in the following illustration.

Assume now that $v(c) = -c^{-n}$ and $f(k) = k^b$. We know from the last section that $v(c) + sv'(c) = (n+1)(Z^{n+1} - Z^n)\left(\frac{n+1}{ny}\right)^n$, where Z is defined as $\frac{n}{(n+1)(1-\sigma)}$. $\theta(y)$ is $y/(n+1)$. Thus (78) becomes:

$$Z^{n+1} - Z^n = \frac{a^n k^{nb}}{n+1} D\left\{E\left(\int_0^\infty a^{-n} e^{-(r+n\alpha)t - n\varepsilon_t} k_t^{-nb}\,dt\right)\right\}.$$

The differential equation for k_t is

$$\frac{dk_t}{dt} = \frac{a}{n+1} k_t^b e^{\alpha t+\varepsilon_t}.$$

We can solve this. $\int_0^t e^{\alpha\tau+\varepsilon_\tau}\,d\tau$ is a well-defined random variable for each t. Hence

$$k_t = \left[k^{1-b} + \frac{a(1-b)}{n+1}\int_0^t e^{\alpha\tau+\varepsilon_\tau}\,d\tau\right]^{\frac{1}{1-b}} \qquad (79)$$

is a well-defined random variable, and it also satisfies the differential equation and is equal to k when $t = 0$. Thus (79) gives capital for each t . Writing, as we did before, Λ for $\dfrac{a(1-b)k^{b-1}}{\alpha(n+1)}$

we have

$$Z^{n+1} - Z^n = \frac{a^n}{n+1} E\left\{ D\left(\int_0^\infty a^{-n} e^{-(r+n\alpha)t - n\varepsilon_t} \left[1 + \alpha\Lambda \int_0^t e^{\alpha\tau + \varepsilon_\tau} d\tau\right]^{-B} dt\right)\right\}. \quad (80)$$

B is $nb/(1-b)$.

We can apply the operator E before applying D . We cannot hope to obtain a finite expression for the integral, so what we must do is estimate its value when β is small. In other words, we must attempt to expand the integral in powers of β : more precisely, in powers of β/α . There is no reason, in principle, why one should not push this expansion as far as one wants, but it is a very complicated process. I shall content myself with showing how one can go as far as terms in $(\beta/\alpha)^2$.

What we must do is expand the expression $(1 + \alpha\Lambda\int_0^t e^{\alpha\tau + \varepsilon_\tau} d\tau)^{-B}$ in powers of $\alpha\int_0^t e^{\alpha\tau}(e^{\varepsilon_\tau} - 1).d\tau$, which is roughly of order β/α . Pushing this as far as the square, we shall find ourselves faced with terms of the form of a non-stochastic factor multiplying expressions

$$\alpha E(e^{-n\varepsilon_t}.\int_0^t e^{\alpha\tau}(e^{\varepsilon_\tau} - 1)d\tau) \quad \text{and} \quad \alpha^2 E(e^{-n\varepsilon_t}.\left\{\int_0^t e^{\alpha\tau}(e^{\varepsilon_\tau} - 1)d\tau\right\}^2)$$

For convenience, I shall denote these expressions by P_n and Q_n . Furthermore, I shall denote $\alpha\Lambda\int_0^t e^{\alpha\tau} d\tau$, $= \Lambda(e^{\alpha t} - 1)$ by A. Then we find, on expanding, that

$$E\left\{\int_0^\infty e^{-(r+n\alpha)t - n\varepsilon_t}(1 + \alpha\Lambda\int_0^t e^{\alpha\tau + \varepsilon_\tau} d\tau)^{-B} dt\right\}$$

$$= \int_0^\infty e^{-(r+n\alpha)t}(1 + A)^{-B}\left[e^{n(n+1)\beta t} + \frac{\Lambda}{1+A} P_n + \frac{\Lambda^2}{(1 + A)^2} Q_n\right]dt \quad , \quad (81)$$

equation and is equal to k when $t = 0$. Thus (79) gives capital for each t. Writing, as we did before, Λ for $\frac{a(1-b)k^{b-1}}{\alpha(n+1)}$ we have

$$Z^{n+1} - Z^n = \frac{a^n}{n+1} E\left\{ D\left(\int_0^\infty a^{-n} e^{-(r+n\alpha)t - n\mathcal{E}_t}\left[1 + \alpha\Lambda\int_0^t e^{\alpha\tau + \mathcal{E}_\tau} d\tau\right]^{-B} dt\right)\right\}. \quad (80)$$

B is $nb/(1-b)$.

We can apply the operator E before applying D. We cannot hope to obtain a finite expression for the integral, so what we must do is estimate its value when β is small. In other words, we must attempt to expand the integral in powers of β: more precisely, in powers of β/α. There is no reason, in principle, why one should not push this expansion as far as one wants, but it is a very complicated process. I shall content myself with showing how one can go as far as terms in $(\beta/\alpha)^2$.

What we must do is expand the expression $(1 + \alpha\Lambda\int_0^t e^{\alpha\tau + \mathcal{E}_\tau} d\tau)^{-B}$ in powers of $\alpha\int_0^t e^{\alpha\tau}(e^{\mathcal{E}_\tau} - 1).d\tau$, which is roughly of order β/α. Pushing this as far as the square, we shall find ourselves faced with terms of the form of a non-stochastic factor multiplying expressions

$$\alpha E(e^{-n\mathcal{E}_t}.\int_0^t e^{\alpha\tau}(e^{\mathcal{E}_\tau} - 1)d\tau) \quad \text{and} \quad \alpha^2 E(e^{-n\mathcal{E}_t}.\left\{\int_0^t e^{\alpha\tau}(e^{\mathcal{E}_\tau} - 1)d\tau\right\}^2)$$

For convenience, I shall denote these expressions by P_n and Q_n. Furthermore, I shall denote $\alpha\Lambda\int_0^t e^{\alpha\tau} d\tau$, $= \Lambda(e^{\alpha t} - 1)$ by A. Then we find, on expanding, that

$$E\left\{\int_0^\infty e^{-(r+n\alpha)t - n\mathcal{E}_t}(1 + \alpha\Lambda\int_0^t e^{\alpha\tau + \mathcal{E}_\tau} d\tau)^{-B} dt\right\}$$

$$= \int_0^\infty e^{-(r+n\alpha)t}(1 + A)^{-B}\left[e^{n(n+1)\beta t} + \frac{\Lambda}{1+A} P_n + \frac{\Lambda^2}{(1+A)^2} Q_n\right]dt \quad , \quad (81)$$

as far as terms of the orders we want. I have used here the fact that $E(e^{-n\varepsilon_t}) = e^{n(n+1)\beta t}$, which can be deduced at once from the characteristic function for ε_t.

We must now multiply the right hand side of equation (81) by a^{-n} and apply the operator D. We have to remember that Λ is a multiple of a. Abandoning terms that are too small to concern us, we find that

$$a^nD(a^{-n}(1+A)^{-B}) = -(r+n\alpha-n(n+1)\beta)(1+A)^{-B} - (\alpha-2n\beta)BA(1+A)^{-B-1}$$
$$+\beta B(B+1)A^2(1+A)^{-B-2} ;$$
$$a^nD(a^{-n}\Lambda(1+A)^{-B-1}) = -(r+n\alpha-n(n+1)\beta)\Lambda(1+A)^{-B-1}$$
$$+(\alpha-2n\beta)\Lambda(1+A)^{-B-1} - (\alpha-2n\beta)(B+1)\Lambda A(1+A)^{-B-2}$$
$$-2\beta(B+1)\Lambda A(1+A)^{-B-2} + \beta(B+1)(B+2)\Lambda A^2(1+A)^{-B-3} ;$$
$$a^nD(a^{-n}\Lambda^2(1+A)^{-B-2}) = -(r+n\alpha)\Lambda^2(1+A)^{-B-2}$$
$$+2\alpha\Lambda^2(1+A)^{-B-2} - \alpha(B+2)\Lambda^2A(1+A)^{-B-3} .$$

It is convenient to multiply P_n and Q_n by $e^{-n(n+1)\beta t}/(e^{\alpha t}-1)$ and $e^{-n(n+1)\beta t}/(e^{\alpha t}-1)^2$, respectively, denoting the results by P_n' and Q_n' respectively. We find that

$$Z^n - Z^{n+1} = \frac{1}{n+1}\int_0^\infty e^{-(r+n\alpha-n(n+1)\beta)t}(1+A)^{-B}.R.dt., \qquad (82)$$

where:

$$R = r+n\alpha-n(n+1)\beta + ((\alpha-2n\beta)B + (r+(n-1)\alpha-n(n-1)\beta)P_n')\frac{A}{1+A}$$
$$-(\beta B(B+1) - (\alpha-2(n-1)\beta)(B+1)P_n' - (r+(n-2)\alpha)Q_n')\frac{A^2}{(1+A)^2}$$
$$-(\beta(B+1)(B+2)P_n' - \alpha(B+2)Q_n')\frac{A^3}{(1+A)^3} \qquad (83)$$

further terms being of the order of β^3 or more.

It can be checked, by the method used in the previous chapter that (82) reduces to the improved first approximation for the no-uncertainty case when $\beta = 0$. As in that case, a form of the integral better adapted to numerical integration is obtained when $e^{-\alpha t} = X$ is made the variable of integration. We must therefore evaluate P_n' and Q_n' in terms of X.

$$P_n = \alpha E\left[\int_0^t e^{\alpha\tau}\left(e^{-n(\mathcal{E}_t - \mathcal{E}_\tau) - (n-1)\mathcal{E}_\tau} - e^{-n\mathcal{E}_t}\right).d\tau\right],$$

$$= \alpha e^{n(n+1)\beta t}\int_0^t \left[e^{(\alpha-2n\beta)\tau} - e^{\alpha\tau}\right].d\tau$$

Therefore,

$$P_n' = \frac{e^{(\alpha - 2n\beta)t} - 1}{e^{\alpha t} - 1}\left(1 - 2n\frac{\beta}{\alpha}\right)^{-1} - 1 \; . \qquad (84)$$

As $t \longrightarrow \infty$, this expression tends to -1 ; as $t \longrightarrow 0$, it tends to 0 . For positive t , it is negative. We can write

$$P_n' = \frac{X^{2n\beta/\alpha} - X}{1 - X}\left(1 - 2n\frac{\beta}{\alpha}\right)^{-1} - 1 \; ,$$

where $X = e^{-\alpha t}$. When $X = 1$, $P_n' = 0$.

It is not a good idea to expand this expression in powers of β/α, for not only is it more easily calculated in this form, but the approximation would be bad near $X = 0$. The fact that P_n' is not of order β/α at $X = 0$ need not worry us, for the integrand will be zero at $X = 0$ (see (86) below and following comments.)

The same method of splitting $\mathcal{E}_t$ up into independent parts can be used to evaluate Q_n' . It is found that

$$Q_n = 2\alpha^2 E\left(\int_0^t e^{-n(\mathcal{E}_t - \mathcal{E}_\tau) + \alpha\tau}\int_0^\tau e^{\alpha\tau'}\left(e^{-(n-1)(\mathcal{E}_\tau - \mathcal{E}_{\tau'}) - (n-1)\mathcal{E}_{\tau'}} - e^{-n(\mathcal{E}_\tau - \mathcal{E}_{\tau'}) - n\mathcal{E}_{\tau'}}\right)\right.$$

$$\left. \times\, (e^{\mathcal{E}_{\tau'}} - 1).\, d\tau' d\tau\right)$$

$$= 2\alpha^2 \cdot e^{n(n+1)\beta t}\Big(\int_0^t e^{(\alpha-2n\beta)\tau}\int_0^\tau (e^{(\alpha-2(n-1)\beta)\tau'} - e^{\alpha\tau'})\,d\tau'\,d\tau$$

$$- \int_0^t e^{\alpha\tau}\int_0^\tau (e^{(\alpha-2n\beta)\tau'} - e^{\alpha\tau'})\,d\tau'\,d\tau\Big) \; .$$

It follows that

$$Q_n' = 1 - \frac{2}{1-2n\frac{\beta}{\alpha}}\,\frac{e^{2(\alpha-2n\beta)t}-1}{(e^{\alpha t}-1)}$$
$$+ \frac{\dfrac{(1-2n\beta/\alpha)e^{2(\alpha-(2n-1)\beta)t} - 2(1-(2n-1)\beta/\alpha)e^{(\alpha-2n\beta)t} + (1-2(n-1)\beta/\alpha)}{(1-2(n-1)\beta/\alpha)(1-(2n-1)\beta/\alpha)(1-2n\beta/\alpha)}}{(e^{\alpha t}-1)^2} \tag{85}$$

The limits at $t = 0$ and $t = \infty$ are 0 and 1 respectively. Q_n is always positive for positive t. It is again a simple matter to transform to the new variable X. As with P_n', it is unwise to expand the expression in powers of β/α : it is best to calculate it as it stands. (82), (83), (84), (85) together allow the improved first approximation to be calculated reasonably closely for the usual values of α, β, and r.

When the variable of integration is X, it is easy to express R in terms of X, and we have (from (82)):

$$Z^n - Z^{n+1} = \frac{1}{n+1}\int_0^1 X^{n-1+\frac{r}{\alpha}-n(n+1)\frac{\beta}{\alpha}} \cdot \left(1+A\frac{1-X}{X}\right)^{-B}\frac{R(X)}{\alpha}\,dX \tag{86}$$

This is the form to use for numerical work. Fairly simple methods of numerical integration are quite sufficient at the levels of accuracy attainable. (See, e.g., (Comrie 1961), p.352.) If a calculation to the first order in β/α is deemed sufficient, some of the terms in the expression for R, (83), can be omitted, remembering that, except near $X = 0$, P_n' is of order β/α, and Q_n' is of order $(\beta/\alpha)^2$. At $X = 0$, the integrand is zero so long as

$$n - 1 + \frac{r}{\alpha} - n(n+1)\frac{\beta}{\alpha} + B > 0$$

and $R(X=0)$ is finite. It is reasonable to assume this last, although it seems to be difficult to prove. The series of which (83) is the first few terms would seem to converge when $A/(1+A) = 1$. A derivation of the value of R at $X=0$ (which is only of interest when the above inequality fails to be satisfied) and a more precise evaluation of the right hand side of (80) must remain questions for further study. It should be noted that the integral in (86) converges so long as

$$n + \frac{r}{\alpha} - n(n+1)\frac{\beta}{\alpha} + B > 0 .$$

The empirical values suggested later for the parameters suggest that this inequality will only very rarely fail to be satisfied: it is a reasonable assumption that the previous inequality is satisfied.

The improved method of approximation is the best method I have been able to find for obtaining numerical estimates of optimal policies under uncertainty. The question arises: in cases where the optimum policy without uncertainty can be calculated precisely (as in the constant-elasticity case,) will we not do better to follow the policy that can be calculated precisely, rather than a policy that is only known approximately, although for a more correct model? I would suggest that the most sensible procedure is to correct the no-uncertainty policy by adding to it (or subtracting) the correction that has to be made to the improved approximation, that is, the difference between the improved approximations in the two cases. When the first process of approximation works reasonably well, this could be done using the simpler and more easily calculated approximations produced by the straightforward process.

When $\beta = 0$ and $B < 1$, we know from equation (58) that

$Z^{n+1} - Z^n$ is asymptotically equal to a multiple of Λ^{-B} . It is quite easy to see that the same is true in the present case. The asymptotic form is obtained by putting $A/(1+A) = 1$ in (86). The working is straightforward, and similar to the no-uncertainty case, so that there is no point in reproducing it. The asymptotic form is not a good approximation, and it as easy to calculate the integral (86).

When β is large enough, the valuation integral for the static policy diverges, and the second method of approximation fails. I shall suggest later that this is an empirically improbably contingency, but presumably it is possible. A rather natural device to solve this difficulty is to replace the valuation integral by the corresponding integral with $\beta = 0$, and it might be used for computation. It is not, however, theoretically a very respectable device.

\- 3 -

Let us turn now to the general features of the solution in the special case. When there was no uncertainty, we could find enough information about the solutions of the ordinary differential equation to be able to discover virtually anything we should like to know, and calculate the optimum strategy to any desired accuracy into the bargain. However we now have to deal with an equation that is essentially much more complicated. It is

$$\beta \frac{d^2 z}{d\eta^2} + (\alpha - (2n+3)\beta)\frac{dz}{d\eta} - (n+1)\alpha\gamma z = e^{\eta}\left[(1-b)\frac{d}{d\eta}H(z) + nbH(z)\right], \qquad (23)$$

where $H(z) = z - \frac{n+1}{n} z^{n/(n+1)}$.

z is $(1-\sigma)^{-n-1}$, and η is $\log_e(af(k)/k)$.

If p and q are the roots of the quadratic equation

$$\beta X^2 + (\alpha - (2n+3)\beta)X - (n+1)\alpha\gamma = 0 \quad , \tag{87}$$

the left hand side of the equation (23) can be written in the form $\beta\left(\frac{d}{d\eta} - p\right)\left(\frac{dz}{d\eta} - qz\right)$. It may be noted that when $r = 0$, the two roots of (87) are $n + 1$ and $n + 2 - \alpha/\beta$. Let us define a variable

$$w^* = \frac{dz}{d\eta} - qz - \frac{1-b}{\beta} e^{\eta} H \ . \tag{88}$$

Then it is clear that z and w^* satisfy the pair of equations:

$$\left.\begin{aligned} \frac{dz}{d\eta} &= qz + w^* + \frac{1-b}{\beta} e^{\eta} H \\ \frac{dw^*}{d\eta} &= pw^* + \frac{(n+1)b - 1 + p(1-b)}{\beta} e^{\eta} H \end{aligned}\right\} \tag{89}$$

I eliminate the term in $e^{\eta} H(z)$ from one of the equations, by defining

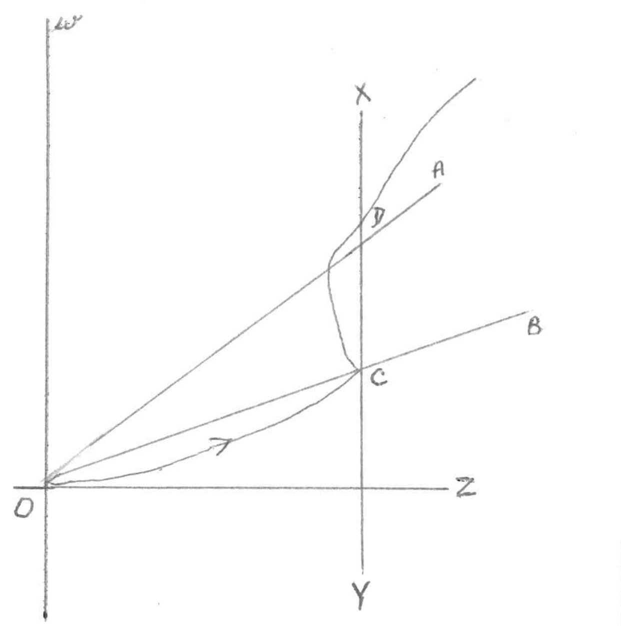
$$w = \frac{(n+1)b - 1 + p(1-b)}{1-b} z - w^* \ . \tag{90}$$

Then

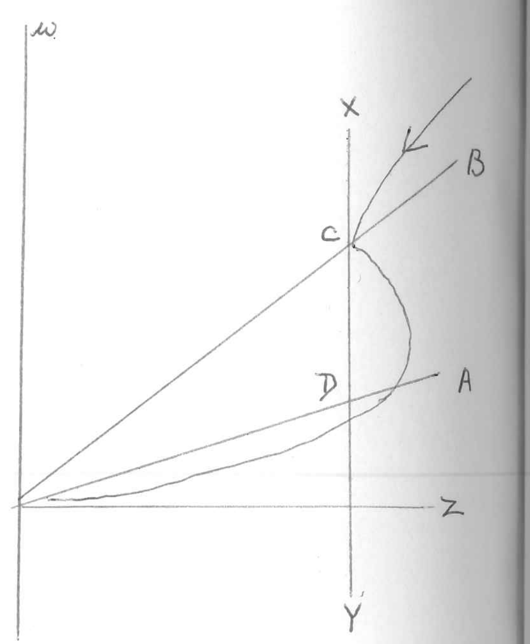
$$\begin{aligned} \frac{dz}{d\eta} &= (B - 1 + p + q)\, z - w + \frac{1-b}{\beta} e^{\eta} H \\ \frac{dw}{d\eta} &= (B - 1 + p)(B - 1 + q)z - (B - 1)w \end{aligned} \tag{91}$$

B being, as before, $nb/(1-b)$.

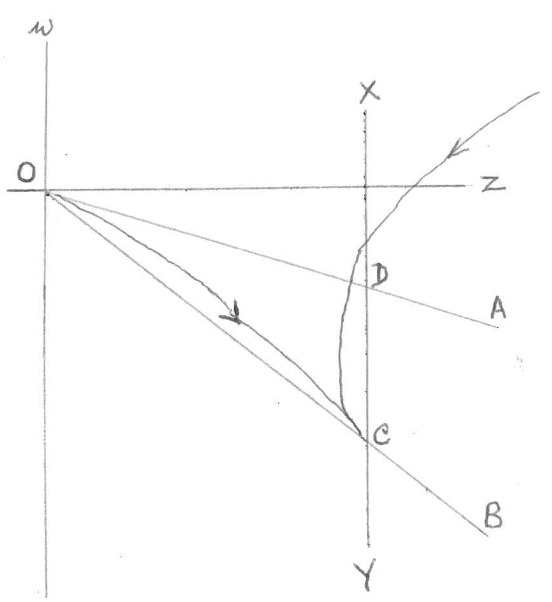
Although the variable η occurs explicitly in the equations (91), it is helpful to represent their solutions in the two-dimensional (z,w)-space, and this is done in the accompanying diagrams (6.1 - 6.8). The solutions that interest us are those for which $z > 1$ as η tends to infinity, for then σ is positive for very small k , and so k can never become negative. From among these solutions, we have to choose the one for which $\int_0^{\infty} x^{-(n+1)b} z(\log ax^{b-1})dx$ is a minimum for all k . (When the integral diverges, "minimum" is to be understood in the



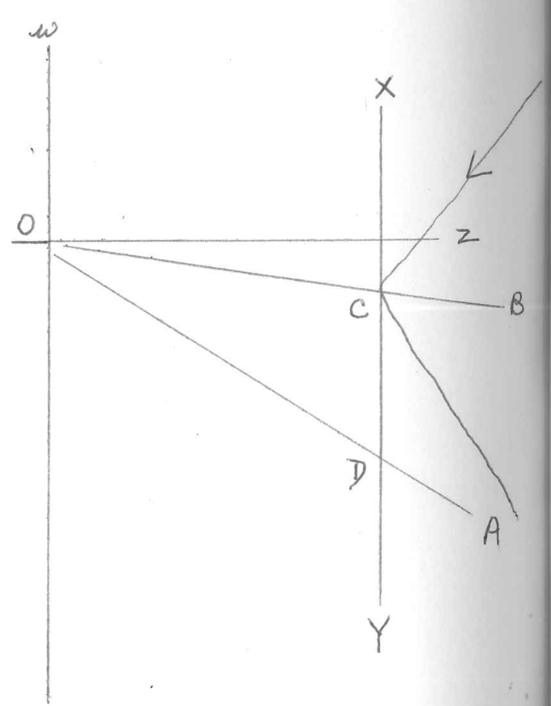
6.1: $B > 1,\ \gamma > 0,\ B - 1 + p + q + \frac{pq}{B-1} > 0$



6.2: $B > 1,\ \gamma < 0,\ B - 1 + p + q > 0$



6.3: $B > 1,\ \gamma > 0,\ B - 1 + p + q + \frac{pq}{B-1} < 0$



6.4: $B > 1,\ \gamma < 0,\ B - 1 + p + q <$

extended sense.)

The first thing to notice is that $H(z)$ is zero when $z = 0$ or $\bar{z} = \left(\frac{n+1}{n}\right)^{n+1}$, and that $H(z) < 0$ when $0 < z < \bar{z}$. $H(z) > 0$ when $z > \bar{z}$. If OA in the diagram is the line

$$w = (B - 1 + p + q)z \ ,$$

then $\frac{dz}{d\eta} < 0$ for points above OA with $z \leqq \bar{z}$, and $\frac{dz}{d\eta} > 0$ for points below OA with $z \geqq \bar{z}$.

If $B > 1$, $\frac{dw}{d\eta}$ is greater than or less than zero according as the point (z,w) is below or above the line OB:

$$w = \left(B - 1 + p + q + \frac{pq}{B-1}\right)z \ .$$

If $B < 1$, it is greater than zero below the line, less than zero above it. Clearly OA lies above or below OB (for $z > 0$) according as $\frac{pq}{B-1}$ is negative or positive. Since $pq = -(n+1)\frac{\alpha\gamma}{\beta}$ (because p and q are the roots of the equation (87)), and α and β are both positive, the condition for OA to lie above or below OB is

$$\frac{\gamma}{1 - B} \gtrless 0 \ .$$

When $\gamma = 0$, the two lines are identical. The case $B = 1$ will have to be dealt with separately.

Let us ask first from where a trajectory may start; that is, what can happen to (z,w) as η tends to $-\infty$. If either z or w tends to a finite limit, the corresponding derivative must tend to zero. If $B > 1$, it is clearly possible for both z and w to tend to $+\infty$, but they cannot both become infinite in any other way: negative infinity for both, which may seem possible, is excluded because H becomes a complex function when $z < 0$. If $B < 1$, the only possibility is

$w \longrightarrow -\infty$, $z \longrightarrow +\infty$. If the limit of w is finite, the limit of the trajectory lies on OB , and so the limit of z must be finite too. The derivative of z vanishes as η tends to $-\infty$ only if $z = w = 0$. If the limit of z is finite, the limit of w must also be finite.

As η tends to $+\infty$, there are a number of possibilities. z may tend to infinity, but it will appear that such solutions do not concern us, since others provide superior strategies. In the limit both z and w may remain finite. The limit point would have to lie on OB , and $H(z)$ would have to be zero. Thus the two possibilities are $(0,0)$ and $(\bar{z}, (B-1+p+q+\frac{pq}{B-1})\bar{z})$. Of these two, only the latter, which I call C , is of interest to us, since z must not be less than 1 as η tends to $+\infty$. The derivative must be the same sign as the variable if the variable tends to ∞: therefore w tends to infinity only if $B < 1$. Consider this case, assuming that z tends to a finite limit z_∞ . From the second equation (91), it is clear that w is of order $e^{(1-B)\eta}$; hence $H(z_\infty)$ cannot be non-zero, since $\frac{dz}{d\eta}$ has to be zero. This means we are interested only in the case $z_\infty = \bar{z}$.

We can integrate the second equation (91) to obtain:

$$we^{-(1-B)\eta} = (B-1+p)(B-1+q)\int ze^{-(1-B)\eta}\, d\eta ,$$

the integral being an indefinite integral. As $\eta \longrightarrow \infty$, $z = \bar{z} + o(1)$, so that the integral is

$$W^* - \frac{\bar{z}}{1-B} e^{-(1-B)\eta} + o(e^{-(1-B)\eta}) ,$$

Where W^* is a constant. If we substitute this expression for w in the first equation (91) and remember that $\frac{dz}{d\eta} = o(1)$, we have

$$\frac{1-b}{\beta} e^{\eta} H = (B-1+p)(B-1+q) \overset{*}{W} e^{(1-B)\eta} - \frac{pq\bar{z}}{1-B} + o(1) .$$

It is easy to check that in the neighbourhood of $\bar{z}$,

$$H(z) = \frac{z - \bar{z}}{n+1} + o((z - \bar{z})^2) .$$

Thus $\quad z - \bar{z} = We^{-B\eta} +$ terms of smaller order, (92)

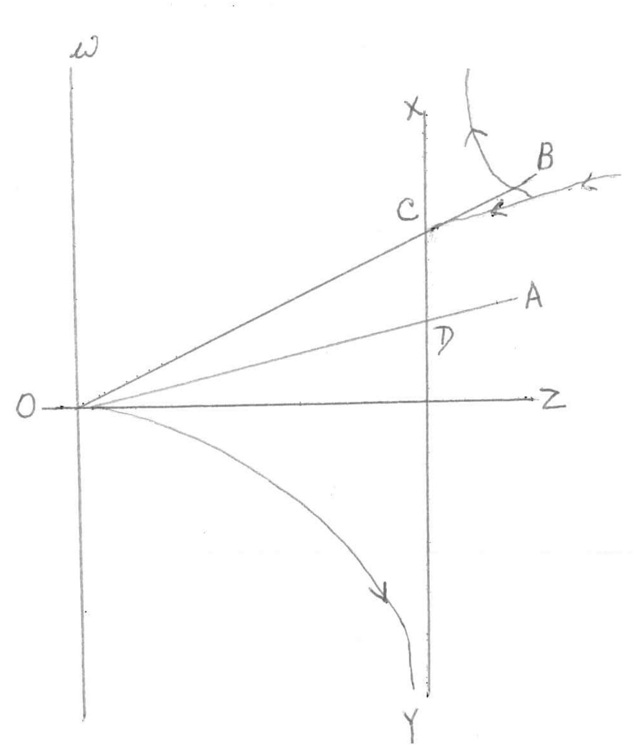
where W is a non-zero constant, or

$$z - \bar{z} = -\beta\frac{n+1}{1-b}\,\frac{pq}{1-B}\,\bar{z}.e^{-\eta} + \text{ terms of smaller order.} \qquad (93)$$

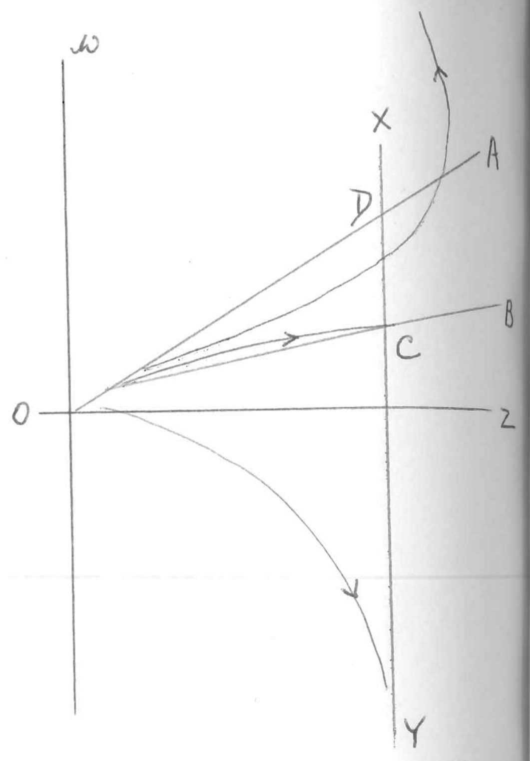
We must notice that z will be smaller (in the neighbourhood of $\bar{z}$) when W is strictly negative than when W is zero or positive, for any large η .

With this information, we can map the relevant solutions roughly. A number of different cases have to be considered: there is a diagram for each. In the diagrams, XY is the line $z = \bar{z}$. The first four diagrams cover the cases $B > 1$. In all these $\frac{dw}{d\eta} < 0$ at points above OB , and > 0 at points below OB . In all the diagrams $\frac{dz}{d\eta}$ is certainly positive in the region ODX , and certainly negative in the region YDA (D being the point in which OA cuts XY.) These facts enable us to sketch in the shape of solution curves apart from those for which z tends to zero or infinity. There are two possibilities in each of the first four diagrams. In the first three cases, that is when $B > 1$, $\gamma > 0$, and when $B > 1$, $\gamma < 0$, and $B - 1 + p + q > 0$, it is possible for a solution curve to start from O , and end at C . Such a curve must correspond to a strategy that is superior to that given by any solution curve coming in from infinity. Thus in these three cases, the optimum strategy is given by a curve OC . In the fourth case, it is not easy to tell precisely where the solution

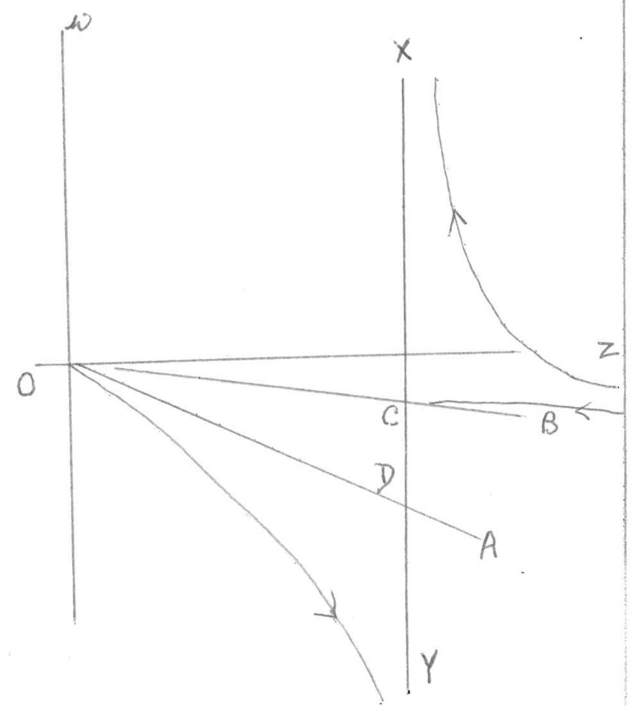
Opposite p. VI - 18



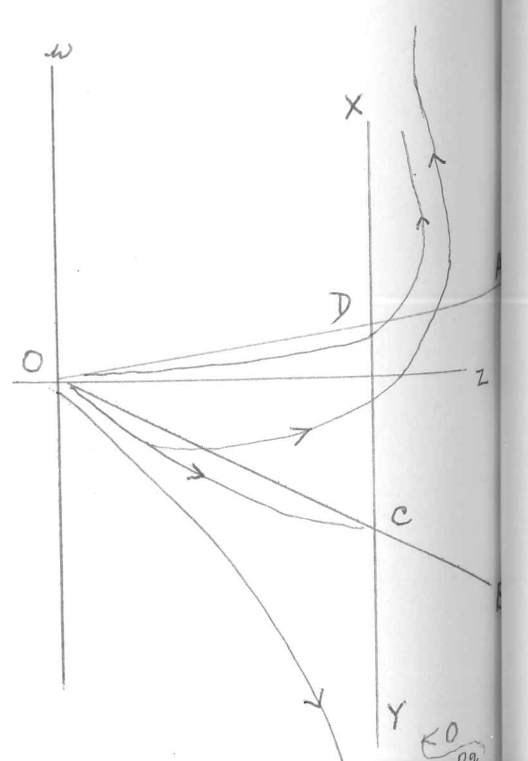
4.5: $B < 1$, $\gamma > 0$, $B-1+p+q+\frac{pq}{B-1} > 0$



4.6: $B < 1$, $\gamma < 0$, $B-1+p+q+\frac{pq}{B-1} > 0$



4.7: $B < 1$, $\gamma > 0$, $B-1+p+q+\frac{pq}{B-1} < 0$



4.8: $B < 1$, $\gamma < 0$, $B-1+p+q+\frac{pq}{B-1} < 0$, $B-1+p+q > 0$

curve for the optimum strategy comes from, except that it always lies to the right of XY and above OA . In this case $z > \bar{z}$ always for the optimal strategy, whereas in case 2 ($B > 1$, $\gamma < 0$, $B-1+p+q > 0$,) although $z > \bar{z}$ for large enough η, it tends to 0 as η tends to $-\infty$. In all the four cases, the asymptotic form of the solution is given by (93).

The second set of four diagrams represents the cases for which $B < 1$. In these cases, as we have just seen, w can tend to infinity (positive or negative). It appears that in each case, there are three possible positions for the solution curve we seek. In the fifth and seventh cases, i.e. when $B < 1$ and $\gamma > 0$, it is clear which is the solution we want, for only one of the three kinds of curve begins at 0 . Thus the optimum strategy is given by (92) for some negative value of W. When $\gamma < 0$, there are three kinds of curve to choose from, corresponding to negative, zero, and positive values of W, and no obvious way of choosing between them.

If we are to discover what shape of curve gives the optimum strategy in cases 6 and 7 , we shall have to study the solutions a little more closely, especially in the neighbourhood of 0 . I shall show that in both cases, the optimum strategy corresponds to a curve going upwards, along which w tends to $+\infty$, provided that an optimum strategy exists at all.

Consider two solution curves beginning at 0 . One lies above the other, at any rate for small enough η. I shall show that for given η, z is smaller on the upper curve; and furthermore that the two curves do not intersect. I assume that $z < \bar{z}$, for the moment. We have two solutions, then: $(z_1(\eta), w_1(\eta))$, and $(z_2(\eta), w_2(\eta))$.

Both tends to $(0,0)$ as η tends to $-\infty$; and for given z , not equal to zero, but sufficiently small, $w_1 > w_2$. I shall treat z as the dependent variable, and - temporarily - write x for e^η . We can express the two solutions as $(w_1(z), x_1(z))$ and $(w_2(z), x_2(z))$.

The first equation (91) can be rewritten:

$$\frac{dx}{dz} = \frac{x}{(B + p + q - 1)z - w(z) + (1-b)/\beta . H(z)x} \tag{94}$$

On both the solution curves, $\frac{dz}{d\eta}$ must be positive. (It can be shown quite easily that such a curve would continue leftwards once it had turned that way.) Hence the denominator is positive. From (94):

$$\frac{dx_1}{dz} - \frac{dx_2}{dz} > \frac{(B+p+q-1)z(x_1-x_2) - x_1w_2 + x_2w_1 \cancel{(B+p+q-1)z(x_1-x_2) - x_1w_2 + x_2w_1}}{((B+p+q-1)z - w_1)((B + p + q - 1)z - w_2)}$$

the inequality being produced by removing negative terms from the two positive factors in the denominator. If we now use the inequality $w_1 > w_2$, we can cancel the positive factor $(B+p+q-1)z - w_2$; and, writing G for $((B+p+q-1)z - w_1)^{-1}$, we find that

$$\frac{d}{dz}(x_1 - x_2) > G.(x_1 - x_2) . \tag{95}$$

$G > 0$. Of course the strict inequality does not hold when $z = 0$, and equality is possible there. Let us write (95) as:

$$\frac{d}{dz}(x_1 - x_2) = G.(x_1 - x_2) + g(z) ,$$

where $g(z)$ is some function of z positive in the relevant range. Without loss of generality, we can assume that G is a constant, say the greatest lower bound of $G(z)$ on the first curve in the range we are concerned with. Integrating the equation, we have

$$(x_1 - x_2)e^{-Gz} = \int_0^z g(\zeta)\, e^{-G\zeta} d\zeta + \text{constant} .$$

Since x_1 and x_2 are both zero when z is zero, the constant is zero. Hence, since g is a positive function,

$$x_1(z) > x_2(z) .$$

It follows that for any given x, z on the first curve is less than on the second, since z increases with x. Thus we have shown that

$$z_1(\eta) < z_2(\eta) \tag{96}$$

for sufficiently small η, when $w_1 > w_2$.

The solution we seek is the one for which

$$\int_k^{\infty} x^{-(n+1)b} z(\log ax^{b-1})dx \quad *$$

is a minimum for all k. (And we know that it is a minimum for all k if it is a minimum for some k, because the optimum strategy is a solution of the equation.) Large k corresponds to small η. Hence the eligible solution with the smallest z for small values of η is the one we want. The result I have just proved shows that this solution is the one that comes out of O above all the others. To prove that it is the one that <u>ends</u> above all the others, viz. one on which w tends to $+\infty$, we should have to prove that no other eligible solution crosses this one.

Consider first case 6: $B < 1$, $\gamma < 0$, $B-1+p+q+\frac{pq}{B-1} > 0$.

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* In this expression, x is only a dummy variable of integration, and not connected with the variable x I have just been using.

Opposite p. VI - 21

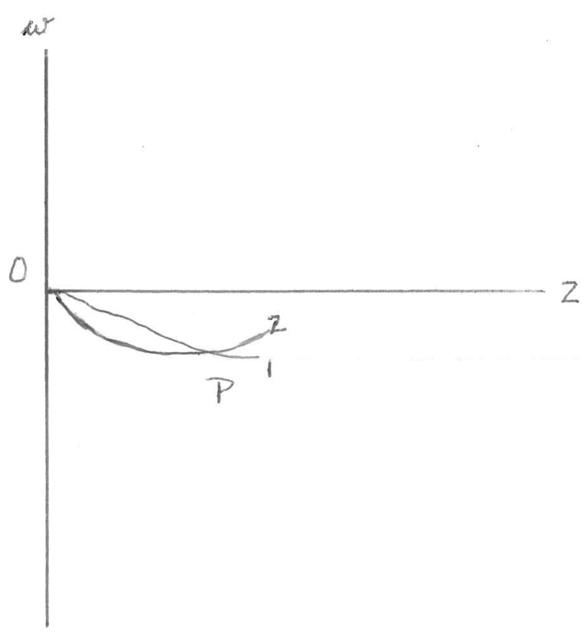

diagram 6.9

Suppose that two solution curves beginning at O cross at some point P. (see diagram 6.9) Let P be the first point in which these two curves cross. Denote by the subscript 1 the curve on which $\dot{w}$ is greater between O and P, and the other one by subscript 2. I have just shown that η_1 is greater at P than η_2. z and w are the same at P for both curves, naturally; and $\frac{dw}{d\eta}$ is the same on both curves. Since $\eta_1 > \eta_2$, $\frac{dz}{d\eta}$ is greater on the second curve. If $\frac{dw}{d\eta}$ is positive, this is plainly inconsistent with the assumption that the second curve cuts the first from below at P.

Now in case 6 the downward moving curve is excluded, since the curves above the line OB lie entirely above it. $\frac{dw}{d\eta} > 0$ for the other eligible curves, and so they do not cross one another. On the assumption that there exists a solution curve beginning at O, curling round D in the manner portrayed in diagram 6.6, and going away vertically upwards, it is thus proved that this curve represents the optimum strategy. Can we prove that such a solution curve exists? It is rather important, for if it does, no solution curve beginning at O can remain to the left of XY : that is, the other forms of solution curve cannot exist. This is so for the following reason: if the upper solution curve crosses XY at a certain value of η, any solution curve that remains to the left of XY must attain that value of η at a lower value of z, that is, less than $\bar{z}$; yet we have just seen that a given value of η is reached at a greater value of z on the <u>lower</u> of two solution curves beginning at O. However, there is no doubt that a solution curve beginning at O crosses CD above C. Solution curves certainly cross CD : they cannot previously have crossed OD or OC, since curves go from left to right across OC, and upwards across CD . Therefore a solution curve that crosses CD must have begun at O . Therefore all solution curves beginning at O must cross XY . Continuity considerations show that one must tend vertically to infinity as shown.

There remains case 8 , which is properly two cases, since the situation is different according to whether OA lies above or below the z-axis . However we are not really interested in the case $\gamma < 0$, $B - 1 + p + q < 0$, for it can only happen if r is somewhat less than 0 . $\gamma < 0$ implies that

$$r + (n+1)\alpha < (n+1)(n+2)\beta \ . \quad p + q = 2n+3 - \frac{\alpha}{\beta} > 2n+3 - (n+2) + \frac{r/\beta}{n+1}$$

so that the conditions imply that $\frac{r}{\beta} < -(B+n)(n+1)$. Certainly, the case is excluded if $r \geqq 0$, which is an assumption that seems to me sensible.

This leaves us with the sub-case illustrated, in which $B - 1 + p + q + \frac{p\,q}{B - 1} < 0$, and $B - 1 + p + q > 0$. I shall show that there exists a solution curve beginning at O , on which w always increases. It then follows, in the same way as for case 6, that one such curve lies above the other eligible curves, and approaches XY as asymptote from $z > \bar{z}$.

In order to prove that a curve of the desired type begins at O , I must show that some solution curves leave O at an angle greater than zero (but less than $B - 1 + p + q$, of course.) One way of doing so is to transform to new variables, $X = -\frac{H(z)}{z}e^{\eta}$, $Y = \frac{w}{z}$. The equations turn out to be:

$$\frac{dX}{d\eta} = X - \frac{X + e^{\eta}}{n + 1}\left(B - 1 + p + q - Y - \frac{1-b}{\beta}X\right) \qquad (97)$$

$$\frac{dY}{d\eta} = (Y - p + 1 - B)(Y - q + 1 - B) + \frac{1 - b}{\beta}XY$$

We want to examine the behaviour of X and Y as η tends to $-\infty$.

If Y tends to a finite limit, $\frac{dY}{d\eta}$ tends to zero. If X tends to zero, the limit can be $\bar{Y} = p + 1 - B$ or $q + 1 - B$. If X tends to a finite limit, $\bar{X}$, $\bar{X} = 0$, or satisfies

$$\bar{X}(n+1) - \bar{X}(B - 1 + p + q - \bar{Y} - \frac{1-b}{\beta}\bar{X}) = 0$$

and also

$$(\bar{Y} - p + 1 - B)(\bar{Y} - q + 1 - B) + \frac{1-b}{\beta}\bar{X}\bar{Y} = 0$$

If these two equations are solved for $\bar{Y}$, it is found that

$$\bar{Y} = \frac{(p+B-1).(q+B-1)}{B+n} \tag{98}$$

Since we are dealing with case 8, $(p+B-1)(q+B-1) > 0$, and therefore this $\bar{Y} > 0$. It remains to check that $\bar{Y}$ (which is the initial slope of the solution curve, of course) is less than $B-1+p+q$., $(p+B-1)(q+B-1) <$ $< (B+n)(B+p+q)$ if and only if

$$pq < (n+1)(B-1+p+q) .$$

On substituting the expressions for pq and $p+q$ in terms of r, α, and β, it is found that the condition is equivalent to

$$r > -(n+1)(B+n)\beta .$$

which was, indeed the condition for $B-1+p+q$ to be greater than zero. I am going to restrict myself to the assumption $r \geqq 0$. Those who are interested in the assumption of exponential population growth will be interested in smaller values of r : but when $r < -(n+1)(B+n)$, it is difficult to see what the shape of the optimal solution curve is.

I have not as yet considered the boundary cases, when one or other of the inequalities defining the different cases is replaced by an equality. Cases 1 and 3 are divided by the conditions $B - 1 + p + q + \frac{pq}{B-1} \gtrless 0$.

When $B - 1 + p + q + \frac{pq}{B-1} = 0$, the line OB coincides with the z-axis. No solution curve OC is possible. z is greater than $\bar{z}$ for small enough η . On the boundary between cases 2 and 4 , OA coincides with the z-axis: the solution trajectory has z and w positive throughout. Cases 5 and 7 are divided by the possibility that OB coincides with the z-axis. If it does, no solution curve can terminate at C , but this does not affect our conclusion that the optimum strategy is given by the curve moving downwards from O . In the boundary case between cases 6 and 8, with OB coinciding with the z-axis , there can be no solution OC , but we have seen that this is so in any case. Again, our previous conclusion about the position of the optimum solution curve stands.

Consider what happens when $\gamma = 0$. The important feature of this case is that $z = \bar{z}$ is a solution of the equations: in other words, the point C is in itself a solution curve. Is the optimum strategy given by $z = \bar{z}$? It seems not. When $\gamma = 0$, OA and OB coincide. If $B > 1$, and $B - 1 + p + q > 0$, a solution curve can go from O to C : it is superior to the point C , considered as a solution curve, since it begins with smaller z . If $B > 1$, and $B - 1 + p + q \leqq 0$, no curve can run from O to C , so that $z = \bar{z}$ does give the optimal policy. (But this is not an interesting case, since it is excluded when $r \geqq 0$). If $B < 1$, and $B - 1 + p + q > 0$, the only eligible solution curves are C itself, and curves beginning at O and tending to $w = -\infty$. Since the latter start from a smaller value of z , the optimal policy must be represented by one of them. If $B < 1$, and $B - 1 + p + q > 0$, the same is true; but again, the case is not really of interest.

Finally, we must examine what happens when $B = 1$. In this case $\frac{dw}{d\eta} = pqz$. If $pq > 0$, w is an increasing function of throughout the region $z > 0$. Since $\frac{dw}{d\eta}$ cannot vanish except at $z = 0$, w tends to plus infinity as η tends to plus infinity. z tends to $\bar{z}$ from above, since $\frac{dz}{d\eta} < 0$ when w is large enough and $z < \bar{z}$. If $p + q > 0$, the solution curve we seek begins at 0 , crosses XY , then turns back towards XY as asymptote. Similarly, when $pq < 0$, the curve begins at 0 , then moves downwards towards XY as asymptote, z tending to $\bar{z}$ from below. If $p + q \leqq 0$ (not an interesting case) the solution curve comes in from infinity, rather than from 0 . When $B = 1$ and $\gamma = 0$, $pq = 0$. Therefore w is a constant on any solution curve. $z = \bar{z}$ is the optimal strategy in this case.

I have analysed a wide range of cases now, but the possibility $b = 1$ has been excluded. When $b = 1$, we must use the equations (89); it turns out, as one might expect, that the qualitative features are no different in this case from the results I have already stated. It is of no particular interest to go into details, and I shall not do so.

There are so many different cases, that it is worth summarising the results in tabular form. We are only interested in the behaviour of z , since w was only a tool in discovering the form of the solution. In terms of z , there is really very little variety in the final results. I list them in terms of σ. e^{η} is ak^{b-1} . We have discovered the behaviour in the neighbourhood of $ak^{b-1} = 0$, and in the neighbourhood of $ak^{b-1} = \infty$. In the last two columns of the table I list whether σ is $-\infty$ or 1 when ak^{b-1} tends to 0 or ∞ ; and whether σ tends to $\frac{1}{n+1}$ from above or below as ak^{b-1} tends to infinity. If σ is greater than $\frac{1}{n+1}$ in the neighbourhood of $e^{\eta} = 0$, for example, a positive sign is used.

Table II

			$\sigma(0)$	$\sigma(\infty)$
$B > 1$	$\gamma > 0$	$\frac{\alpha}{\beta}(1 + \frac{n+1}{B-1}\gamma) \neq B + 2n + 2$	$-\infty$	$\frac{1}{n+1}$ $-$
		$\frac{\alpha}{\beta}(1 + \frac{n+1}{B-1}\gamma) = B + 2n + 2$	1	$\frac{1}{n+1}$ $-$
	$\gamma < 0$	$\frac{\alpha}{\beta} < B + 2n + 2$	$-\infty$	$\frac{1}{n+1}$ $+$
(		$\frac{\alpha}{\beta} \geq B + 2n + 2$	1	$\frac{1}{n+1}$ $+$)
	$\gamma = 0$	$\frac{\alpha}{\beta} < B + 2n + 2$	$-\infty$	$\frac{1}{n+1}$ $-$
(		$\frac{\alpha}{\beta} > B + 2n + 2$	$\sigma \equiv$	$\frac{1}{n+1}$)

			$\sigma(0)$	$\sigma(\infty)$
$B < 1$	$\gamma \geq 0$	$\frac{\alpha}{\beta} < B + 2n + 2$	$-\infty$	$\frac{1}{n+1}$ $-$
	$\gamma < 0$		$-\infty$	$\frac{1}{n+1}$ $+$
(		$\frac{\alpha}{\beta} \geq B + 2n + 2$	not known	)
$B = 1$	$\gamma > 0$	$\frac{\alpha}{\beta} < 2n + 3$	$-\infty$	$\frac{1}{n+1}$ $-$
(		$\frac{\alpha}{\beta} \geq 2n + 3$	1	$\frac{1}{n+1}$ $-$)
	$\gamma < 0$	$\frac{\alpha}{\beta} < 2n + 3$	$-\infty$	$\frac{1}{n+1}$ $+$
(		$\frac{\alpha}{\beta} \geq 2n + 3$	1	$\frac{1}{n+1}$ $+$)
	$\gamma = 0$		$\sigma \equiv \frac{1}{n+1}$	

(brackets enclose cases that can only occur when $r < 0$.)

The importance of these results is that they show that γ indicates whether the right investment policy involves more or less investment than the static policy, for relatively small values of $\frac{\alpha}{a}k^{1-b}$. They also show us that in most cases, a large enough stock of capital (in relation to $\frac{a}{\alpha}$) requires a small. even negative, rate of investment.* It certainly cannot be said that the optimum rate of investment is greater the less is γ, for the conditions in the third column of the table play an important part in determining whether or not σ is ever less than $1/(n+1)$.

When $\gamma = 0$, we have found that it is possible to discover the asymptotic behaviour of the optimum rate of investment; and this had the considerable advantage that we could identify a point on the curve $\sigma = \sigma(x)$ other than $x = 0$, $\sigma = 1/(n+1)$, and thereby calculate the curve to any desired accuracy. Naturally, when we have uncertainty, the state of the economy cannot tend to an unambiguously defined limit. The whole problem of the situation as t tends to infinity seems to be much more subtle. I have not solved it, and do not propose to discuss it in this thesis. We are therefore left without a completely satisfactory method of determining the optimum policy numerically when $\beta > 0$. It will appear that this is not as serious as might be thought, but it is nevertheless an important lacuna, and it must be hoped that further study of the problems will bring enlightenment.

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* If for some reason, the rate of investment cannot fall below a certain point, say zero, the economy should undergo the minimum rate of investment whenever our analysis recommends a rate of investment less than or equal to it.

- 4 - It is almost time to assess the magnitude of the adjustments to optimal policy that the presence of uncertainty makes necessary. By way of prelude, I want to consider a type of uncertainty that has been absent from the model of this chapter, despite its prevalence. In the model as it stands, the planners are in no doubt about present production possibilities, and can choose the present rate of investment precisely. It would have been more natural to assume that there was considerable uncertainty about present production possibilities, and even about the rate of investment that would result from the planners' actions. It would, at any rate, have been more realistic, but it would have added to the complexity of the model. Insofar as we want to begin a systematic development of a theory of optimal development under uncertainty, simplicity is all to the good, methods being more transparent, and results more clearly expressed; when relevance to the world of action is in question, such an important feature of the situations facing planners can scarcely be neglected.

Here as elsewhere, I shall adopt more or less sensible, but special, assumptions, rather than seek to present results in the most general possible form. I shall distinguish two cases. In the first, the wisdom and power of the planners is such that they can arrange for investment to adjust itself to the (at present imperfectly known) state of the economy. This is not as unrealistic as it may seem, for we may reasonably enquire whether the optimum investment policy is rigid, or flexible, in the face of uncertainty; whether, that is, investment should be little or much different if output turned out to be much less than, or greater than, expected. In the second case, investment policy is related to expected production possibilities, but neither these production possibilities, nor the actual rate of investment, are then known with certainty for the current period.

It is to be expected that the first case is the simpler, and our expectations are not disappointed. Assume that production possibilities at present are given by:

$$Y_o = ae^{\varepsilon_o} f(k_o) \quad ,$$

where ε_o is some random variable.

Investment strategies relate investment to the value of capital, and the actual value of the coefficient of f : for example, in the first period, investment is not known precisely, but is a function of k_o and ae^{ε_o}. For any particular value of ε_o, and an investment strategy, $i(k,a)$, the (expected) valuation of the economy is some function $V(k, ae^{\varepsilon_o}; i)$. For the economy that begins in uncertainty, the one we are considering, the valuation is

$$E_{\varepsilon_o}(V(k, ae^{\varepsilon_o}, i)) \qquad (99)$$

Now, whatever particular value ε_o takes, $V(k, ae^{\varepsilon_o}; i)$ is maximum for the same <u>function</u> $i = s(k,a)$. Therefore the expression (99) is maximum when i is this same function. In other words, the optimum investment strategy is precisely the same in this more general case. Provided that the planners can arrange that actual investment corresponds in any desired way to actual production, it is not necessary for them to know present production possibilities precisely. The analysis of this chapter tells us what optimum planning is, assuming this extreme degree of perfection in the workings of the planning system.

To take a particularly simple case, let us see what is implied when present uncertainty is superimposed on the static case (or on any case where the optimum investment strategy is a constant proportion of total output.) I have shown that the optimum policy is to arrange that investment is a constant proportion of actual output - not, be it noted, a constant proportion of estimated (i.e. expected) output. Ideally the rate of investment produced by the planning system would respond instantaneously to fluctuations in output. Insofar as this may tend to happen in a partially controlled economy - as when the increase in agricultural working capital may be less when agricultural production is poor - we have an argument against interference directed at stabilising the rate of investment. More generally, it is seen to be desirable that planning authorities should distinguish between policies designed simply to fulfil their investment promises, regardless of variations in rates of consumption, and those designed to press towards rates of investment that, though optimal, are almost beyond hope of attainment. Rigidity in an investment policy that is roughly optimal taking one year with another may seriously reduce its value, whereas the nearer approach to optimality produced by downward fluctuations of consumption (that would have happened in any case), as a result of a rigid investment policy that is perforce less than optimal, may, paradoxically, be of real benefit. It is peculiarly difficult to see which is the case in practice, but it may be suspected that the second situation is more prevalent than the first. At any rate, rigidity is a more plausible criticism when directed at a rapidly growing economy than when used to berate the efforts of the near-stagnant.

This argument shows that the pursuit of ideally optimal policies is not entirely pointless; it is, however, often preferable to accept as necessary and realistic an uncertain response to investment policies.

For my second case, I assume that the production of consumer goods, and the production of investment goods (both of which are assumed to be used if produced) are separately uncertain, and that the expected level of investment is related to the expected production possibilities. More precisely, I assume that when the expected production possibilities are given by $af(k)$, and the expected rate of investment is $i(k,a)$, consumption and investment are given by the random variables:

$$\begin{aligned} C &= X_o \, (af(k) - i(k,a)) \;, \\ I &= X_o' \, . i(k,a) \;. \end{aligned} \tag{100}$$

X_o and X_o' are non-negative random variables with means equal to 1. At future times, a and k are themselves random variables; the definitions are not affected.

In order to find the optimal policy for this extended case, it is necessary to return to our original derivation of the fundamental equation by means of the "principle of optimality". The same method can be used to derive an equation for the optimal policy in the present case. Let us write $V(k,a)$ for the total expected valuation of the economy at time $t = 0$ when it follows the optimal policy, the initial stock of capital being k, and the initial mean production function $af(\,.\,)$. In exactly the same way as before, it can be seen that

$$0 = \mathrm{Max}\left\{ E(v(C_o)) + \left[\frac{d}{dt} E(V(k_t, a_t))\right]_{t=o} - r\,V(k,a) \right\} \tag{101}$$

The expectation operators in this equation cover not only the stochastic process $\mathcal{E}_t$ that was included before, but also the new random variables X_o and X_o'; and maximisation is to be achieved from among investment policies $i(k,a)$. When X_o and X_o' had not been created, the expectation operator on the first term inside the curly brackets could be dropped. This is no longer so; but $E(v(C_o)) = E[v(X_o[af(k) - i(k,a)])]$ is a function of $c_o = af(k) - i(k,a)$, and we may write it as $\omega(c_o)$.

The second term in (101) is a little harder to evaluate. For small t ; $k_t = k + t.X_o'i(k,a)$; a_t is, of course $ae^{\alpha t+\varepsilon}t$. As far as a_t is concerned, we can do exactly what we did before. Thus

$$0 = \text{Max}\left[\omega(c_o) + DV(k,a) + \frac{\partial V(k,a)}{\partial k}E(X_o'i)\right]$$

$$= \text{Max}\left(\omega(c_o) + DV + \frac{\partial V}{\partial k}.i\right) \quad ,$$

the expectation operator commuting with $\partial V/\partial k$ in the first line because $\partial V/\partial k$ is a simple quantity, not a random variable. To ensure the maximum, the derivative with respect to i must be zero:

$$\frac{\partial V}{\partial k} = \omega'(c_o) \quad . \tag{102}$$

Furthermore, s being the optimal policy, we have the equation

$$\omega(af(k) - s(k,a)) + s(k,a)\omega'(af(k) - s(k,a))$$

$$= - DV(k,a) \quad . \tag{103}$$

Equations (102) and (103) together provide the fundamental equation. If the total valuation is finite, we can integrate (102), and substitute the resulting integral for V in (103); otherwise we can differentiate (103) and use (102) to eliminate V , the equation for s , though illicitly produced, is easily shown to be valid.

There is no need to discuss the points of rigour that this derivation raises, for the situation is analogous to the simple model, with the function $\omega(c)$ substituted for the original instantaneous valuation function $v(c)$. Thus the new assumptions about present uncertainty certainly make a difference, but one that is easy to assess. In general $\omega(c)$ is essentially different from $v(c)$; but in one important case, namely when $v(c) = - c^{-n}$, the two are proportional. Explicitly, $\omega(c) = E(X_o^{-n})v(c)$. The two are then essentially identical, and the optimal strategy is therefore the same.

I regard the constant-elasticity valuation function as a reasonable median case, and I am deliberately emphasising it in the present thesis. In the absense of clearly formulated and well understood values, it seems to me difficult to make a case for allowing present uncertainty, however large it may be, to affect policy recommendations, although it certainly provides grounds for urging flexible implementation of the optimal policy. If this is granted, it makes more sense than might have appeared at first to ignore present uncertainty in the succeeding discussion of the influence uncertainty should have on optimal policy.

\- 5 -

How important uncertainty is, within the context of the models in this chapter, reduces to the question: how big is β ? We may first get our bearings by reminding ourselves of the relation between a normal distribution and its variance. The square root of the variance is called the standard deviation. The probability of the (absolute) deviation from the mean being greater than one standard deviation is about 0.32 ; the probability of the deviation being greater than two standard deviations is less than 0.05 . e^{ε_t} is the ratio of actual output to expected output (the stock of capital being known). This ratio fluctuates from year: the difference between the logarithm of the ratio in one year and the logarithm of the ratio in the preceding year is - in our model - a normal random variable with mean zero, and variance 2β. The standard deviation is $\sqrt{2\beta}$. Indeed it is a tolerable approximation, when β is small, to say that the ratio of actual to expected output in one year bears to the ratio in the preceding year a proportion that is a normal random variable of mean 1 and standard deviation $\sqrt{2\beta}$. Thus the proportion of this year's output to last year's will differ from the ratio of the expected outputs by more than ______ in roughly one year out of three, while it will differ by more than ______ in only one year out of twenty.

Consider for example the situation when $\beta = 1/2\%$. $\sqrt{2\beta}$ is then 10%. This implies that the proportional growth of output is at least 10% different from what is "usually" expected, in one year out of three; and differs from the expected rate of growth by twenty per cent in one year out of twenty. In most countries, the rate of growth will not differ from the "average" rate of growth by ten per cent as often as one year in three. When the long run pattern of growth suggests a five per cent growth in output in a particular year, it seems - fortunately - to be rare that output contracts by fifteen per cent. In the past, no doubt, large fluctuations have been the rule, and in parts of the world they still are; but in well-developed economies, especially those in which the overall rate of investment might be closely regulated, such an eventuality seems very remote. We may conclude without further ado that - at any rate for the more developed economies - β must be less, perhaps much less, than 0.005. When $\beta = 1/8\%$, fluctuations in the growth rate of about 5% should occur one year in three over the long run, and fluctuations of 10% would be expected about five times a century. I suppose we should now regard an economy of which this was true as somewhat unstable today. Perhaps our judgment in these matters is warped by a tendency to exclude "exceptional" years from consideration. Though we may hope that wars and the acquisition of armaments, revolutions and strange economic policies, might be exceedingly rare in the future, we have little warrant for our optimism: it might indeed be argued plausibly that it is optimistic to suppose that the rate of growth of the output available for consumption, or investment in the capital for consumption, will differ from the expected rate of growth by ten per cent or more only five times in the next century. At any rate it scarcely seems ridiculous to assume $\beta = 1/8\%$ for many developed countries.

These arguments are very rough, and it is not reliable to guess at the likely stability of instability of growth on the basis of rough impressions. If disciplined statistical methods are to be used, the evidence must be time series, and reasonably long ones at that. We have so much (informal) information that is relevant to predicting the future (if only we could use it), that one is forced to be almost as suspicious of the mechanical processing of time series that inevitably refer to ages past, as one is of guesswork about the instability of the future age. But it is some sort of solid ground, evidence that should not be ignored. Despite the difficulties in defining and calculating the net national product, and the stock of capital at constant, depreciated prices, relations between them could be used to predict future production possibilities by means of our simple model, and β can be estimated from past time series.

By way of illustration, I have used some of the statistics for the British Economy. Figures for national income and capital stock are taken from (Mitchell and Deane 1962) , and suitably adjusted. The period of years to which they refer is 1920-38.

Figures for the national income (the net national product at factor prices) are given on p.368 of (Mitchell and Deane). They are taken from (Prest 1948), and the national income at 1900 prices is calculated by using the Ministry of Labour cost-of-living index (which is given on p.478). Naturally the figures obtained by national income statisticians, especially those for years before systematic estimation was begun, corresponds only roughly to what we should like to put into our model; but it is scarcely possible to make all desirable adjustments. It does seem worthwhile, however, to reduce Prest's figures by some estimate of that part of national income, as usually measured, that corresponds neither to consumption or investment, in the senses appropriate to our model. The category of production that most clearly fails to belong either to consumption or investment is the production that is in some way forced upon the economy by the international situation or by bad government. Expenditure on armaments or on colonial administration does not add to the stock of capital (even if it is necessary to maintain it); nor does it add to the material consumption of the people (though again it may be necessary to maintain the existing level of consumption.) Such a distinction could be extended to other expenditures, and is more plausible for some categories of government expenditure than for others. But in order to illustrate the kind of adjustment that could be made, I have subtracted from the national income figures, figures for government expenditure on "Army and Ordinance," "Navy", "Royal Air Force," and "Colonial, Consular, and Foreign" administration. Figures for all these items, referring to above-the-line expenditure, are given on pp. 398 - 400 of (Mitchell and Deane). On the assumption that this part of national income was, with the rest, reduced to constant prices by means of the Ministry of Labour cost-of-living index, this index was used to reduce these items to "1900 prices", and the result was subtracted from the Prest estimates to yield "adjusted national income."

The estimates for the stock of capital in the years 1920-38 have been produced at the Cambridge Department of Applied Economics, and are given on pp.377-8 of (Mitchell and Deane). They are made up of figures for Buildings and Civil Engineering Works, Plant, Vehicles, Ships etc.; Stocks in Trade and Work in Progress; Livestock; and Standing Timber; each being the depreciated value at "1930 prices". It is not necessary, when estimating β, to measure output and capital in the same units, so that there is no need to reduce the two sets of estimates to the same "prices". In the following table, the figures for national income, adjusted national income, and capital, are given:-

TABLE III

	National Income (Fixed Prices)	Adjusted National Income(Fixed Prices)	Capital Stock (depreciated, fixed prices)	Adjusted Output-Capital ratio
1920	2.079	1.823	9.155	0.199
21	1.804	1.673	9.306	0.180
22	1.917	1.796	9.313	0.193
23	2.011	1.941	9.339	0.208
24	2.038	1.967	9.431	0.208
1925	2.070	2.001	9.692	0.206
26	2.071	1.997	9.769	0.204
27	2.259	2.185	9.944	0.220
28	2.277	2.202	10.106	0.218
29	2.319	2.247	10.278	0.219
1930	2.294	2.219	10.409	0.213
31	2.270	2.191	10.608	0.207
32	2.271	2.193	10.636	0.206
33	2.422	2.343	10.603	0.221
34	2.504	2.411	10.764	0.224
1935	2.616	2.530	10.928	0.232
36	2.717	2.618	11.115	0.236
37	2.728	2.601	11.385	0.231
38	2.725	2.551	11.682	0.217

The unit in the first three columns is a thousand million pounds sterling.

From the final column, it appears that there is something of a trend in the output-capital ratio; but the figures would hardly support a production model $y = ake^{\alpha t}$ with α as much as 1% . It is quite plausible to assume $y = ak$ as the "normal" production function as far as these figures are concerned. If we do, the first differences of the logarithms* of the final column are independent observations of a normal random variable with mean zero and variance 2β . We can thus estimate β , and it appears that

If $y_t = ak_t e^{\varepsilon_t}$, $\beta =$ 0.13% per annum.

The rate of growth of the adjusted national income over the period is roughly $2\frac{1}{4}\%$ (allowing for the first and last observations being somewhat exceptional). Therefore if $b = \frac{1}{2}$, we may reasonably put $\alpha = 1.1\%$; and if $b = \frac{1}{4}$, it is reasonable to assume that $\alpha = 1.7\%$. It turns out that these values of α are in fact close to the average rates of growth of $\frac{\text{adjusted output}}{\sqrt{(\text{capital})}}$ and $\frac{\text{adjusted output}}{(\text{capital})^{1/4}}$, respectively. If we use these values of α as estimates of the means of $\alpha + \varepsilon$, in the two cases, β can be estimated by treating the increments of the logarithms of the adjusted output divided by the square and fourth roots of capital, respectively, as independent observations of $\alpha + \varepsilon_t$. I have found that, on the basis of the above data:-

If $y_t = ak_t^{\frac{1}{2}}e^{\alpha t + \varepsilon_t}$, $\beta =$ 0.10% per annum ;

and
If $y_t = ak_t^{\frac{1}{4}}e^{\alpha t + \varepsilon_t}$, $\beta =$ 0.09% per annum .

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* Here as elsewhere, the base of the logarithms is e .

It should perhaps be emphasised that, as has been suggested already, there is no compelling reason to choose as the form of the production function that which yields the smallest β. There are too many other sources of knowledge about production possibilities to allow us to seek merely to "explain" past production with the smallest possible residual (in some sense). These calculations certainly do not provide grounds for assuming that b should be about $\frac{1}{4}$. Their value is that they indicate the range of values of β suggested by the data. That data covers a period of some economic turbulence (a turbulence that is not, perhaps, as clear in this statistical record as in the political events of the times), but avoids the extreme fluctuations before 1920 and after 1938. While certain sources of uncertainty that one might hope to see avoided in the future are present in the period, the effects of war and rumours of war are now, no doubt, potentially of greater magnitude, and the uncertainties of programmes of deliberately accelerated investment - a matter of some importance for any theory of planning - are scarcely to be found at that time. The evidence suggests that a value of β greater than 0.1% is sensible.* There is little to support convincingly a value of β greater than 0.25% in the more developed countries.

What of the less-developed countries? Instinct suggests that a considerably greater degree of uncertainty is one of the marks distinguishing

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* Calculations on the basis of United States data provide rather larger estimates of β, ranging from 0.25% to 0.35%. The decadal data given in (Domar 1961) for 1869 - 1955 yields an estimate of 0.10%, suggesting that there is an element of periodicity, that is, a degree of self-correction, in the fluctuations, as one might expect.

underdeveloped from developed economies.* The neo-classical form of model is also less appropriate to these economies, but I shall ignore that for the time being. It is not easy to check the plausible assumption, that β should be much larger for many of the poorer nations, against hard figures. It is true that estimates of capital stock exist for some underdeveloped countries, but comparisons over time are scarcely worth attempting. However some indication of orders of magnitude is given by the fluctuations in output itself. (If we could assume that the fluctuations in capital and in output were quite uncorrelated, which we cannot, the character of ε_t would be entirely reflected in the variations of output from its normal path.) In order to obtain a very rough figure for β, we can treat the first differences of the logarithms of output as independent observations of $\alpha^* + \varepsilon_t$, where α^* is the "trend rate of growth" of output.

India is the obvious example to choose, since the statistics for India are, with all their defects, at least as good as those for any other really poor country. The official estimates of national income during the decade 1950-60 are as follows:

TABLE IV

1950-1	1951-2	1952-3	1953-4	1954-5	1955-6	1956-7	1957-8	1958-9	1959-60
88.5	91.0	94.6	100.3	102.8	104.8	110.0	108.9	116.5	117.6

* A striking example is the reported fall by 25% in the Cuban Gross national product in the four years 1959-63 (The Times, 19th. August 1963). Expectations were much influenced by the political views of the observer, but it was certainly not unreasonable to expect a significant increase in the gross national product over these years; furthermore, the reduction has certainly not taken place steadily. It should be noted however that redistribution of income, and the elimination of some highly valued, but relatively unimportant, parts of the gross national product (such as expensive hotels) may imply that the apparent fall is exaggerated from the welfare point of view.

(Unit: Rs. 1000 million at 1948-9 prices. Source: Statistical Abstract of the Indian Union ~~Union~~ 1961 (Central Statistical Organisation, Government of India), table 8.)

These figures yield an estimate of 3.2% for α^*, and 0.03% for β. However rough this method of estimating β, it is clear that the degree of uncertainty (in the sense I am concerned with) must be regarded as small on the basis of these figures. It must be said that the methods by which the national income is estimated are such as to smoothe out many of the important fluctuations*; that since 1960, fluctuations about the mean seem to have been greater; and that a time series of only nine observations is scarcely adequate for a serious estimate. On the other hand, one can fairly readily see that uncertainty about changing production possibilities is not likely to be so great in India as in many other countries. Foreign trade covers only a small part of the national income; the country is very large, so that the uncertainties for more limited regions tend to cancel out; development planning has been (relatively) steady and efficient, not adventurous or violent. It is a little surprising that the balance of payments crises and the hasty revision of the second Five Year Plan have had so little effect, but in fact only a small part of the economy was directly affected. It may be, however, that we do wrong in estimating the rate of growth from the statistics, for it might have been proper in 1958 to expect a faster rate of growth, an

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* This may be seen from (Raj 1961), which contains a discussion of national income data.

expectation that the foreign exchange difficulties falsified: yet if we assume that α^* should be 5%, the resulting estimate of β is only 0.04% . These calculations suggest that we would be unwise to assume that uncertainty about changing production possibilities is any greater in underdeveloped countries than it is in developed economies: which demonstrates the importance of distinguishing carefully between present uncertainty and uncertainty about change.

The economies in which uncertainty might be expected to be large are, generally, those for which it is not easy to get reliable statistical information. One would expect uncertainty about changes in production to arise mainly from agricultural uncertainties, caused by the interference of weather or disease; or from foreign trade, where the terms of trade may fluctuate considerably, causing large fluctuations in the value of the home consumption that production for export represents; or from variations in military expenditure, resulting from an uncertain international situation; or from political instability. This is not an exhaustive list, but it indicates where we might look for larger values of β. The case of Cuba has already been cited. Even more notorious is the experience of China, where, although foreign trade plays an extremely small part, the response of agriculture to the development drive seems to have been extremely erratic. Total production of foodcrops is given in The China Quarterly, April-June 1961, p.74 as:

TABLE V

1949	1955	1956	1957	1958	1959	1960
113.2	183.9	192.7	195.0	261.5	281.5	309.5

The unit is a million metric tons. The last figure is a rougher estimate than the others, which are official figures, and may be an overestimate.

It is doubtful whether production rose much in 1961. A meaningful estimate of β can hardly be based on these figures. An average rate of growth of 9% per annum is indicated. But even in these few years the official figures give rates of growth varying from 1% to 30%. How big β is depends on how exceptional that 30% rate of growth was, and also on whether it really happened. These figures are certainly consistent with $\beta = \frac{1}{2}\%$; it is not possible to be more definite.

It should not be thought that $\frac{1}{2}\%$ is by any means the ceiling. The records of marketing and cooperative schemes can show much greater fluctuations. The Gezira scheme in the Sudan for instance (1) has a history of enormous fluctuations both in cotton output and in cotton prices received. If the figures for net crop proceeds (net, that is, of marketing and other expenses) are taken, even for the period 1935-55 (2) after initial teething troubles had been conquered, analysis reveals values of β well in excess of 10%. Clearly the fluctuations in net crop proceeds considerably exaggerate the fluctuations in the income of the tenants; yet not so much as to bring β down into more familiar regions, it would seem, from accounts of the variations in their living standards.

When fluctuations reach these levels, however, we have to recognise the importance that spreading benefits between years may have. Not only

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(1) (Gaitskell 1959).

(2) op.cit., p.267 and the graph on p.321.

is it possible to store grain and phase the construction of social capital so that consumption fluctuates much less than production (this may not be optimal, of course), but, more important, international lending and borrowing may play a big part in smoothing out the fluctuations of a particular country, albeit at a price. This was certainly the case in the Sudan. I have carried out the analysis for a closed economy, but it is as well to remind ourselves of this possibility; while remembering that countries whose international credit is fully used may not be able to avail themselves of it.

\- 6 -

The purpose~~s~~ of these statistics is not so much to calculate the parameters of the model with any precisions for particular countries, as to find evidence for its order of magnitude. Detailed studies for particular countries, should, I think, be tied to more satisfactory production models than the simple "neo-classical" one. Having established that β is likely to lie within the range 0.05% - 0.5%, being greater than that only in exceptional circumstances, we can go on to make some numerical estimates of optimum strategies for this range, and seek to answer the question: will it make much difference if uncertainty is ignored in deriving optimal policy?

I shall consider only the cases for which $v(c) = -c^{-n}$, and $f(k) = b$. The straightforward first approximation for this case is

$$s(k,a) = \frac{ak^{b}}{n+1} - \frac{n\alpha\gamma}{(n+1)b-1}k. \tag{72}$$

$(n+1)\alpha\gamma = r + (n+1)\alpha - (n+1)(n+2)\beta$: We can tabulate the first approximation for some cases (previously included in table I) for different values of β :-

Table VII

(In all cases $r = 0$ and $k/y = 3$. The optimum investment-output ratio is entered in the table.)

b	α %	n	$\beta = 0$	$\beta = 0.05\%$	$\beta = 0.10\%$	$\beta = 0.2\%$	$\beta = 0.5\%$
1	1	1	0.470	0.474	0.479	0.488	0.525
		3	0.220	0.227	0.235	0.250	0.295
$\frac{3}{4}$	1	1	0.440	0.444	0.448	0.456	0.480
		3	0.205	0.216	0.227	0.250	0.317
	2	1	0.380	0.384	0.388	0.396	0.420
		3	0.160	0.171	0.182	0.205	0.272

Because the straightforward method of approximation is not at all reliable for $b = 1/2$ and less, I have not extended the table further. As a rough guide, we may suppose that a one or two per cent difference in the optimum investment ratio is too small to be worth worrying about. According to this criterion, the adjustment for uncertainty does not begin to be important until β reaches 0.2% , a value somewhat higher than seems to be appropriate for the developed countries. Whether it is important even then depends on whether n is large enough. These conclusions are upheld when the second perturbation is included to give the second approximation:

Table VIII.

(Again $r = 0$ and $k/y = 3$.)

b	α %	n	$\beta = 0$	$\beta = 0.05\%$	$\beta = 0.10\%$	$\beta = 0.2\%$	$\beta = 0.5\%$
1	1	1	0.47	0.47	0.48	0.49	0.54
		3	0.22	0.23	0.23	0.25	0.30
3/4	1	1	0.47	0.45	0.46	0.46	0.50
		3	0.21	0.22	0.23	0.25	0.34
	2	2	0.27	0.26	0.26	0.27	0.33
		3	0.18	0.17	0.19	0.21	0.28

(When $b = 3/4$, $\alpha = 2\%$, $n = 1$, the second approximation is not reliable; the case $n = 2$ is given instead - the second approximation is not very accurate in the second place of decimals.)

In this table, the figures give a reasonable accurate approximation to the optimal policy. While it is true that the rich, well-organised and unadventurous nation has no pressing reason to take account of uncertainty in formulating an optimal policy (although the correction for uncertainty might make a difference of up to ten per cent in the rate of investment); it is also true that a country where production uncertainty is considerable ought to pay careful attention to uncertainty, and the optimum rate of investment is likely to be very different when it is so calculated, especially if the egalitarian bias is large.

It seems very likely that the same holds true when b is smaller. In these cases, the only means of discovering the optimum policy is the improved approximation calculated earlier. For the values of α^* and k/y that interest us, this approximation seems to be rather inaccurate. I propose to calculate one example to illustrate the situation. The example suggests that the uncertainty correction may be greater when b is smaller (just as the technical-change correction is larger); we are in the regrettable position of being least able to calculate the proper correction when the correction is most significant.

The example I take is described by the following values of the parameters:

$$r = 0, \quad \alpha = 2\%, \quad \beta = 0.2\%, \quad b = 0.5, \quad n = 1, \quad k/y = 3 \ .$$

For this case modified by taking $\beta = 0$, we already know the improved first approximation: it was calculated in chapter V, and found to give:

$$\sigma = 0.29$$

(compared with the correct value of 33%.)

If formulae (82) - (85) are used to calculate the improved approximation, it is found that

$$\sigma = 0.37$$

(It is clear from the innards of the calculation that the series R used in calculating the integrand converges slowly even with β/α as small as 0.1 : one cannot therefore put a great deal of trust in the result.)

These values for σ suggest that even when β is apparently very small, the correction for uncertainty may be significant. In the present case, our best guess at the correction is an upward adjustment of 8% of the national product over a no-uncertainty optimum investment ratio of 33%. Calculation of other examples confirms this as an order of magnitude.

When n is larger, we can expect the uncertainty-correction to be larger. This was apparent from the straightforward approximations, and it is easy to check that the same is true now. It is also apparent that the approximations are less accurate when n is larger. When β/α is larger - say of the order of 1/4 , the evaluation of the improved approximation would need to be much refined before usable results were produced. The considerable computational labour required would seem to be justified only for more immediately practical purposes.

The conclusions from our empirical investigations are these:-

Uncertainty can vary considerably from country to country. While there is a tendency for more developed countries to be less uncertain about changes in production possibilities, this is not necessarily the case; for uncertainty depends upon weather conditions, the position in international trade, political stability, defence commitments, and many other factors, as well as on the state of technological development. The effect of uncertainty in modifying the optimal policy is greater for greater n and for smaller b. It is insignificant for the ordinary developed country when b is as large as $3/4$; but there is some evidence to suggest that a developed country facing moderate uncertainty ought to increase its optimum rate of investment significantly when b is $1/2$ or less, especially if n is rather large. The position when uncertainty is great but b is small is obscure. It must be remembered that the effect of uncertainty on the pure static case is to decrease the investment ratio when β is great enough: it is possible that the effect of uncertainty decreases as β increases beyond some point.

I conclude that the case for further investigation is strong; but that one may not be seriously wrong if one neglects uncertainty in computing the optimal policy for an economy where future production possibilities can be predicted with great assurance (apart from the uncertainty inherited from imperfect knowledge about present production possibilities.) Countries in the early stages of development, in the process of beginning serious economic planning, for instance, probably should not neglect the importance of uncertainty. The existence of uncertainty strengthens the case for attempting to increase the rate of investment beyond what is found acceptable to established economic interests.

The proposition that the acknowledgment of uncertainty increases the optimum rate of investment is new, and in some degree, surprising. In arguing that there is a <u>prima facie</u> case for a government to encourage saving because people show an irrational time preference, Pigou says, "Nobody, of course, holds that the State should force its citizens to act as though so much objective wealth now and in the future were of exactly equal importance. In view of the uncertainty of productive developments,..., this would not, even in extremest theory, be sound policy." (Pigou 1952), p.29,). This must be read, I think, to mean that uncertainty about future production weakens the case for more investment. This is widely believed. Economists are not usually very precise about the kind of uncertainty involved. Here I have distinguished present uncertainty, and uncertainty about changes. The first is seen to be virtually irrelevant; the second works in the opposite direction to popular prejudice. But this is not surprising if one thinks about it. Normally an appreciable proportion of that part of a man's income that he chooses not to spend on current consumption is spent on insurance, and this may be a considerable motive in determining his investment in monetary assets. It is quite reasonable, therefore, that a nation's investment should be regarded as having an element of insurance in it - that is, insurance against the possibility of production being less than is hoped.(1) The uncertainty that provides a reason for investing less than otherwise seems desirable is valuation uncertainty - and even that, we have seen, is ambiguous.

It must be said that it is very difficult to turn the notion of insurance into a rigorous argument that uncertainty about changes in production possibilities requires extra investment. The difficulty is that future consumption (which is what is valued) is related to future

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(1) Knight recognised this in a slightly different context. ((Knight 1921), p.132, footnote.)

production through the rate of investment - which has to be determined. I doubt whether it is worth trying to reduce the mathematical argument to literary form, beyond indicating why the conclusion arises. Some of the practical policy arguments that spring to mind when the proposition "uncertainty means more investment" is enunciated really depend on more general or slightly different arguments, where the form of investment chosen depends on the uncertainty expected, and may even have a direct effect on that uncertainty. Examples will be given later.

A more or less thorough examination of the simplest relevant planning model has proved complicated and extensive. More realistic models will no doubt be more complicated and extensive in their working out. In the next chapter, I propose simply to indicate some of the more obvious ways in which the methods and conclusions of this and the previous three chapters could be extended, and say something about the major questions that have been neglected by the definition of the model.

CHAPTER VII EXTENSIONS AND DEVELOPMENTS

- 1 -

A number of modifications can be made in the model more or less directly: the methods expounded earlier are adequate to deal with them. In this section, I shall deal with two of the simplest of them: in the first, the production function, $f(.)$, is assumed to be bounded; in the second the parameters are assumed to depend on capital, investment, and on time.

It was remarked in chapter IV that the fundamental equation in its differential form is valid without change when $f(.)$ is bounded. Denote the upper bound of f by $\bar{f}$, the minimum value of k for which it is attained being $\bar{k}$ $(\leqq \infty)$. We can write the fundamental equation in the form:

$$\frac{d}{dk}(v(c) + sv'(c)) = - D\, v'(c). \qquad (1)$$

If this is integrated between k and $\bar{k}$, we find that

$$v(c) + sv'(c) = v(a\bar{f}) + D\int_{k}^{\bar{k}} v'(af(x) - s(x,a))dx. \qquad (2)$$

The straightforward approximation process can be applied to this, starting from the static solution for the bounded production case, i.e., the solution of

$$v + sv' = v(a\bar{f}).$$

This solution is very close to the solution of $s + sv' = 0$, when $a\bar{f}$ is large in relation to current production. For example, if $n = 2$

and $a\bar{f}$ is ten times current production, the investment ratio would have to be adjusted by only 2/3% of itself, which is certainly negligible and likely to remain so when the policy is corrected for dynamic conditions. If $n = 1$, the adjustment might not be negligible but would usually be small.

It seems to me difficult to assign a maximum level to present production, and production at all future times. Not only that, but it seems likely to be an unrealistic assumption: for in time, little more production would be available by investing more, and the optimum rate of investment would fall towards zero. That is an odd assumption to make without evidence. I therefore regard the possibility of calculating the optimal policy under these conditions as of only academic interest.

Rather more important is the possibility that the dynamic parameters are not constant.(1) In deriving the fundamental equation, the following equation was derived:

$$\operatorname*{Sup}_{s}\left(v(c) + s\frac{\partial V}{\partial k} + DV\right) = 0 \quad . \tag{3}$$

If the process by which this was derived is carefully checked, it will be found that the equation remains valid when r, α, and β depend on a, k, and $\dot{k}$. If, however, any of them depends on t, V will depend on t, and the equation must be modified by inserting $+\frac{\partial V}{\partial t}$ inside the brackets.

When the parameters depend only on a and k, the next step -

(1) When they are not constant, r, α, and β, are the discount, technical progress, and uncertainty <u>densities</u>; so that the rate of discount is $\int_0^t r(\tau)d\tau$; the projection factor for production is $e^{\int_0^t \alpha(\tau)d\tau}$; and the variance of ε_t is $\int_0^t \beta(\tau)d\tau$.

differentiation with respect to s - presents no difficulty, and so the fundamental equation, in its integral form, remains valid. The differential form remains valid when the parameters depend only on a. The straightforward approximation process raises few difficulties: the qualitative results for the less general case would seem to hold still. Whether a model in which the parameters depend on a and k is plausible is difficult to say. Dependence on a is awkward to deal with: if α depends on a, one may easily find that a explodes over time, tending to infinity for some finite t. On the other hand, the equations remain valid for any time-path of a, for any time path can be represented in the form

$$a_t = a_o e^{\int_0^t \alpha(a_\tau) d\tau}.$$

Similarly, they remain valid for any time-path of the variance density of the stochastic process. Nevertheless, it is difficult to think of time-paths for the rate of technical change and rate of variance more plausible than the constant α and β used in earlier chapters. Similarly it was argued in chapter II that the most sensible assumption for r is constancy.

As for dependence on k, one might argue that technical progress will be less when the stock of capital becomes very great. Equally, one might argue that the wealth of opportunities for new discoveries provided by the highly complex capital of the future argues that the rate of technical change increases with increasing capital. Again agnosticism is at present the only possible position. In the long run, uncertainty may very well fall as capital increases: this really does seem a sensible assumption, that the rate of increase of uncertainty should fall as investment proceeds. Since the straightforward approximation process remains the same, we may take it that the uncertainty correction would fall over time. The qualitative conclusion that uncertainty increases the optimum rate of investment is unchanged. To a first approximation, the dependence of β on k does not affect matters.

Direct dependence on t is not very plausible, in the sense that it would be difficult to find evidence for any particular assumptions. The principles of the straightforward approximation process can be used.

Dependence on the rate of investment is particularly interesting; but it is difficult to specify the likely direction of this dependence in general. A greater rate of investment in a steady well-organized economy may bring greater uncertainty; in an underdeveloped country, it might bring a reduction. Again, it is frequently argued that the rate of technical change should be made to depend on the rate of investment, to a first approximation. Most who suggest this believe that faster investment increases the rate of technical change because investment-good industries are particularly good growth points for innovation, because a higher rate of investment means the faster introduction of new equipment, because a more rapidly growing economy encourages a more "dynamic outlook". Who knows? It could be argued that faster investment concentrates attention on the use of present techniques rather than on fundamental discoveries (but not very plausibly.) There is certainly something to be said for a production function of the form:

$$y = af(k)e^{\int_0^t \alpha(\dot{k}_\tau / k_\tau)\,d\tau} .$$

Obviously it is very difficult to give any empirical form to such a function. Nevertheless it is of interest to see what can be done about determining the optimum policy for such cases.

We must differentiate (3) with respect to s. r, α, and β depend on s. Let their derivatives with respect to s be r', α', and β'. Assume that these derivatives are small of the order of r, α, and β. We have

$$v'(c) = \frac{\partial V}{\partial k} + \left(\alpha' a \frac{\partial}{\partial a} + \beta' a^2 \frac{\partial^2}{\partial a^2} - r'\right)V. \qquad (4)$$

This has to be solved in conjunction with:

$$v(c) + s\frac{\partial V}{\partial k} + DV = 0 . \qquad (5)$$

Simultaneous partial differential equations are, to say the least, awkward. If we concentrate on getting a first approximation to the solution, this is not too difficult. We assume that s and V differ from the optimal policy and maximal valuation for the static case by small quantities, and solve for these small quantities. To this order of approximation, we can substitute for $\frac{\partial V}{\partial k}$ in (5) the expression we get from (4), and for V, the expression we get by integrating (4) with respect to k. Then we find to first order that

$$v + sv' = (D - sD')\int_k^{\infty} v'(af(x) - s(x,a))dx;$$

that is, the ordinary form of the fundamental equation with r, α, and β replaced by $r - sr'$, $\alpha - s\alpha'$, $\beta - s\beta'$, respectively. To the first order, s may be replaced by θ, the static optimal policy.

Thus, roughly speaking, when one of the parameters increases with the rate of investment, its effect on the optimal policy is reduced. If the rate of technical change is increased by more rapid investment, then - as one would expect - the optimal rate of investment should be greater than otherwise. That is a conclusion that those who believe the rate of technical change is positively correlated with the rate of investment would want to draw. It also follows that the rate of investment should be yet greater if faster investment tends to reduce uncertainty, but reduced if faster investment makes uncertainty increase faster. All this is as one would expect.

\- 2 -

We must turn to the more awkward discussion of the particular way in which uncertainty has been introduced. The particular projection transformation embodied in the model is meant to capture an important feature of the world we know, the fact that our uncertainty about future possibilities increases as their distance from us increases. That this is so will not be disputed, nor will it be doubted that just this fact is the motive for introducing time-horizons, discount-rates and similar devices in previous discussions of planning problems. The particular assumptions I have made are certainly very simplified, however. I have already mentioned that there is considerable auto-correlation in economic time-series: we should like to be able to assume the same of the stochastic process in the model. The way in which all the uncertainty is put into a multiplicative factor while the form of the production function remains fixed is also open to objection. Finally, there must be a suspicion that a more realistic treatment of statistics, whereby the parameters of the model are adjusted in the light of the actual data thrown up by the economy's working, not established at the beginning of the planning process in a flash of inspiration, would make some difference to the results. I want to consider these possibilities in turn.

Auto-correlation may be effectively represented by rather a simple model, by assuming that the variable a is the "normal" coefficient of the production function, the actual coefficient at any time being a value of a random variable with mean a, say ae^{ζ}. The ζ for different times are independent. Planners "observe" both the normal and the actual production coefficient. The optimal policy and the maximal valuation depend on the current value of ζ as well as on a and k. However, because the distribution of ζ_t is independent of t, it is quite apparent in the derivation of the fundamental equation that ζ does not appear except in the argument of v. In other words optimal policy is independent of ζ, provided that a is interpreted as the normal production coefficient, except that the production function is to be $e^{\zeta} f(.)$.

The effect of introducing auto-correlation into the model in this way is to reduce the appropriate value of β (since some of the actual uncertainty is incorporated in the new random variable ζ). One accepts the normal production coefficient as the actual one, and the then the apparent production function as the actual one. This amounts to treating the apparent situation as the actual one, while assuming a smaller value of β than the evidence would otherwise have suggested. The optimum investment/output ratio is consequently smaller than it would otherwise have been.

It is natural to ask what should be done when the production function itself is subject to random variations. What if b in the function $f(k) = k^b$ were a random variable? In principle, exactly the same techniques can be applied. Assume for example that $\log b$ is a Brownian process. The optimal policy and the maximal valuation depend on b now. We have to redefine the operator D as:

$$D = -r + \alpha a \frac{\partial}{\partial a} + \beta a^2 \frac{\partial^2}{\partial a^2} + \alpha^* b \frac{\partial}{\partial b} + \beta^* b^2 \frac{\partial^2}{\partial b^2}$$

where $be^{\alpha^* t}$ is the mean of the random variable b, and $2\beta^*$ is the variance of $\log b$. The rest of the theory would be more complicated, but in principle no more difficult than for the simple model. It will be clear now that the model is capable of considerable generalization since the technique of deriving the fundamental equation can be applied to almost any reasonable projection transformation. It may be remarked that there seems to be no reason to alter the earlier conclusions that technical change implies a smaller optimum rate of investment while uncertainty implies a larger one, when the model is generalized by placing some of the dynamics on b.

On the face of it the possibility of introducing statistical techniques directly into the model presents enormous difficulties. By analogy with the theory of control systems, we may call the case, the case of real-time statistics. The parameters of the stochastic process are now functions of the time-series of past production coefficients. Thus the optimal policy (and the maximal valuation) depend on this past time-series. They depend on it, however, through the dynamic parameters, and that makes the case easier to handle. In deriving the fundamental equation, the problem is to evaluate

$$\left[\frac{d}{dt} E(V(a_t, k_t, \alpha_t, \beta_t))\right]_{t=0} \qquad (6)$$

Looking forward, the planners must allow for the change in the model parameters, α and β, induced by the changing production coefficient (the change in which can be predicted by means of the current estimates of the parameters: if the estimators are consistent, this will not lead to inconsistency between possible predictions of future predictions.) In general, it appears that α and β can be dropped out of (6), so that the problem reduces to the one previously considered. Suppose, for example, that β is estimated by summing the squares of past changes in the logarithm of the production coefficient. If the known time series starts at some time T, we can write (forgetting problems about meaning!)

$$(\mathrm{Est}(\beta))_t = \frac{1}{t+T} \int_{-T}^{t} \frac{(d\mathcal{E}_t)^2}{dt} . dt ,$$

the expression on the right denoting the estimate of β that will be used at t. Naturally the rate of change of the expectation of the estimate is zero. We must ask what the rate of change of the variance of the estimator is. The variance can be seen to be $\frac{\beta^2 t^2}{(t+T)^2}$. The derivative of that with respect to time tends to zero as t tends to zero. The same is true of higher derivatives. Consequently the usual procedure for evaluating (6) yields no terms in the derivatives with respect to α and β. We need not, I think, trouble ourselves about using real-time statistics.

\- 3 -

There are certain matters connected with production that deserve our attention. The most serious omissions in the model are that it ignores lags, the initial transition to an optimal state, and the problems of many products and a realistic capital structure. These features are left out because they are so difficult to deal with. Such problems cannot be solved in a few pages. It is worth saying a little about them however.

Lags are a less serious omission than may appear. Suppose we introduced a lag between capital and production. Then it would be natural to classify capital into idle and active capital. Any investment would result in a net change in both idle and active capital. If, for example, there were a lag of a year between the establishment of capital and its use, the quantity of idle capital would be roughly a year's investment. The rate of change of idle capital would be the second derivative of the stock of capital. That is to say, a part of output equal to the rate of change of the rate of investment must be put aside each year to increase idle capital, the rest being divided between active capital and consumption. If the rate of growth of output is to be 6%, the investment ratio 20%, the part of output to be put aside for idle capital amounts to less than $1\frac{1}{2}\%$ of output, which we may take to be too small to matter much. If the optimum policy is calculated for the no lag case, a single iteration could be done in this manner to revise the production function, and thus obtain a new estimate for the optimum investment policy, sufficiently adjusted for the presence of the lag. As the effect of the lag is to reduce effective production, its influence takes the form of reducing the proportion of consumption in output, and therefore of increasing the optimum investment ratio a little.

When one begins to think about the period of transition to the optimal policy, one comes up against the admittedly unreal assumption that adjustments in capital stock can be made as quickly as one wants. In an actual economy, an increase in the rate of investment - if it is to be properly designed for the output it is meant to produce - requires a somewhat prolonged period of adjustment during which the heavy machinery industries are expanded and reorganised. This feature has led many to prefer production models in which the economy is divided into an investment and a consumption sector between which capital cannot be moved, so that a change in the proportions between the two sectors must depend on changing the proportion in which investment is allocated between the two sectors. Even this type of model hardly does full justice to the complex process of transition. I suspect that in practice one would do as well to change the rate of investment as fast as is feasible towards the optimum rate, and forget about the lapse from optimality implied by ad hoc methods of transition, as to formulate a really satisfactory model and then attempt the calculation.

An apparent advantage of the two sector model is that it seems to provide a more natural way of comparing the rate of investment with the rate of production of consumer goods in terms of the numbers of workers in the two sectors, provided that one is prepared to assume either that the "same technique" is used in both sectors, or that one sector is uncapitalized. It would be perfectly possible to build up a theory of such a model, analogously to the theory presented in this thesis; but it would, I think, be more difficult to apply quantitatively to actual countries. For all its weaknesses, the standard index of capital is something that we can put a figure to.

It is particularly unfortunate that no theory of replacement is incorporated in the theory as presented. Since output is not a single-valued function of depreciated capital, it is desirable that net investment be the difference between gross investment and replacement, defined in some manner to be the amount of investment needed to maintain production. However, replacement is subject to choice, not entirely determined by wind, weather and the worker. One may very properly ask what is the optimum rate of replacement. But then the optimum decision depends upon the age structure and productivity structure of existing capital. One has to consider the optimal allocation of labour. Presumably computational techniques could be developed for a discrete time model incorporating this feature, but it seems to be extremely difficult to discover anything about general features. All one can say is that a net investment theory is some sort of first approximation to optimal policy. <u>If</u> the rate of replacement were predetermined by the total quantity of existing capital, it would give a reasonably correct result. Not knowing what an optimal replacement policy would be, we may reasonably relate actual rates of replacement to the accumulated net investment (despite the lack of economic logic to any such relation) and proceed from there.

Similar to the problem of capital, and indeed a generalization of it, is the problem of a multi-product economy. To ask for a solution for a multi-product economy is to ask for the solution of a much more complicated economy. Again, so long as the number of relevant variables is finite, there should be no problem in developing a set of fundamental equations for the optimal policy in the manner of chapter IV: but the mathematical manipulations would then be very complicated. An interesting possibility is that we could find the asymptotic form of the optimal policy as time tended to infinity, at any rate for particular cases, just as was done for the simple case. From there, one might be able to go on to establish good numerical techniques. It is a possibility I have not been able to explore.

\- 4 -

Population has been rigorously excluded from this thesis, except when discussing the maximum consumption theorems. There are two respects in which I want to comment upon it here, the general question of population growth that cannot immediately be controlled, and the possible existence of unemployment that cannot be at once absorbed, as in a number of underdeveloped countries.

It was shown, when discussing the possibility of exponential population growth in the context of the maximum consumption theorems, that such growth could be incorporated into a constant-elasticity model by diminishing the rate of discount and augmenting the rate of technical change. It was seen that a rate of population growth equal to the rate of technical change could increase the asymptotic investment ratio by 50% (1). We ought to be able to assume that such population growth rates will not continue for

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(1) The asymptotic investment ratio is

$$\frac{b}{(n+1)\left(1-\frac{(1-b)\nu}{\alpha+c\nu}\right)},$$

where c is the production exponent of labour, ν the rate of growth of population. If $b = c = 1/2$, we get the result mentioned in the text.

any length of time. It is clear that population growth provides a reason for increasing the rate of investment, however. There is no difficulty in principle about constructing a model that takes account of a more complex population growth function than the exponential one. As I have no idea what would be an appropriate form of the function, I have not done so. It would also be of great interest to analyse the effect of uncertainty about population changes, which are in practice rather large. It would appear that the uncertainty would augment the uncertainty in the rate of technical change. There seems to be no reason to modify earlier conclusions about the direction of the effect of uncertainty on the optimum policy.

Properly speaking, it seems somewhat unlikely that it is ever optimal to have unemployment, although it is possible that the existing capital structure, and the economies arising from continuous working by each person, would lead to such a situation in some places. However the combination of a number of factors, of which the chief are the difficulties of controlling the labour market, it seems to be impossible to implement the fully optimal policy in some countries, such as India. In these countries, an appropriate model of the economy to use for deriving a second-best policy would seem to be one in which the industrial real wage is assumed to have a floor, while the residue of the labour force works in agriculture, where the marginal product of labour is very low in the early stages of development, In such an economy, the people are obviously divided into different consumption classes.[1] The valuation function must be specially constructed with that in mind. As a first approximation, a function additive by persons suggests itself. The process of development would divide itself into stages. In the first, there would be a gap between the urban wage rate and the rural marginal product of labour (resulting in disguised, and even open, unemployment). Later the two would be equalized, and finally the real wage in the cities would begin to rise. Only in this last phase would the model of earlier chapters apply. The solution for that case is obviously a prerequisite for the solution of the new problem.

It will be apparent that there are no striking difficulties about calculating the optimal policy in such a case, although the complexity of the equations is inevitably much increased. The conclusions are much as one would expect. The technique in the urban sector (that is, the proportion of capital to labour) is greater the less capital there is available, less when technical change can be confidently expected, greater when there is uncertainty. A full working out of the case must be done elsewhere. Here it is only necessary to indicate that it is possible by means of the techniques developed in this thesis.

It must be remarked, however, that such a theory would by no means

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(1) It is sometimes suggested that as a first approximation, consumption is distributed fairly among the whole population, despite the differential wage structure. This is certainly not so in India, and it is difficult to believe that it is anywhere the case. However the practice of remitting part of the income from industrial work to the cultivating family at home (as in many parts of Africa, and, to some extent, in India) has an ameliorative effect - and makes it very difficult to know how real consumption is distributed in some of these countries.

give a fully adequate account of the optimal investment policy. In chapter I, I discussed the importance of facilities. In chapter III, I discussed the possibility that production would depend on consumption; and it also depends upon the facilities established. These considerations suggest a much more complex model, in which the distinction between consumption and investment would be blurred. My impression is that the importance of investment would be much enhanced. Especially in underdeveloped countries, there are many examples of investments that would seem to have effects much beyond what is allowed for in my model: for example, malaria eradication and dysentery prevention programmes, housing, education. The list seems endless. To some extent, one can take these factors as exogenous decisions, and then the basic model is very useful. But the danger of ad hoc decision in such matters is not to be under-estimated. These problems offer a fruitful field for further enquiry.

\- 5 -

Finally, let me return to my first assumption, that the economy is closed. That it is not is very important, and in two ways, that it is hardly fruitful to attempt to incorporate in an economic model, but which I cannot forbear to emphasize.

In the first place, historically, programmes of accelerated investment have been the result, most frequently, of a concern with national prestige and economic - that is, military - power. I believe that today's patterns of development are still influenced very deeply by the traditions of this aim, as well as by its conscious importance in the present. If this is the aim, it is not true to say that economists have nothing to say. But the issues can be put more simply and more plainly. I cannot but regard the necessity for such programmes as unfortunate, the deliberate pursuit of them without provocation as immoral. On the subject, I have no more to say.

In the second place, the fact of an economy's not being alone means, not only that it is subject to external influences that one could otherwise neglect, but that it is subject to moral claims that did not appear in the analysis before. If we have to balance the claims of Indian peasants against the claims of our descendants, yet richer than ourselves, how shall we do it? Can we pretend that anything we actually do can be optimal in that light? We are not choosing simply between consumption for ourselves, and investment for our own people: there is a third party to consider, the consumption and investment of others much more deserving than ourselves. It would not be right to end a theoretical discussion of the problem of optimum investment, without drawing attention to this overriding moral issue.

CHAPTER VIII CONCLUSIONS

All the mathematics reduces to a few relatively straightforward conclusions. In this final chapter, I shall draw them together, and ask how they should influence the way we look at optimal development policy. It has been my purpose to show how one can do relevant welfare economics for an economy whose structure is changing in a way only imperfectly known. A great deal of economic discussion, especially that dealing with general issues of policy, but not excluding the most recondite theory, is really concerned with problems of optimal development, usually narrowed down to the single problem of the optimum rate of investment. I have felt rather often that the discussion suffers either from a dogmatic belief that everyone knows what the optimum rate of investment is, or from an insufficiently thorough examination of the moral issues involved. The study of the problem has at any rate freed me from the first fault, although I cannot hope that the discussion of the ethical issues is deep enough to satisfy others completely. I think that the study of the optimum investment problem has shown the broad contours of optimal development paths, and helps us to judge the importance of various factors in determining it; it also gives some impression of the sort of difficulties that are likely to present themselves in more complex models. I shall deal with these three sets of conclusions in turn.

On examining the case of technical change without uncertainty, it was found that the optimum rate of investment is less than for the static case (in which it is assumed that there is no technical change.) A study of the particular case $f(k) = k^b$, $v(c) = - c^{-n}$ turned on the fact that, in this case, the optimal investment ratio is a function of the capital-output ratio times the rate of technical change. It was found that the capital-output ratio and the optimum rate of investment tend to equilibrium values as time tends to infinity. If the capital- output ratio begins below the equilibrium value, the rate of growth should be greater than the equilibrium rate of growth until the capital-output ratio increases to the equilibrium value. If, for some reason, the economy had accumulated capital to the point where the capital-output ratio was greater than the equilibrium value before optimal development began, it should have a rate of growth less than the equilibrium rate of growth until the capital-output ratio has fallen to the equilibrium value. Asymptotically, the economy will follow a steady growth path.

There seems to be no reason to doubt that this picture of optimal development is generally true; but, no doubt, for special forms of v and f , optimal development might take the economy towards an infinite capital-output ratio (it does so if $f = k$ and $\alpha > 0$) or even towards no limit at all.

When there is uncertainty, there is no unique development path, but it would seem that the mean development path has roughly the characteristics just described. (This is especially likely when, as often happens, the importance of uncertainty for the optimal policy is negligible.) A rate of discount does not alter the character of the development path, although it alters the asymptotic values of the rate of investment and the capital-output ratio.

The actual value of the asymptotic investment ratio proves to be surprisingly small. For example, when $n = 3$, $b = 1/4$, it is 1/16, which is even smaller than actual rates of investment. We would not usually choose b so small; most would prefer n smaller. But on balance, it appears that actual rates of investment are not unjustifiably low under all circumstances.

We can list the different factors that influence the optimum rate of investment, roughly in order of importance:

(1) The production assumptions. These are crucial. The optimum rate of investment is very sensitive to the form of f for realistic values of α. It is usually fairly sensitive to the particular value of α. The dependence was illustrated for the constant-elasticity case by the curves presented in chapter V. It is quite clear that the investment ratio is considerably larger when capital productivity is larger.

(2) The valuation assumptions. The optimum policy is very sensitive to the valuation assumptions. It was shown that in the constant elasticity case, the difference it made to the optimum investment ratio whether n was 1 or 3 varied from 16% of output when $b = 1/4$ to 24% of output when $b = 3/4$. This great sensitivity is accompanied by considerable difficulty in deciding upon the appropriate valuation function. A thought-experiment for deciding n was suggested in chapter III; but it cannot be pretended that it is easy to apply.

(3) The capital-output ratio. The optimum investment ratio falls as the capital-output ratio increases (for the usual values of the parameters.) In most cases, it falls quite rapidly; but for $b \geqq 1/2$ a doubling of the capital-output ratio would not usually change the optimum investment

ratio by more than 10% of output.

(4) Time-preference. It is argued that this should be zero. If it is not zero, its effect is less than that of the same value for the rate of technical change. Its importance decreases as n increases.

(5) Uncertainty about future existence and future values. Again, it is argued that empirically these do not justify very high rates of valuation-discount. Thus like time-preference, it should not be important.

(6) Uncertainty about changes in production possibilities. This kind of uncertainty requires larger optimum rate of investment than in its absence, at any rate in all cases that are accessible to numerical estimation. In cases where capital is relatively productive (that is, $kf'(k)/f(k)$ is relatively large) countries with relatively low uncertainty need pay little attention to this phenomenon in formulating optimal policies. However there are countries - usually underdeveloped ones - where the effect of this uncertainty is important. In these cases it is often difficult to estimate the uncertainty correction accurately: if capital productivity is high enough, it is quite possible. The effect of uncertainty is greater when the egalitarian bias is greater. It is unlikely that uncertainty and the egalitarian bias would ever be large enough to counteract the effect of technical change and raise the optimum investment ratio to the level of the static optimum, the case solved by Ramsey.[1]

(1) The theorem that uncertainty about changes in production requires an increase in the optimum rate of investment might mean, to take an example from a mainly agricultural economy, that a greater part of agricultural production should be exported in order to buy fertilizer factories, not only because in increase in production is worth investing for, but because it is worth doing additional investment in order to make years of low production, when many are close to starvation, less frequent. This should not be confused with the even stronger argument that it is a good thing to invest in, say, stand-by capital, such as additional irrigation ditches, wells, and implements, so that the economy may be less sensitive to fluctuations in natural conditions,

(1) Continued:

being better equipped to deal with emergencies.

It is clear that any particular investment can influence the degree of uncertainty, and these differences are relevant in choosing the right projects. Neglect of uncertainty might lead to an investment programme that fails to be optimal, not because it is too small, but because it consists of projects that do too little to reduce uncertainty. On a macro-economic level, this is reflected in the model, discussed in the previous chapter, in which β depends on the rate of investment. However, the proper choice of individual projects raises many questions that do not appear at a macro-economic level of analysis.

(7) Present uncertainty about production possibilities. The effect of this may be either way, but it may reasonably be neglected in most cases.

It is clear that the influence of technical change is quantitatively the important part of this study. On the other hand, uncertainty is a virtually neglected problem. It is uncertainty that causes the greatest difficulties in analysis, and it has not been possible to push the analysis of the model with uncertainty as far as for the model in which there is no uncertainty. Almost certainly this pattern would be repeated by studies of more complex models. But numerical problems can almost always be solved. In the present case, numerical methods could be developed by analysing a discrete time model. Such methods smack of the blunt instrument. Yet once it is established that the methods of this thesis, or something like them, are the proper methods for determining optimal development policies, it is this avenue that seems likely to provide the most direct way of answering the questions of planning in numerical terms.

The conclusion of this study that I find most disquieting is the sensitivity of the optimal development policy to the valuation function used. This fact, of course, warns us against any too facile way of expressing the nature of the choices involved in development decisions; for where the results are so sensitive to values, it is very dangerous to present the issues in a misleading way. Must we conclude simply that finding an optimal policy is an impossible business, that man is not yet in a position to act rationally? I am not sure. But I think that once the form of the issues is presented, we may begin to express our values more precisely. I hope my conclusion is not simply that it is very difficult to decide.

Such a conclusion appears so often in the economic literature that one must wonder whether the subject has reached its decadence. There is no merit in not being able to decide; even if it is better than deciding wrongly. I believe that the methods I have presented provide a means for decision. Against calculated optimal policies, economies can be judged, and changed. If we do not want to do it the right way, why should we want to do it at all?

APPENDIX: THREE THEOREMS.

Theorem 1. *If* $\dot{K}_t = i(K_t, ae^{\alpha t+\varepsilon_t})$, $K_o = k_o$, *given;*

and $E[\int_o^\infty v(ae^{\alpha t+\varepsilon_t}f(K_t) - i(K_t, ae^{\alpha t+\varepsilon_t})).e^{-rt}dt]$

attains its maximum, V, *when the function* $i = s(k,a)$; *then*

$$u.af'(k) + s.\frac{\partial u}{\partial k} + Du = 0,$$

where $u = u(k,a) = v'(af(k) - s(k,a))$, *and*

$$D = -r + \alpha a\frac{\partial}{\partial a} + \beta a^2\frac{\partial^2}{\partial a^2}.$$

(ε_t is the random variable defined in chapter III.)

In the proof, I shall require the following simple lemma:

Lemma. *Let* $[X_t]_o^\infty$ *be a stochastic process with finite moments, such that* $Y_t = \int_t^\infty X_\tau d\tau$ *is infinite on at most a set of probability measure zero,* and has finite expectation. *Then*

$$\frac{d}{dt}EY_t = -E(X_t).$$

We invert the order of the expectation and integration operators:

$$E[\int_t^\infty X_\tau d\tau] = \int_t^\infty E(X_\tau).d\tau.$$

This is justified by Fubini's Theorem. Differentiation proves the lemma.

<u>Proof of Theorem 1.</u> Consider the investment strategy defined at time t by $i = s(k,a) + \lambda s^{o}_{t}$ for any function of time s^{o}. The valuation for it, starting at time T, is

$$V^{\lambda} = E[\int_{T}^{\infty} v[ae^{\alpha t+\varepsilon_t}f(K_t) - s(\hat{K}_t, ae^{\alpha t+\varepsilon_t}) - \lambda s^{o}_{t}].e^{-rt}dt;$$

where K_t is given by

$$\dot{K}_t = s(\hat{K}_t, ae^{\alpha t+\varepsilon_t}) + \lambda s^{o}_{t},$$

and $\hat{K}_t$ is the capital path for the optimal policy. The derivative of V^{λ} with respect to λ exists. It must be zero when $\lambda = 0$:

$$E[\int_{T}^{\infty} v'.\left\{ae^{\alpha t+\varepsilon_t}f'(\hat{K}_t)\frac{\partial K_t}{\partial \lambda}\bigg|_{\lambda=0} - s^{o}_{t}\right\}e^{-rt}.dt] = 0.$$

Clearly $K_t = \hat{K}_t + \lambda\int_{0}^{t} s^{o}_{\tau}.d\tau$. Hence $\frac{\partial K_t}{\partial \lambda} = \int_{0}^{t} s^{o}_{\tau}.d\tau$. We can invert the order of integration with respect to t and τ, to derive the equation,

$$E[\int_{T}^{\infty} s^{o}_{t}\left\{\int_{t}^{\infty} ae^{\alpha\tau+\varepsilon_\tau}f'(\hat{K}_\tau).v'(C_\tau).e^{-r\tau}d\tau - e^{-rt}v'\right\}dt] = 0.$$

If the lemma is applied, we can differentiate with respect to T, and cancel the factor s^{o}_{T}:

$$E[\int_{T}^{\infty} ae^{\alpha\tau+\varepsilon_\tau}f'(K_\tau)v'(C_\tau)e^{-r\tau}d\tau - e^{-rT}v'(C_T)] = 0.$$

Differentiating with respect to T again, and once more making use of the lemma, it is found that

$$a e^{\alpha T} E[e^{\varepsilon_T} f'(\hat{K}_T) v'(C_T)] - r E[v'(C_T)] + \frac{d}{dT} E[v'(C_T)] = 0.$$

I shall put $T = 0$ in this equation. Then the only problem remaining is to evaluate the last term. Let us notice first that

$$\frac{d}{dT} E[v'(C_T)]\Big|_{T=0} = s_0 \frac{\partial v'}{\partial k} + \frac{\partial}{\partial T} E[v'(C_T)]\Big|_{T=0}, \quad \text{where } s_0 \text{ is}$$

the optimum rate of investment at time 0, viz. $s(k,a)$.

If ϕ is some arbitrary function,

$$E[\phi(\varepsilon_t)] = \frac{1}{\sqrt{4\pi\beta t}} \int_{-\infty}^{\infty} \phi(x) . e^{-\frac{(x+\beta t)^2}{4\beta t}} dx .$$

It is a straightforward exercise to evaluate the derivative with respect to t. It is found that the derivative is

$$\beta E[\phi''(\varepsilon_t) - \phi'(\varepsilon_t)]$$

It is well known that for any well-behaved function ϕ, the limit of $E[\phi(\varepsilon_t)]$ as t tends to 0 is $\phi(0)$. Thus the derivative tends to $\beta[\phi''(0) - \phi'(0)]$ as t tends to 0. Using this fact, it is easy to check that the limit of $\frac{\partial}{\partial T} E[v'(C_T)]$ as T tends to zero is

$$[\alpha a \frac{\partial}{\partial a} + \beta a^2 \frac{\partial^2}{\partial a^2}] v'(C_0).$$

On putting $T = 0$ in the equation at the top of the page now, we derive the result of the theorem.

<u>Theorem 2.</u> <u>If</u> $s(k,a)$ <u>is any solution of the fundamental equation</u> (<u>i.e., the equation of theorem 1</u>) <u>the corresponding total expected valuation is</u>, <u>if it is finite</u>,

$$-\int_k^\infty v'[af(x) - s(x,a)]dx,$$

<u>for an economy in which capital and the production coefficient are initially</u> k <u>and</u> a.

<u>Proof</u>. I shall prove that

$$\frac{d}{dt}\left\{e^{-rt}E[\int_{K_t}^\infty v'(ae^{\alpha t+\varepsilon_t}f(x) - s(x, a\,e^{\alpha t+\varepsilon_t})).dx\right\}$$

$$= E[e^{-rt}.v(ae^{\alpha t+\varepsilon_t}.f(K_t) - s(K_t, a\,e^{\alpha t+\varepsilon_t}))],$$

where K_t is the capital path produced by using the investment strategy s. The theorem then follows by integration with respect to t.

The argument at the end of the proof of theorem 1 shows that the left hand side of the above equation is equal to

$$E[e^{-rt}\left\{D\int_{K_t}^\infty u[x, ae^{\alpha t+\varepsilon_t}].dx - u.s(x, ae^{\alpha t+\varepsilon_t})\right\}],$$

which, since u satisfies the fundamental equation, is equal to

$$-E[e^{-rt}\int_{K_t}^\infty (u.ae^{\alpha t+\varepsilon_t}.f'(x) + s(x,ae^{\alpha t+\varepsilon_t})\frac{\partial u}{\partial x})dx]$$

$$-E[e^{-rt}u.s],$$

$$= - E\,[e^{-rt}\int_{K_t}^{\infty}(u.ae^{\alpha t+\varepsilon_t}.f'(x) + \frac{\partial s}{\partial x}u).dx]$$

$$= - E\,[e^{-rt}\int_{K_t}^{\infty}\frac{d}{dx}\,v(ae^{\alpha t+\varepsilon_t}f(x) - s(x,ae^{\alpha t+\varepsilon_t})).dx]$$

$$= E\,[e^{-rt}.v(ae^{\alpha t+\varepsilon_t}.f(K_t) - s(K_t,ae^{\alpha t+\varepsilon_t}))].$$

The proof is thus complete.

<u>Theorem 3.</u> <u>If the maximal valuation is finite for some</u> $\beta > 0$, <u>when</u> r <u>and</u> α <u>are zero, and the maximal valuation is denoted by</u> $V(r,\alpha,\beta)$, <u>then</u>

$$V(r,\alpha,\beta) = V(0,0,\beta) + o(\sqrt{r^2+\alpha^2+\beta^2}).$$

<u>Proof.</u> I denote the optimal policy when the dynamic parameters are r, α, and β by $s(k,a)$. Then

$$V(0,0,0) \geq \int_0^{\infty} v[af(k_t) - s(k_t,a)].dt,$$

where k_t is defined by $\dot{k}_t = s(k_t,a)$, $k_o = k$. $V(r,\alpha,\beta)$ is finite for (r,α,β) in some neighbourhood of $(0,0,0)$. Hence we can choose T so that

$$\left|E\,[\int_T^{\infty} v(ae^{\alpha t+\varepsilon_t}f(K_t) - s(K_t,ae^{\alpha t+\varepsilon_t}))e^{-rt}dt]\right| < \delta$$

in this neighbourhood, for some given number $\delta > 0$. T and the neighbourhood can be so chosen that

$$\left|\int_T^{\infty} v[af(k_t) - s(k_t,a_t)].dt\right| < \delta ,$$

too. What has to be proved now is that

$$\left|E\left[\int_T^T v(ae^{\alpha t+\varepsilon_t}f(K_t) - s(K_t,ae^{\alpha t+\varepsilon_t}))e^{-rt}dt\right] - \int_o^T v[af(k_t) - s(k_t,a)]dt\right| < \delta ,$$

for small enough r, α, and β.

I do this by leaning on the assumption that s is a continuous function. It is well known that for any continuous function ϕ, $E[\phi(\varepsilon_t)]$ tends to $E\phi(0)$ as β tends to zero. Therefore the first integral tends to its value when $\beta = 0$, as β tends to zero. Then we can let α and r tend to zero, with the same result; and in this way the inequality is proved. It follows that

$$V(0,0,0) > V(r,\alpha,\beta) - 2\delta.$$

To prove the opposite inequality: $V(0,0,0) < V(r,\alpha,\beta) + 2\delta$, a similar argument can be used with the static investment strategy $i(k,a) = \theta(af(k))$. The two inequalities together prove the theorem.

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