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PART III

LONG-TERM TENDENCIES IN PRIVATE CAPITAL FORMATION

The Rate of Growth and Capital Coefficients

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A. INTRODUCTION

THE first four sections of this study lead to an appraisal of the "rate of growth and capital coefficients" approach to the aggregative theory of investment. A brief statement of the approach itself is found in Section C-4.

Sections F and H apply this approach to the problem of long-term investment projection as it presented itself prior to the outbreak of hostilities in Korea. "Long run" will here mean roughly a period extending from the "present" to a comparable phase of a future business cycle, some years ahead. However, comments on longer-range problems will also be included, involving statistical data for several past decades.

Not all economic theory is directly connected with the objective of prediction. It takes professional analysis to derive *specific criteria of evaluation* applicable to economic processes from *given general principles* considered significant in a social system. Theories performing this function are important and they are at best indirectly related to prediction. They are linked to prediction merely by their ability to disclose the consistency or inconsistency of specific economic results with the survival of some social system which is defined by certain general principles. However, a large part of modern economic theory is much more directly oriented to the objective of making informed guesses. This study is concerned with theories of the latter type.

The justification for developing theories of this sort is not that they perform particularly well as measured by their own standards. The justification is that it frequently is necessary to make

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up our minds on the probable course of future events in circumstances that preclude dependable prediction. In our everyday lives as in the social sciences, it is impossible to do this without some kind of theory, although the "theories" we use in our everyday lives to decide what is likely to happen in the future are usually too crude and unsystematic to warrant the term.

B. THE MEANING OF PROJECTABILITY IN ECONOMICS

At the present stage, we cannot expect to develop a theory of investment which could be used for prediction in a more or less mechanical fashion. All economic theory is based on *ceteris paribus* assumptions and all economic theory requires informal appraisal of how reality is likely to accord with, or deviate from, these assumptions. Alternatively we may say that our theories are established for given "environments." Their logical structure is valid for a specific environment, and the functions of this structure shift when the environment changes. Appraisal of the likelihood and of the consequences of environmental change remains largely a matter of subjective or quasi-intuitive judgment.[1]

Given these limitations, the predictive usefulness of an economic theory depends in a large measure on whether it implies a useful separation of "formal framework" from "environment." The formal framework must enable the economist to collect information and to draw rigorous conclusions for given environmental conditions (i.e., on definite *ceteris paribus* assumptions). At the same time the environment in which the logical structure operates must be defined in such a way that the problem of environmental change appears as an articulate, meaningful problem. The appraisal of the likelihood of environmental change (in this sense) will remain a matter of individual judgment, but not every environmental problem constitutes an articulate complex on which individuals are capable of using their subjective judgment.

A useful theory must separate internal structural elements from environmental elements in a convenient way. More cannot at present be expected. In economics, true projectability is at

[1] These judgments can be characterized by an analogy. They are like feeling an imperfect die and then forming an opinion of roughly how the frequency distribution for a series of throws is likely to be influenced by the imperfections.

present a utopian goal. Any number of predictive theories will hold on the corresponding *ceteris paribus* assumptions, and no theory will hold aside from such assumptions (i.e., no theory is sufficiently complete to incorporate all causal factors into the logical structure itself). We must try to select theories with fruitful *ceteris paribus* assumptions.

Unfortunately, realism is not the criterion of the fruitfulness of these *ceteris paribus* assumptions. They are certain to be unrealistic. Our ability to arrive at objectively justifiable appraisals of the environmental factors is also not an acceptable criterion, because if this criterion were satisfied, the environmental factors could be worked into the logical framework itself. They would then cease to be environmental factors in this sense. The main criterion of fruitfulness with respect to the environmental factors (or *ceteris paribus* assumptions) is our ability to arrive at some kind of quasi-intuitive judgment concerning them. It is helpful if reasonably independent judgments of individuals do not differ too radically from one another, or if most persons forced to make judgments of this sort fall into a small number of groups. This condition cannot always be satisfied. But we should always try to select theories which separate the logical apparatus from the environment in such a way that (a) information is available for drawing inferences from the logical apparatus on definite environmental assumptions, and (b) the problem of environmental change constitutes an articulate complex capable of provoking an answer from the typical individual who is forced to make a judgment. Condition b should probably be labeled "genuinely psychological." The present writer knows of no helpful discussion (explicit treatment) of this problem, but he feels convinced that fruitful theorizing requires awareness of its significance.

This paper is concerned, not just with predictive implications of economic theory in general, but with the question of the usefulness of specific kinds of theory for a definite problem of qualified prediction. We shall be concerned with long-run projections of private capital formation. The difficulties of long-run projection are partly different from those of making informed guesses for the near future. Some errors tend to cancel in the long run. More can be said about average relationships over a longer past period than about individual instances belonging in a universe. Hence, if the appraisal of the *ceteris paribus* assumptions (environmental factors) gave rise to the same difficulties for a short

as for a long future period, long-run projection should be considerably more dependable than short-run projection. However, the appraisal of the environmental factors usually becomes more difficult when the period is extended.

C. PRELIMINARY COMMENTS ON ALTERNATIVE METHODS OF PROJECTION

1. *The current textbook proposition*

The amount of investment is said to equate the marginal efficiency of capital to the rate of interest. This proposition can be worded in several alternative ways and has its equivalents in pre-Keynesian terminology. All propositions of this kind are elaborations on the profit-maximization principle. They form the counterpart of the utility-maximization principle and its applications in the theory of consumer demand.

These propositions are important because they disclose the specific economic corollaries of general principles which play a significant role in contemporary social systems. However, these propositions are not at present directly applicable to microeconomic projection.[2] It is not easy to conceive of statistical techniques which would measure the highly volatile marginal efficiency schedule (or, in general, the investment-vs.-interest-rate schedule, regardless of whether this implies strict profit maximization). It seems to us that the empirical approach to the theory of investment requires keeping some sort of qualified profit-maximization principle in the background of the analysis without losing sight of it. However, lack of data makes it impossible to place the principle in the center of empirical investigation. Whatever relationships may become "established" empirically between investment and other observable variables, it is necessary to stay aware of the fact that these relationships imply something with respect to the profitability to individual firms of the pattern of behavior under consideration. A valid pattern of behavior must be compatible with individual profit objectives, although not necessarily with the all too simple principle of strict profit maximization. In other words, the methods with which we will be concerned are one or more steps removed

[2] On the microeconomic level, predicting the behavior of the firm requires a framework which is largely built around profit objectives.

from the usual textbook propositions, but an attempt should be made to see what the links are.

2. *The questionnaire method*

In recent years data have become available on planned plant and equipment expenditures of business. The so-called SEC-Commerce data, which are based on direct inquiries to a sample of firms accounting for a substantial proportion of total investment, apply to periods ahead ranging from a few months to a year. The McGraw-Hill survey is an example of the attempt to obtain information about long-range plans by similar methods. It seems to us that the questionnaire method is more promising for short-run than for long-run projection.

This does not mean that long-range data of this sort are useless. However, their potential usefulness does not derive from the reliability of the planned investment-outlay figures. Investment plans for several years ahead are very tentative and are almost certain to be changed with the passage of time. Direct information concerning long-run plans may prove to be revealing, not because the planned investment outlays are likely to be realized, but because the relationship between different planned magnitudes or changes may be indicative of how much of something may be expected to go with how much of something else. The answers of firms may disclose the fact that they intend to increase their capacity by a certain number of output units and that they expect to spend a certain amount on such a program. The ratio of these figures contains a more useful piece of information than do the two figures in isolation. Moreover, this piece of information—the planned investment outlay associated with a unit increase of capacity—must be obtained from long-run relationships, if it is to be useful as supplementary information for long-run projections based on other methods. Therefore, answers of firms to questions concerning long-run investment plans may contain valuable information. But the planned total outlays bear no easily understandable relationship to the total outlays the investigator should expect over a longer period.

The planned total outlays may be quite different from those realized, even in shorter periods. But considering the fact that it is wasteful to stop halfway in the realization of short-run plans, and that commitments are entered before outlays are actually made, it is conceivable that appropriate interpretation of planned

outlays will prove to be of direct help for the short-run projection of private investment. It is possible that sufficiently inclusive inquiries will lead to the collecting of planned data which, without further manipulation, will give tolerably good approximations to realized data over sufficiently short periods. This result could scarcely be achieved for total private investment including inventory accumulation, because inventories are partly determined by daily fluctuations of demand. But it is not inconceivable that, for short periods, a good approximation to realized plant and equipment outlays could be obtained by collecting data on planned outlays. Short-run GNP (gross national product) projection would still remain a thorny problem, because additional information would be required on income-consumption relationships, on government expenditures, and on planned inventory accumulation to appraise the level toward which output is moving. Subsequently, it would be necessary to allow for the fact that a rise (so obtained) may be somewhat counteracted by an unplanned reduction of inventories, and that a fall (so obtained) may be counteracted by an unplanned increase of inventories. Yet, it is worth while to make an effort to obtain reliable information on plant and equipment outlays for a period ending a few months ahead. Even if businessmen should change their minds very frequently, and if, therefore, data of this kind should never become good approximations to the subsequently realized magnitudes, it is conceivable that valuable information could be derived by comparing the planned magnitudes with the subsequently realized ones for a succession of short periods. Some property of the planned series may become an advance indicator of some subsequent property of the realized series.[3]

In summary, we feel that the survey method may in due time become significant, but that its significance is likely to be greater for short-run than for long-run projection. However, the method may yield valuable supplementary information even for long-run projection, because the estimates of firms concerning the long-run relationship between different magnitudes may be subject to less severe limitations than their estimates of what they are going to spend in the course of a period of considerable duration.

[3] For example, it is conceivable that a sufficiently great decrease in the rate of increase in the planned series goes with a fall in the realized series.

3. *Projecting investment from profits*

Methods based on observed relationships between profits and subsequent private investment are but one step removed from textbook propositions of the "marginal efficiency vs. rate of interest" variety. This is especially true if some allowance is made for the availability of funds for investment. In a sense, it is true even aside from this. Any reasonable interpretation of the effect of present profits on future investment must imply that present profits have something to do with profit expectations for the future. Consequently, if future investment is said to depend on present profits and some magnitude related to the availability of funds, we come close to the "marginal efficiency and interest rate" proposition. Even if, technically, no separate allowance is made for funds, we may be interpreted as using the same general kind of approach on the further assumption of given expectations concerning the availability of funds. If only profits and lagged investment are included in the technical approach, it is necessary to keep in mind the dependence of the observed relationship on the liquidity position of firms and to make informal allowances for possible changes. If the technical apparatus is rendered more complete by the inclusion of some measure of the availability of funds (and possibly by the inclusion of further variables), then these informal allowances pertain to a different complex of "outside" (environmental) factors, but they still will have to be made. In statistical work, some investigators have found consistent relationships between profits and lagged investment outlays. Some statistical business cycle models rely heavily on this relationship.[4]

The same statistical relationships do not bear directly on long-run projection. One reason for this—but perhaps not the ultimately significant one—is that the lagged profit-investment relationship must be assumed to depend on the nature of cyclical development. In a long period of typically high output with moderate cyclical swings, a given rate and amount of profit may call forth more investment than in a less satisfactory (more insecure) period. Most of our statistical experience relates to decades during which cyclical instability was greater than we hope

[4] Cf., e.g., Lawrence R. Klein, *Economic Fluctuations in the United States, 1921-1941*, Cowles Commission for Research in Economics, Monograph No. 11 (Wiley, 1950), chap. 3. Cf. also *Factors Affecting Volume and Stability of Private Investment*, Joint Committee Print (Government Printing Office, 1949).

it will be in the future periods with which we are concerned. This is especially true if we disregard the first half of the 1940's as untypical from the point of view of the profit-investment relation. On the other hand, we have had several postwar years of high employment and of little instability. In general it may be argued that it has been possible to observe lagged profit-investment relations during subperiods with markedly different characteristics, and that the difficulties of making up our minds on the relevance of the one or the other of these experiences for projection are not fundamentally different from a great many other difficulties with which long-run projection is fraught.

What really destroys the usefulness of this type of analysis for long-term projection is the shortness of the lag between profits and investment outlays. Whether this lag is in the order of six months or of a year, its short duration excludes the possibility of reaching the average investment outlays of a long period ahead from profit data now available. Profit-investment relations may still retain a good deal of indirect usefulness for long-term projection because they point to the likelihood that sustaining a given amount of investment in some future period of longer duration will require profits of some magnitude during approximately the same period. This piece of knowledge in itself tells us nothing about how to project future investment from data now observable, for it contains no indication concerning the dependence of future profits on present data. However, propositions of the sort here considered do possess indirect usefulness for long-run projection, in the sense of telling us something about certain conditions which will have to be met in the future if projections derived by other methods are to be trustworthy. Projections resulting from other methods will, of course, have to be interpreted as implying certain *ceteris paribus* assumptions, or qualifying clauses, pertaining to the environmental factors not included in the theory. One group of significant environmental factors is connected with the profit problem considered. For long-term projection, this group of factors will have to remain "environmental," because we have no way of projecting distant future investment from present profits, or distant future profits directly from now observable variables.

4. *Projecting the rate of increase in output and the capital requirement for achieving it*

In the present essay, this kind of approach will receive more attention than those previously discussed, although our conclusions will take account of supplementary considerations based on the preceding discussion.

The dominant trait of the theory and the method in question is that they imply our ability to appraise crudely (in part, quasi-intuitively) whether certain observed relationships between stock and output are likely to change and, if so, what the nature of this change might be. It would be unfair to the theory, and to the method of projection based on it, to maintain that it implies constancy of some property of the capital-output relationship, let alone constancy of the capital coefficients themselves. Crude statements of the theory may lead one to believe that it implies just this, but such a theory would be clearly unacceptable. The general limitations of economic theory and of methods of projection must be kept in mind here as elsewhere. At the present stage, we cannot expect to develop a complete theory of investment which would incorporate into its formal structure all factors influencing the outcome. We can merely place in the foreground of our analysis a formal relationship which holds on *ceteris paribus* assumptions (i.e., given the "environment" in which the logical structure operates), and then try to allow in a quasi-intuitive fashion for changes in the environmental factors. No general agreement can be expected on the appropriateness of these allowances, but the problem of these environmental changes (that is, of the deviations from *ceteris paribus* assumptions) may or may not be posed in a meaningful way. Individuals—and perhaps groups of individuals similarly inclined—may or may not feel that they have tentative answers to these environmental questions. What the method to which we now turn implies is that certain aspects of the capital-output relationship lend themselves to reasonable generalizations on given "environmental" assumptions, and that the question of environmental change so posed is an articulate (i.e., meaningful) question in the sense here described. In the opinion of the present writer these conditions are better satisfied for the approach now under discussion than for the approaches previously considered.

In this introductory section, only the broad characteristics of

the approach will be described. More detailed analysis will be made in subsequent sections.

We may begin by stating a truistic relationship and by inquiring into the circumstances in which the relationship acquires more meaning. It follows from the *ex post* savings-investment identity that the product of the percentage rate of growth of output times the incremental (i.e., marginal) capital-output ratio must always equal the percentage of output which is absorbed by investment. Hence we may write

$$\frac{\Delta V}{\Delta O} \cdot \frac{\Delta O/\Delta t}{O} = a \qquad (1)$$

where V means the stock of wealth, O the rate of output, t time, and a the ratio of savings to output (i.e., one possible definition of the "average propensity to save"). This follows from the fact that the foregoing expression is a slightly changed version of $\Delta V/\Delta t = aO$, that is to say, of aggregate investment equals aggregate savings. If we want to have on the right-hand side the average propensity to save in a now more usual sense, that is, in the sense of the ratio of individual savings to the disposable income of individuals, then the expression becomes somewhat more complicated. It then takes the form

$$\frac{\Delta V}{\Delta O} \cdot \frac{\Delta O}{\Delta t} + G_n = a(O - \text{NBS} - \text{TP} + \text{TR}) + \text{NBS} + \text{TP} \qquad (2)$$

where O (output) is interpreted as the net national product, NBS means net business savings, TP tax payments, TR government transfer payments, and G_n that part of the government output which does not increase the capital stock and also is not included in consumption. Considering the nature of the data which will be used in this study, we shall mainly be using (1), but it must be remembered that in (1) G_n equals zero, and hence all government output is interpreted either as consumption or as capital formation (see Section F).

Purely logical relations do not in themselves solve empirical problems. A useful relationship is obtained only if the values of the magnitudes entering into these equations are capable of being interpreted in a reasonable way. This means that it must be possible *to appraise the conditions under which the values in question would tend to repeat themselves and also to appraise, in a general way, the nature of the change in these values which may be*

produced by changes in the surrounding conditions. Short-run values of the variables of our equation do not, in themselves, satisfy this condition. Long-run average values may come closer to satisfying it and, if they do, comparisons of the long-run average values with the fluctuating short-run values may prove revealing for short-run (i.e., business cycle) analysis.

Changes in the rate of growth of output from one short period to the next are quite erratic in the sense of disclosing no orderly pattern. The truism that the rate of change would be the same in each short period if all properties of the environment repeated themselves is completely sterile, because we are incapable of forming a judgment on the relevance of the various environmental factors and on the likelihood of their repeating themselves. Similar statements may be made of the short-run behavior of $\Delta V/\Delta O$ and of a. But it is hoped that the same misgivings apply to long-run average data merely in an attenuated form, so that investigators may find it useful to study the past behavior of the long-run data and to try to appraise by general judgment the likelihood of outside changes which may alter the values in question for future periods. The cycle (or cycles) may then be represented in terms of deviations from long-run values, although interrelations between the trend and cyclical deviations must not be overlooked. A theory corresponding to the long-run interpretation of our equations leads into questions of this sort: For what reasons and in what way is the future average rate of growth of output likely to be different from the typical past rate of growth, as observed over a period of several decades? For what reasons and in what way is the behavior of the marginal capital-output relationship likely to deviate from its past behavior? And that of the saving ratio a? Opinions will differ on these matters, but it nevertheless seems to us that these questions are posed in an articulate and useful fashion.

The method of projection corresponding to this approach is one that is intended to find consistent future values for the variables entering into the equation used, with the proviso that they must bear a plausible relation to the past behavior of these variables. By the consistency of values, we mean that the equation must be satisfied. By plausible relation to past behavior, we mean that the investigator must be able to "make a case" for the assumption that the past values will repeat themselves or that they will change in one way or another. This "case" must be

made in terms of properties of the "environment," that is to say, in terms of factors external to the formal apparatus itself. The reader, if he is dissatisfied with these environmental assumptions, may then substitute his own for those developed in any particular analysis.

In the treatment of the "environmental" problem (or *ceteris paribus* problem), the influence of government policy on these variables must not be overlooked. This influence is quite direct on government expenditures in general and it is direct on a, too (especially as defined in [1], where it expresses government capital formation as well as savings in the ordinary sense). The influence of policy on $\Delta V/\Delta O$ and on $(\Delta O/\Delta t)/O$ is perhaps less "direct," but it is also significant. Relative prices (scarcity and abundance in specific areas) and the state of the credit market are likely to have an influence on $\Delta V/\Delta O$, because there is no reason to assume extreme insensitivity of coefficients of production in the long run. These same factors are almost certain to influence the rate of growth of output, which, at the same time, may obviously be affected by tax policy and also by wage policy.

Treatment of the problem in terms of the equation formulated has the advantage of avoiding unnecessary restrictive assumptions with respect to the sequence of causation. The various magnitudes included in the equation interact. The significant questions are those relating to the level of activity at which the equation becomes satisfied and to the nature of the adjustments by which the two sides are made equal. Obviously, there exists a great deal of difference between an automatic adjustment in a, expressing itself in the lowering of its value through underemployment, and an automatic adjustment of the rate of growth of output to the available resources. If analysis of this sort is to be useful, it is necessary to make up one's mind as to what is likely to adjust under different conditions, and as to what the nature of the process of adjustment is likely to be. But, it is not necessary to develop a sweeping general hypothesis which explains investment as being caused by the rate of growth of output, via the capital-output relation; or an alternative general hypothesis explaining the growth of output as being caused by investment, via the capital-output relation. Interactions may be recognized. The requirement is merely that of the consistency of various changes.

The foregoing discussion was deliberately held in very general terms. Subsequently, it will be made more specific in two respects. The problem of the environmental factors (or "outside" factors, *ceteris paribus* assumptions) will be discussed explicitly, and the question will be raised as to what really is involved in appraising these by general judgment. Secondly, some methods of obtaining information on the past behavior of the relevant magnitudes will be discussed. However, before turning to these problems, we shall compare the present approach with more or less closely related ones which received attention in the recent literature.

D. SOME NEWER VARIANTS OF THE ACCELERATION PRINCIPLE

If we interpret the term broadly enough, the approach just considered is a variant of the "acceleration principle." This is because it conceives of the amount of investment as the product of the rate of growth of output and of a relationship ($\Delta V/\Delta O$) by which the rate of growth is linked to investment. But most presentations of the acceleration principle are based on more specific or restrictive assumptions which are not expressed in our general equations (Section C-4) and which will also not be introduced into our subsequent specific discussion. In some recent expositions of the acceleration principle, investment is linked to the rate of increase in consumption rather than output. In most expositions of the principle, high rigidity of $\Delta V/\Delta O$ (or at least of the $\Delta V/\Delta O$ compatible with dynamic equilibrium) is assumed. In some cases this may be a consequence of the fact that the principle is used in the framework of business cycle analysis, i.e., for the discussion of short-run change. But even where the short-run analysis proceeds by way of contrasting long-run relationships (observable along trend lines) with the fluctuating relations of cyclical development, it is not unusual to imply rigid capital-output relations throughout the analysis. Furthermore, the causal sequence is frequently represented as running unequivocally from the rate of increase in output, or in consumption, to investment, rather than possibly the other way around, or both ways. This is an unnecessarily restrictive assumption even for short-run analysis. If one or more of these restrictive assumptions are considered essential to the acceleration principle,

then the more flexible approach to be adopted here should not be called a variant of this principle. Nor should it be called a variant of this principle if the latter is limited to business cycle analysis. The equations of Section C-4 can be interpreted as expressing the acceleration principle only in the very general sense that they emphasize a consistency requirement between the rate of increase in output and output itself according to equation 2:

$$O = \frac{\Delta O}{\Delta t} \cdot \frac{\Delta V}{\Delta O} + G_n$$

This analogy makes it desirable briefly to survey some recent treatments of the acceleration principle and also to attempt to make explicit the most essential differences.

1. *The Samuelson analysis*[5]

Professor Samuelson's well-known presentation of a multiplier-acceleration model is of an essentially short-run character; at least one of the magnitudes ($\Delta O/\Delta t$) shows a behavior which is not found in long-run average values and discloses disturbances (incorrect expectations) of the sort encountered in various phases of cyclical development. The corresponding long-run system (pertaining to trend values) could be characterized by

$$\frac{\Delta V}{\Delta C} \cdot \frac{\Delta C}{\Delta t} = a \cdot O \qquad (3)$$

where C stands for consumption. Otherwise, the notation of our equation 1 is used. Equation 3 differs from ours merely in that the capital stock is conceived of as producing consumer goods, rather than output (i.e., consumer goods *and* investment goods). There is no need for introducing an assumption of this kind, unless it simplifies the exposition and is otherwise unimportant for the purpose of a specific piece of analysis. No simplifications would be achieved by writing ΔC instead of ΔO in the long-run equation just considered. Samuelson uses no equation of such long-run variety but he uses dated, short-run relationships by which investment is derived from the rate of change of consumption rather than output. To the investment so derived (I) there is added a constant flow of "autonomous" investment.

[5] Paul A. Samuelson, "Interaction between the Multiplier Analysis and the Principle of Acceleration," *Readings in Business Cycle Theory*, American Economic Association (Blakiston, 1944).

(It may therefore be appropriate to add a term for autonomous investment on the left-hand side of Equation 3, too, and to interpret ΔV as applying merely to induced investment, that is, to Samuelson's I.)

Whether the results are affected by using ΔC instead of ΔO is a question which could be answered in different ways. It is true that the Samuelson system can be rewritten with ΔO in the place of ΔC; in *his* notation, with

$$I_t = \beta\,(C_t - C_{t-1} + I_t - I_{t-1})$$

in the place of his

$$I_t = \beta\,(C_t - C_{t-1})^6$$

and that the broad characteristics of the formal apparatus remain the same after this change. But the change brings out the fact that a sufficient degree of optimism is self-justifying as long as the economy does not run into specific scarcities. For in the broadened model characterized by

$$I_t = \beta\,(C_t - C_{t-1} + I_t - I_{t-1})$$

there exists no good reason for assuming that the value of β and that of I_t are technologically determined by the capital-depleting effect of a past increase in consumer income such as results in a rise in consumer demand. The *justified* values of β and of I_t (the values which will create no excess capacity) become dependent upon how much will be invested in subsequent periods. With no scarcities and no uncertainty in the model, there is nothing to limit investment in the successive periods. This is an essential characteristic of such a system. A similar conclusion could be read even from a model using ΔC rather than ΔO, although the treatment would then have to include a discussion of the significance of changes in the so-called autonomous investment of private producers.

If we link the endogenous or induced investment to movements *in consumption alone,* then our proposition relating to the self-

[6] C = consumption; I = investment; β corresponds to $\Delta V/\Delta O$ or to $\Delta V/\Delta C$, depending on whether, in the model we use, investment depends on the change in output or on that in consumption (that is, whether it corresponds to $\Delta V/\Delta C$ in the Samuelson model proper). Subscripts relate to time periods. Samuelson also has $C_t = \alpha Y_{t-1}$, where α stands for the marginal propensity to consume and Y for income. This fully describes the system, except that there is a constant amount of autonomous investment, in addition to $I_t = \beta\,(C_t - C_{t-1})$. Hence, in addition to this last equation for I_t and in addition to $C_t = \alpha Y_{t-1}$, the model assumes $Y_t = 1 + C_t + I_t$.

justification of investment aside from specific scarcities and uncertainty must be developed with reference to an unexplained flow of "autonomous" investment. For not even from the viewpoint of purely aggregative theory is investment justified *by consumption alone*, except to the extent that a rise in consumption which is occasioned by a past rise in consumer income has already depleted the capital stock or currently tends to deplete it. Aside from this, investment must justify itself partly by future consumption and partly by future investment, provided that the marginal propensity to consume is less than 1. In a growing economy, investment must become justified partly by subsequent future investment because the income created by investment is only partly consumed. In a model linking investment to an increase in consumption alone, only that part of the investment can be explained which is called forth by the fact that a preceding rise in income may, via the propensity to consume, be exerting a capital-depleting influence. If investment is linked to ΔC rather than ΔO, then it must be pointed out that, in addition to the investment in question, any amount of autonomous investment would be self-justifying in the foregoing sense, and that the nature of a purely aggregative cycle model so developed depends ultimately on what is postulated concerning the behavior of autonomous investment. This is not very satisfactory. The same is true if we link endogenous or induced investment to movement in output ΔO but limit the concept of this investment to what is technologically justified by the *past* ΔO (as if no further ΔO were expected). This is what Professor Hicks has done. In this case, too, we have to deal separately with an unexplained and self-justifying flow of autonomous investment, because not all investment can be related to the *past* ΔO in a purely technological way. The outcome will again depend on our postulates concerning autonomous investment. However, *all investment may be said to bear some reasonable relation to expected movements in output.* A system stressing this relationship does not have to exclude part of the investment flow by labeling it autonomous. Only if some constituent of total investment can be explained more satisfactorily by a definite relationship of a different sort, is it advisable to distinguish between flows of investment induced in different ways. The distinction between induced and autonomous does not seem fruitful and it can be avoided if we relate investment to movements in output, without implying that the relevant relation-

ship links past changes to present investment in a purely technological way. Specific scarcities and uncertainty then become the limiting factors. Even the question of how completely the long-run "tendency" toward diminishing returns must be offset in each period by improvements depends on the appraisal of uncertainty (that is, on required risk premiums). However, this question does not arise in the Samuelson system, in which investment does not appear to be subject to a tendency toward diminishing returns. The "tendency" toward diminishing returns, of course, is also a matter of relative resource scarcities, although not of scarcities in specialized resources.

In the present paper we shall relate investment to output trends and we shall not limit the concept of the "induced" to what is technologically justified by past rates of increase. The attempt will be made to deal with the magnitudes involved in equation 1 in terms of long-run average values with the underlying assumption that errors may have largely cancelled over these periods, and hence that the realized magnitudes disclose planned expected relationships, except where there exist specific indications to the contrary. Over these longer periods all terms of our basic equation tend to adjust. Obviously adjustments are far more limited in the short run, and the general suggestion will be made that comparison of the fluctuating short-run values of these variables with their average long-run values may contribute to the understanding of the business cycle. This paper will not be concerned with the interpretation of cyclical developments. But the kind of cycle theory by which the present approach may be supplemented is one that develops cyclical errors and their consequences by contrasting short-run with long-run values. Such a treatment focuses attention, not on the unexplained size of autonomous investment, but on the question of how and with what lags the internal structure of the economy can adjust to the requirements that were expressed in aggregative terms.

2. *The Harrod-Domar analysis*

If the type of analysis developed by Mr. Harrod in England and by Professor Domar in the United States is characterized broadly enough, it may be considered identical with that underlying the present approach.[7] Similar views have been expressed by other

[7] Cf. R. F. Harrod, "An Essay in Dynamic Theory," *Economic Journal*, March 1939, and *Towards a Dynamic Economics* (London: St. Martins,

authors, including the present writer.[8] However, specific differences must also be pointed out.

Harrod uses the equation $G \cdot C = s$, where G stands for our $(\Delta O/\Delta t)/O$, C stands for our $\Delta V/\Delta O$, and s for our a. In elaborating upon this relationship, he later writes $G \cdot C = s - k$, where k expresses capital formation such as is "not deemed to have any immediate relation to current requirements." We prefer not to include such a term because consistent treatment of it would require the kind of period analysis suitable for short-run analysis and employed neither by Harrod nor in the present approach. Sooner or later all additions to capital must prove to be justified by "requirements," if no disturbance is to develop. Harrod's k is not intended to explain disturbances, but merely the lag that may arise between investment and its justification by "requirements."

Domar's apparatus is of a similar kind. The "equilibrium" rate of growth is defined as $a\sigma$, where a is the average propensity to save and σ is the reciprocal of our $\Delta V/\Delta O$ (or, more precisely, of our V/O, with explicit recognition of the fact that if the incremental ratio should be different from the average ratio, then the incremental ratio must be used). The resulting equation is identical with ours. It is not quite clear to us how much flexibility Domar attributes to σ (or to a) in the long run. Extreme rigidity is not assumed, but it seems to us that the present analysis places more emphasis on long-run adjustments of all variables to equilibrium requirements than does that of Domar. Our way of handling the matter is to find the long-run average values of these variables, recognizing the fact that they are likely to result from (usually incomplete) internal adjustments to changing conditions and that their long-run values may change considerably from one long period to another. We suggest that short-run disturbances may be analyzed in terms of deviations from these long-run values. Domar's way of handling the matter is that of finding the theoretical long-run rate of growth compatible with a more or less *given* (perhaps somewhat adjustable) $\Delta O/\Delta V$, and with a more or less *given* (perhaps somewhat adjustable) a. Economic conditions depend on whether this theoretical rate can

1948), lecture 3; E. D. Domar, "Expansion and Employment," *American Economic Review*, March 1947, and "The Problem of Capital Accumulation," *ibid.*, December 1948.

[8] William Fellner, *Monetary Policies and Full Employment* (University of California Press, 1946-47), pp. 27-45, 73-83.

be achieved in reality. However, some adjustability is assumed by him; and on the other hand, we do not postulate that the long-run values always express complete adjustment to equlibrium requirements.

The Harrod analysis possesses a specific characteristic which will not be incorporated into the present approach. When analyzing the equation

$$\frac{\Delta O/\Delta t}{O} \cdot \frac{\Delta V}{\Delta O} = a$$

(or, in his symbols, $GC = s$), Harrod describes three possible ways of interpreting $(\Delta O/\Delta t)/O$, and his theory consists largely of contrasting the values obtained on these three different interpretations. The *actual* rate of growth and the actual values of the other terms are the *ex post* magnitudes found for any period, i.e., the values by which this tautological relationship becomes always satisfied. *Warranted* values are those compatible with the equilibrium requirements of the system, so that the warranted rate of growth is that which is compatible with a true incremental capital requirement (leaving producers satisfied with what they have done), and compatible with the true or intended value of the propensity to save. The *natural* rate of growth is the maximum rate compatible with the underlying real factors, such as population growth, new resources, technological progress, etc. Only for a short while can the actual rate of growth exceed the natural. This limited possibility, whenever it exists, is a consequence of excess capacity. Aside from this, the actual rate can be no greater than the natural rate of growth. The analysis is based on the idea that the natural rate of growth may be lower than the warranted rate, in which case the actual rate will also become lower than the warranted and hence a deflationary tendency will develop. Later, given enough excess capacity, expansion again becomes possible, because, for a while, the actual rate of growth may exceed the natural. But if the natural rate falls short of the warranted, the *trend* is unfavorable. If, on the other hand, the natural rate of growth exceeds the warranted, the trend is likely to be favorable (even inflationary?), because there is no lasting reason for the actual rate to fall short of the warranted rate. In general, comparison of the natural rate with the warranted rate leads to an appraisal of the long-run tendency, while comparison of the warranted rate with the actual rate characterizes the framework in which the cycle problem may be approached.

In the analysis of the present paper, the existence of a definite "natural rate of growth" will not be assumed. This is because the marginal capital-output ratio depends on the methods of production selected, and these may adjust, especially in the longer run. With the appropriate capital-output ratio, the economy *could* always grow at the warranted rate (see Section D-1, supra). We will be concerned with observable long-run values involved in Harrod's $GC = s$, and we will assume that these tend to be the "warranted" values at the same time, except in periods where some disturbance was so long-lasting (or so repetitive in one and the same direction) that the values of the variables could not adjust during these periods. But even in these chronically disturbed periods, the trouble should not be attributed to the existence of some well-defined natural rate of growth. It should be attributed to the inability of G, C, and s,—or, in our terminology, of $(\Delta O/\Delta t)/O$, $\Delta V/\Delta O$, and α—to find their "equilibrium values" in relation to one another. In the short run, they can scarcely be assumed to hit their "equilibrium values" (or warranted values), except by accident. The observable values of any short period, selected at random, are practically certain to be different from the warranted values of Harrod. Hence we also suggest that the comparison of *warranted* with *short-run actual* values may prove to be a fruitful avenue of cycle research. But we will assume that the *long-run actual* values may be interpreted as first approximations to the warranted values and that, when they cannot be so interpreted, the reason is the same as that producing short-run discrepancies (namely, failure of these values to settle down at the appropriate level *in relation to each other* during the period of observation). We will not build on the concept of the natural rate of growth.

The rate of growth and the capital-output ratio may well show, and in many periods have shown, a tendency toward long-run adjustment at warranted levels. Furthermore, the propensity to save may also show such a tendency in the long run, because the saving habits of a society need not be independent of its investment opportunities. The rate of growth, taken in isolation, could always be great enough to absorb the available resources, were it not for the possibility of specific scarcities, which are frequently overlooked in purely aggregative analysis. But even though the rate of growth, taken in isolation, could always be sufficient (aside from specific scarcities), it is not always, in reality. Each investor

is faced with uncertainty concerning the willingness of other investors to invest, and hence each investor is faced with uncertainty concerning aggregate effective demand. *Furthermore, each investor is faced with uncertainty concerning the specific composition of the aggregate demand which will be forthcoming.* Specific scarcities *or* these varieties of uncertainty may prevent the rate of growth from reaching "warranted" levels. Even aside from scarcities in specialized resources, the amount of new investment required for matching savings may, of course, give rise to gradually diminishing returns. This is because the relative scarcity in one of the broad factor categories may not be fully offset by improvements. But gradually diminishing returns could not limit the investment process were it not for uncertainty, which in each period sets *some* limit to the downward flexibility of the interest-plus-profit level. The concept of a definite natural rate of growth implies that the yield of investment, as well as relative income shares, is completely unadjustable to the equilibrium requirements of the system. Thereby the concept significantly overstates a limitation which is imposed by uncertainty.

The piece of truth overstated by the concept of a natural rate of growth is that the growth process could not continue in the face of a consistent and secular decline of returns. A "tendency" toward such a decline develops from a rate of increase in the capital stock which far exceeds the rate of increase in the supply of cooperating factors. The growth process requires that the tendency toward a consistent secular decline of returns should be counteracted by improvements the character of which must adjust to relative factor scarcities in the framework of a response mechanism. This is because the adjustability of interest rates, of relative income shares, and of saving habits to available investment opportunities is *limited.* Consequently, the adjustability of V/O and of $\Delta V/\Delta O$ to equilibrium requirements is also limited.

3. *The Hicks analysis*

Professor Hicks' recent statement of the relationship between the variables here considered brings out the point which we raised in connection with the extension of Samuelson's analysis.[9] Hicks views the "accelerator"—his v, which corresponds to Samuelson's β—as expressing the relationship between the rate of increase in

[9] J. R. Hicks, *A Contribution to the Theory of the Trade Cycle* (Oxford University Press, 1950).

output, on the one hand, and the amount of investment, on the other. It is the increase in output, not merely that in consumption, which induces investment. We pointed out earlier that, in such a broadened model, it becomes very clear that more investment justifies more investment, unless the economy runs into scarcities. With a given "accelerator"—a given v in Hicks' terminology, or a given $\Delta V/\Delta O$ in ours—economic activity may fluctuate and underutilization may develop. But whenever resources are available for expansion, producers *could* make the variable $(\Delta O/\Delta t)/O$ and the variable $\Delta V/\Delta O$ assume values which would result in an amount of investment such as absorbs savings at a higher level of output. Hence, when a cycle model is derived from such assumptions, the analysis rests essentially on the implied psychological attitudes (insufficient optimism) of investors.

In fact, the Hicks analysis makes this rather explicit, although the proposition is not stated in these terms. For the analysis proceeds on the assumption that the "accelerator" links the output increase of the past "period" (or sometimes of several past periods) to the "present" amount of investment, where, if no further assumption were added, one would have to conclude that such an "accelerator" is as much a psychological as a technological coefficient. This is because, given the past increase in output, more present and future investment will justify itself through future increases in output. But Hicks adds a further assumption. The "accelerator" determines merely the technologically "induced" investment from the *past increase in output*. In other words, it determines the investment which would be *technologically* justified if output were now stabilized at the level just achieved. In addition to this induced investment, there is "autonomous" investment, which, as long as resources are available, will justify itself in the future, provided that, in the future, enough of it is again undertaken. A cycle model can be obtained only on specific assumptions pertaining to some limited amount of autonomous investment which producers are "willing" to undertake. This is precisely what Hicks does. In his theory, the psychological assumption in question is "ultimate." It is given, and is incapable of being further explained, just like the technological or institutional data of the system.

We have seen that Harrod also introduces a term relating to investment undertaken with a long view. Harrod does not use the period analysis and consequently we did not find it easy to

interpret his expression for this far-forward-looking investment with much precision. No such difficulty arises in the conceptual interpretation of Hicks' autonomous investment, since Hicks' analysis is developed in terms of functional periods. In Hicks' model, the difference between induced and autonomous investment is well defined. But, is it possible to form an opinion of the realism of an assumption concerning the relative amounts of "induced" and "autonomous" investment so defined? Such a distinction is an absolutely essential property of the Hicks model. In contrast to Harrod's analysis, the Hicks theory cannot be presented without stressing the distinction between autonomous and induced investment, and no opinion can be formed of the validity of this theory without the appraisal of quantitative assumptions concerning the two.

The analysis results in fluctuations that would tend to become explosive, were it not for the fact that, in the upper regions, the rise becomes slowed down significantly due to the increasingly full utilization of the available resources. The accelerator transforms this slowing down of the expansion into contraction, which, in the low regions, becomes slowed down (and hence, via the accelerator, becomes reversed) through the fact that undermaintenance determines a floor level for net disinvestment per period. This outcome of the Hicks analysis depends on the author's quantitative assumptions concerning the relationship between the numerical coefficients, that is, between the "accelerator" (linking the past output increase to the present induced investment), the propensity to save, and the amount of autonomous investment. A judgment on the plausibility of any such assumption involves appraisal of how much investment is continuously induced by "past" increases in output (i.e., would be technologically justified if, after a rise, output were to continue at "present" levels), and how much investment is forward-looking in the sense of not meeting this test.

It seems to us preferable to bypass this difficulty. For long periods the distinction between "induced" and "autonomous" investment is not fruitful because all justified investment must bear a reasonable relation to output trends. Unless restrictive psychological assumptions are introduced, purely aggregative analysis cannot discover the reasons why the proper relation is not always satisfied at full employment. In a cycle study the relevant question would seem to pertain to the deviations of all coefficients

from their long-run values. This is a question of lagging internal adjustments, in specific sectors, to aggregative requirements and of uncertainty. It is not a question implying a distinction between investment *induced by past output increases alone* and a postulated rate of "autonomous" investment. We prefer not to build on such a distinction, although at the present stage of this kind of theory one should not purport to have very categorical views on what avenues will ultimately prove fruitful and what avenues will not.

E. THE *CETERIS PARIBUS* ASSUMPTIONS OF PROJECTIONS BASED ON THE RATE OF GROWTH AND THE MARGINAL CAPITAL-OUTPUT RATIO

We shall now return to that version of the "rate of growth and capital coefficients" approach which was broadly outlined in Section C-4. What is involved in using that version as an instrument of "projection"?

From past long-run experience we may obtain an idea of the likely future rate of growth of output, on certain *ceteris paribus* assumptions. The investigator will presumably not try to project the growth of output directly, but he will project population trends (or, more specifically, growth trends of the labor force), trends concerning the length of the working week, and trends in output per man-hour. These three types of projection, each interpreted on its own *ceteris paribus* assumptions, add up to a tentative projection of the rate of growth of output, provided that some further assumption is made concerning the future degree of "fullness" of employment. Wherever the investigator feels that he can (crudely) appraise the likelihood of deviations from the implied *ceteris paribus* assumptions, he can either try to make adjustments, carefully indicating what he has done, or he can simply call attention to the likelihood of these deviations and let the persons responsible for the ultimate decisions make the adjustments. No one is a "professional expert" in making adjustments of this sort.

From the long-run behavior of data, the investigator may also derive projections concerning the future marginal capital-output ratio. These also imply *ceteris paribus* assumptions, and the likelihood of deviations from these must again be appraised informally.

One element in the picture which calls for informal appraisal, and requires special attention at this point, is the consistency of the assumptions underlying the output-growth projection with the assumptions underlying the capital-output-ratio projection. For example, if the output trend is projected on the assumption of (practically) full use of capacity, the question arises as to how the past capital-output-ratio experience is affected by the fact that (practically) full use did not exist all the time. If, on the other hand, the output trend is projected on the assumption of a degree of utilization such as existed in the past, the future likelihood of more effective employment policies and perhaps primarily the likelihood of considerably higher government expenditures call for the same kind of supplementary appraisal.

The investigator now has a more or less "informed" guess of future private capital formation, on several corrected *ceteris paribus* assumptions and on some specific assumption concerning the degree of utilization. The consistency of this must be tested against a similarly "informed" guess of government expenditures and of tax payments plus individual and business savings. For, at the levels of output so projected, the planned private capital formation plus the government expenditures on goods and services must absorb the tax payments and the voluntary savings. The contrary assumption implies the kinds of disequilibrium and disturbances which are inconsistent with the method of approach here envisaged. The past experience which we have consulted is one relating to *average* conditions over longer periods, and it is implied that the observed magnitudes express habits and plans, i.e., that they do not to any substantial extent contain haphazard and irregular components such as unintentional savings (or dissavings) and unplanned accumulations (or shortages) of capital. If the data of some specific "long period" seem to be significantly affected by such unintentional components (e.g., if a decade was one of protracted depression or of chronic inflationary pressures), then it is advisable either to disregard this part of the experience, or at least to "correct" it by some method of informal appraisal. Projections so derived should be made to apply to average conditions over longer future periods, with similar implications. An entirely different kind of apparatus would be required to try to trace the "cyclical" consequences of a discrepancy between planned savings and investments. We are concerned with long-run projections, and even the

most self-confident "forecaster" of cyclical developments would presumably shy away from saying anything about cyclical impacts several cycles ahead.

The method here considered implies that the projected trend expresses conditions in which the plus and minus differences between planned and realized magnitudes have cancelled out, so to speak.[10] This, in turn, requires that the planned private investment plus the government expenditure should balance with the tax payments plus voluntary savings. It is possible, of course, to postulate that government expenditures and tax rates will be set in such a way as to accomplish this balance. This means deriving a private investment projection on the assumption of some definite degree of employment (for example, practically full employment), and adding that one condition of this amount's becoming "attainable" is that government expenditures and tax rates be set in a fashion such that voluntary savings plus tax revenues at the levels of activity in question should equal the estimated private investment plus the government expenditure. However, any agency (or individual) interested in such a projection would like to know what its probable quantitative implications are for taxation and government spending. Moreover, not every spending and taxing policy is feasible, and not every amount of taxation (let alone every kind of taxation) is compatible with the incentives required to call forth the projected output trends. Consequently, a satisfactory treatment of this problem requires an analysis of what voluntary savings may be expected at the output levels implied, and also of the feasibility of the fiscal policy which would produce monetary equilibrium at those levels. The projected investment figure is attainable only if these fiscal policies are feasible, and if the other assumptions of the individual projection (the "corrected *ceteris paribus* assumptions" of each component of the projection) turn out to be realistic.

Appraisal of these various assumptions by general "judgment" is a highly involved matter. Consequently, the logical case that can be made for using such a method for projection is not particularly strong. But this is true of economic projection in general. It is true of all attempts (including the attempts we make in our

[10] But the method does *not* imply that the plans themselves, and hence the long-run averages realized, or trends, were uninfluenced by cyclical fluctuations and the ensuing uncertainties (cf. footnote 25).

everyday lives) at analyzing highly incomplete experience with the purpose of basing our decisions on it. In all these cases, informal correction of essential *ceteris paribus* assumptions is required. The result depends just as much on these quasi-intuitive corrections as on the rigorous part of the logical analysis. In all these cases it is easy to ridicule the method, if it is measured by the standards of some highly developed natural science. But it remains a fact that we are compelled to make up our minds on a great many matters where the best we can do is to combine the analysis of experience with essential supplementary judgments of an informal kind. The general type of approach here envisaged poses very complex problems to this faculty of appraisal, and different persons are very likely to arrive at different conclusions with respect to the appropriateness of alternative appraisals. But we submit that, on the whole, the problems so posed are fairly articulate, and that the *type of analysis* of which the projections now discussed are the "engineering" equivalents has the merit of throwing light on the mechanism underlying the process of capital formation.

However, it is necessary to emphasize one consideration which is frequently overlooked in connection with the requirement of consistency between the estimate of future capital formation and hence of real saving, *as a constituent of an output estimate* (via the propensity to save), on the one hand, and the capital formation estimate derived from output trends and capital coefficients, on the other. In an automatically and fully adjusting economy, this consistency requirement would always be met. It would not have to be created by fiscal policy. This is because, in such an economy, the capital-output ratio would always assume the value corresponding to the postulate that all voluntary savings (plus whatever tax payments there are) must be absorbed by the planned private capital formation (plus whatever government expenditures are made). A given amount of savings will result in more output growth if investment opportunities are available at a low capital-output ratio than if absorption of all savings requires increasing capital-output ratios. But investment opportunities are always available at some capital-output ratio, and these opportunities would always be seized upon in a fully adjusting economy. Moreover, if all investors acted on the expectation that these investment opportunities would prove profitable, and if, in each subsequent period, they again absorbed all

savings, then these expectations would justify themselves, except in the event of specific scarcities which might preclude the completion of investment projects.[11] We all know that the economy is not fully adjusting in this sense. But it should not be assumed that the economy is fully nonadjusting in the same sense.

In other words, any inconsistency that may be found between the resulting capital formation estimate (as a constituent of an output estimate) and the output trend plus capital coefficients initially implied in the derivation of the capital formation estimate should give rise to the question of whether, in the given circumstances, some or all of the inconsistency is likely to become eliminated by the internal flexibility of the system. Short-run flexibility may be very limited, but we are here concerned with long-run trends. Long-run flexibility cannot simply be assumed away, especially if major short-run disturbances are effectively counteracted. The possibility that inconsistencies of this sort may be partly eliminated by the internal flexibility of the system should lead the investigator to pose to himself the question whether his "corrected *ceteris paribus* assumptions" should not be corrected once more, after provisional completion of his estimate (i.e., whether the first round of estimating should not be followed by a second). *This is because elimination of inconsistencies by internal adjustments is in itself a possible source of deviations from ceteris paribus assumptions.* Growth trends and trends in the capital-output ratios may become different from what they were because they partially adjust to the requirements of the system.

The problem now considered may give rise to a substantial dilemma on the policy level, because the policies appropriate to promoting internal adjustments are not, in general, identical with those suitable for eliminating inconsistencies by "compensatory fiscal policy." It is possible to overemphasize this conflict of objectives, because in some important respects the two types of policy overlap. For example, the use of monetary and fiscal policy for counteracting cumulative contractionary disturbances (to the extent that this is compatible with a reasonably stable price level) presumably promotes internal adjustments in addition to having desirable direct effects. But, if we are faced with a secular imbalance between saving and investment at desirable levels of employment, the policy suitable for the direct filling

[11] See comments on this point in Section D-1.

of gaps *may* differ from the policy appropriate to promoting balance through internal adjustments. The one may call for undertaking government investment, even if this is somewhat competitive with private investment; the other may lead to suppressing projects of this kind. The two objectives may also call for different degrees of tax graduation. These are matters which must ultimately be decided by policy makers rather than "professional persons," but economic analysis is required for describing the alternative sets of policy decisions and for weighing the likelihood of their success as measured by their own standards. As we shall see later, there is reason to believe that long-run internal adjustments have played an important part in shaping the course of events over many decades.

It follows that the internal adjustments of the system need to be taken into account *not merely* when the question is raised whether there exists an automatic tendency toward the elimination of possible inconsistencies between investment projections (as constituents of output projections) and the output projections implied in the investment projections. Even if the results are consistent, it must be remembered that the past experience so projected already reflects adjustment processes, and that this experience might have been quite different if the behavior of the cost-price data on which individual firms partly base their investment decisions had been different from those actually observable. If there exist reasons to expect differences between the past and the future with respect to price-wage relations, relative prices, the amount and the kind of taxation, the state of liquidity, the state of the capital market, international relations, and so forth, then allowances must be made for these. Only in exceptionally fortunate circumstances could there exist a satisfactory *technical* (statistical) method of measuring them.

It is impossible not to be impressed by what is involved in the quasi-intuitive allowances which were discussed in the present section. The method of approach with which we are concerned in this paper certainly does not qualify as a method of "projection" in any true sense. But as a method of guidance toward informed analysis—and, if necessary, toward informed guesswork—it still seems superior to its potential alternatives. Mechanical projections based on the equations of Section C-4 may create much confusion and they may do much harm. But an investigator who analyzes the relationships expressed in the

equations and supplements such analysis by proper discussion of the factors that stay in the background of the equations—that is, an investigator who pays proper attention to the nature of the underlying *ceteris paribus* assumptions and to possible ways of correcting these—may contribute to the understanding of the investment process. Occasionally he may also prove helpful to persons who are compelled to base decisions on highly incomplete evidence.

F. ESTIMATES OF AGGREGATIVE DATA: KUZNETS' DECADE AVERAGES FROM 1869 TO 1929 AND ROUGH SUPPLEMENTARY COMPUTATIONS FOR THE PAST TWO DECADES

The estimates published in Simon Kuznets' *National Product since 1869*[12] will here be used for computing the decade averages of the magnitudes entering into the equation

$$\frac{\Delta O/\Delta t}{O} \cdot \frac{\Delta V}{\Delta O} -$$

The ΔV term excludes the value of unimproved land and consumers' stocks and it includes government expenditures resulting in tangible assets.[13] Other government expenditures, if included in O (that is, if not interpreted as instrumental services or mere transfers), are regarded as part of consumption. O will stand for net national product, unless otherwise explained.

A few introductory comments are necessary.

1. If the equation above is used $(\Delta O/\Delta t)/O$ must mean $\Delta O/\Delta t$ divided by the O of the second of the two periods used in computing ΔO; and $\Delta V/\Delta O$ must mean the ΔV between the end-dates of two periods, divided by the change in output between these two periods. This can most easily be seen if we write the equation in the following form (with the subscripts standing for periods, such as decades):

$$\frac{O_n - O_{n-1}}{O_n} \frac{V_n - V_{n-1}}{O_n - O_{n-1}} = a_n$$

This reduces to

$$V_n - V_{n-1} = a_n O_n$$

[12] (NBER, 1946).

[13] Therefore, the "capital" concept in question includes land improvements, building, durable producers' goods, business inventories, and claims against foreign countries. Cf. *ibid.*, particularly tables II-16 and IV-10.

an equation which is correct if V_n applies to the end-date of period n, and V_{n-1} to the end-date of period $n-1$. If we want to trace through the mutual adjustments of the data in the framework of this kind of equation, then the dating must be such as is just described. But for certain purposes we may prefer to have an estimate of $\Delta V/\Delta O$ in the sense of the change between capital stock at the *midpoint* of output periods divided by the change of output between the two periods. What we want for some purposes is better approximated by

$$\frac{V_{1889} - V_{1879}}{O_{1884\text{-}93} - O_{1874\text{-}83}} \text{ than by } \frac{V_{1889} - V_{1879}}{O_{1879\text{-}88} - O_{1869\text{-}78}}$$

where the subscripts stand for years and decades, respectively. This is because the output of a longer period was more nearly produced (in some average sense) by the factors of production available at the midpoint of the decade than by the stock of the end-date. The equation will not come out right with such dating, and hence this dating does not provide a consistent framework for investigating the mutual adjustments of the terms appearing in our equation. But if we are interested in a question such as "What does experience show with respect to the relationship between capital and output?" midpoint stock estimates are more pertinent. It is not very desirable to try to answer this question in a framework which cannot be broadened into one suitable for tracing mutual adjustments. Hence we are faced here with a limitation, provided that the behavior of $\Delta V/\Delta O$ depends *very much* on whether we use midpoint or end-date data for the capital stock. All data of the first six columns of Table 1 are dated consistently with the requirements of equation 1 of Section C-4, and hence the $\Delta V/\Delta O$ data of column 4 are based on V data for the end-dates of the output periods, for example,

$$\frac{V_{1889} - V_{1879}}{O_{1879\text{-}88} - O_{1869\text{-}78}}$$

The columns following column 6 contain supplementary information not pressed into the framework of the equation, and one of these columns (7) includes $\Delta V/\Delta O$ data computed from capital stock changes at midpoints, for example,

$$\frac{V_{1889} - V_{1879}}{O_{1884\text{-}93} - O_{1874\text{-}83}}$$

TABLE 1

CAPITAL-OUTPUT AND INVESTMENT-OUTPUT (SAVING-OUTPUT) RATIOS (IN PERCENT) BASED ON CAPITAL AND OUTPUT FIGURES IN BILLIONS OF 1929 DOLLARS

Decades	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*Decades*	*(7)*	*(8)*	*(9)*	*(10)*
1869-78	9.5						1874-83			2.98	2.85
1879-88	17.9	46.9	26.8	3.19	15.0	14.5	1884-93	3.67	3.22	3.20	2.98
1889-98	24.2	26.0	39.3	6.24	16.2	16.2	1894-1903	4.46	4.38	3.58	3.39
1899-1908	37.4	35.3	51.5	3.90	13.8	13.6	1904-13	3.37	3.38	3.50	3.39
1909-18	49.4	24.3	54.4	4.53	11.0	13.0	1914-23	5.18	5.08	3.82	3.75
1919-28	68.3	27.7	62.5	3.31	9.1	10.2	1929[a]	2.01	2.30	3.18	3.26

Column	*Title*	*Explanation of Title*
1	O	Average yearly output, in the sense of "net national product, peacetime concept." Net change in claims against foreign countries is excluded. Inclusive of this, the figures would be the following, from top to bottom: 9.3, 17.9, 24.2, 37.3, 50.6, and 69.0.
2	$\frac{\Delta O/\Delta t}{O}$	Change in total output from preceding decade to decade in question divided by output of decade in question.
3	ΔV	Change in capital stock, excluding foreign claims, from end of preceding decade to end of decade in question. Stocks in possession of households and unimproved land are excluded here and in all subsequent columns.
4	$\frac{\Delta V}{\Delta O}$	Column 3 divided by change in column 1 from preceding decade to decade in question.
5	α	Increment of capital stock excluding foreign claims divided by column 1.
6	α_1	Increment of capital stock, including foreign claims, divided by output including change in foreign claims.
7	$\frac{\Delta V}{\Delta O}$	Like column 4, except that change in capital stock is computed from *middle* of preceding decade to *middle* of decade in question.
8	$\frac{\Delta V}{\Delta O}$	Like column 7, except that capital stock includes foreign claims and output includes the change in foreign claims.
9	$\frac{V}{O}$	Capital stock of middle of decade excluding foreign claims divided by output excluding the change in foreign claims.
10	$\frac{V}{O}$	Like column 8, except that capital stock includes foreign claims and output includes the change in foreign claims.

[a] The decade of 1924-33 is significantly affected by the Great Depression, which is not "smoothed out" effectively by decade averaging. Consequently, we used the data applying to the single year 1929 instead of those applying to 1924-33.

The type of behavior shown by the data of column 7 seems sufficiently similar to that shown by those of column 4 not to rule out this kind of approach on these grounds alone.

2. The capital stock estimates from which the V figures of column 3 were computed exclude the claims of the United States against foreign countries. This is because we had to take those stock estimates which Kuznets made comparable with his capital formation estimates, so that increments of the stock equal Kuznets' net capital formation. The Kuznets series, which in this sense was made comparable with capital formation estimates, does not include foreign claims. Such stock estimates are meaningful. However, the figures of column 5 (a) computed in this fashion are not directly meaningful because the savings going into foreign claims cannot be distinguished meaningfully from other savings. It was necessary to give in column 5 the a figures computed by omitting the increment of foreign claims from capital formation. The first five columns contain data that, in conjunction with each other, satisfy equation 1 of Section C-4, and hence the figures of column 5 could not include foreign claims. Column 6 contains figures which differ from the a figures in that they do include increments of foreign claims. Again it may be submitted that the difference between the behavior of the two series is not so great as to rule out this type of approach.

For the reason just indicated, the $\Delta V/\Delta O$ figures of columns 4 and 7 are based on ΔV estimates which exclude changes in the claims against foreign countries. In column 8 we give $\Delta V/\Delta O$ estimates which include these changes. This correction was made only for the "midpoint of the period" method and not for the "end-date" method. In other words, column 8 is like column 7, not like column 4, so far as dating is concerned.

3. In the framework of which this analysis is developed, equation 1 of Section C-4 does not require reliance on stock estimates (V), but merely reliance on capital formation estimates (ΔV). Absolute stock (V) estimates are affected not merely by all the sources of error which inevitably render the capital formation (ΔV) estimates unprecise, but they are affected also by further sources of error. This is because V estimates imply knowledge of the stock at the beginning of the period of analysis. Estimates of this initial stock are very unreliable. However, in column 9, we give estimates of "stock per unit of output" (V/O) which were computed from the stock and the output estimates pub-

lished in Kuznets' *National Product since 1869*. The stock estimates on which column 9 is based are like those from which the increments appearing in column 2 were derived, except that we used the stock estimates applying to the midpoint of each output decade rather than those applying to the end-date. Column 9 is not required for tracing adjustments in the framework of our equation, and for all other purposes midpoint dating seems more desirable. The V figures, which constitute the numerators of the data of column 9, are those made comparable with Kuznets' direct estimates of net capital formation. Hence, they exclude claims against foreign countries.

From decade to decade there was a considerable amount of variation in all magnitudes entering into our equation. The rate of growth of output, the marginal capital-output ratio, and a (also a_1) varied a good deal during this period. There is no justification for interpreting the figures of the table as equilibrium magnitudes in any strict sense because short-run instability influences the planned as well as the realized long-run magnitudes of the economy. Average values over the cycle do not show what would have happened in the absence of cyclical disturbances. Trend values are not equilibrium values. But there surely exist strong reasons for believing that plus and minus differences between planned and realized magnitudes partly "cancel" in the long run. Realized data tend to come closer to planned data in the long run than in the short run, even though the long-run planned data themselves (e.g., decade averages) may be significantly influenced by the fact that everybody is aware of the uncertainties connected with cyclical fluctuations.[14] The data included in Table 1 may be interpreted as expressing this partial realization of error-cancelling tendencies. In this limited sense it is permissible to speak of mutual adjustments, rather than merely of changes from period to period, in the basic data of the table.

The main change in the rate of growth of output (column 2) appears to be retardation, if the first figure—that pertaining to the transition from the seventies to the eighties—is included in the comparisons. If the first figure (46.9 percent) is disregarded—that is, if the comparison is limited to the subsequent four figures—no clear case can be made for retardation, but there still remain oscillations of some significance. The a and a_1 figures show a downward tendency beginning with the turn of century.

[14] See footnote 25.

The marginal capital-output ratio shows considerable fluctuations, only part of which can be explained away with reference to special circumstances. The abnormally high figure for the transition from the eighties to the nineties (especially in column 4) may be partly a consequence of the chronically depressed character of the nineties. That is to say, for the present purpose, decade averaging, which so very obviously does not smooth out the Great Depression of the 1930's, may be more inadequate for the 1890's than for the other decades included in the table (although less inadequate for all these decades than for the 1930's). Furthermore, it may be argued that the very high figures obtained in columns 7 and 8 for the transition from 1904-13 to 1914-23 result partly from the circumstance that the decade output of 1914-23 is affected by the postwar depression, while the corresponding capital stock estimate for 1919 is not. This, however, is not a completely convincing argument in itself, because, aside from this, one would expect to get a low $\Delta V/\Delta O$ figure for the incremental ratio leading into the World War I period, while we obtain a somewhat high (rather than a somewhat low) figure even in column 4, where the output is not affected by the postwar depression. It is conceivable that the remaining difference could be accounted for adequately by excluding from ΔV that part of the government investment which did not increase the "normal" capital stock of the economy. Finally the very low last figure in column 7 (also in column 8) may result partly from the arbitrariness of using the data of a single year (in order to avoid bringing in years of the Great Depression). Tentative calculations made for other years of the late 1920's lead us to believe that the choice, for the sake of symmetry, of 1929 as the relevant single year somewhat lowers the figure as compared with possible alternative choices. But, regardless of how much can or cannot be explained away if enough skill is used, the $\Delta V/\Delta O$ columns must be said to show considerable variation.

The table has not been brought up to date because decade averaging clearly does not perform adequately for our purpose in the 1930's or in the 1940's. Yet, it seems justified to add a few comments on the behavior of the capital-output relationship in the course of the past two decades.

During the Great Depression there existed substantial excess capacity, and spuriously high figures would be obtained for the average capital-output ratio. These could be used only as rough

indicators of idle capital. In the late thirties—in years such as 1937 and 1939—both real output and the aggregate stock were approximately at their predepression levels. This is true if we compute the stock (as we have done throughout this analysis) by adding the price-deflated market value of net capital formation to the "initial stock." In efficiency units the real capital of 1937 or 1939 may well have been greater than that of 1929. But this is irrelevant for the purpose of V/O (or $\Delta V/\Delta O$) computations because measurement in efficiency units necessarily (tautologically) results in unitary ratios. If we measure the stock by the method employed in the present analysis, the average capital-output ratio of the late thirties must have been similar to that of 1929. During World War II, output rose very substantially and capital stock increased in a much smaller proportion. The capital stock of the economy was abnormally "fully" utilized. Since the beginning of the postwar period, the capital stock has risen more rapidly than the output flow, but computations of the sort here used would undoubtedly show that, at present, the *average* capital-output ratio is still well below the prewar figure. The use of Department of Commerce estimates for recent additions to the capital stock leads to this conclusion. Similar conclusions could be based on preliminary estimates by Mr. Raymond W. Goldsmith.[15]

In a crude appraisal of general orders of magnitude, we may say that the stock of the late twenties can be valued at about $280 billion in 1929 prices,[16] or at close to $500 billion in the prices ruling at the outbreak of the Korean war in 1950. The net private capital increment of the period 1929-49 corresponds to roughly $100-120 billion in mid-1950 prices, with allowance for reconversion of wartime capital. In contrast to approximately a 20-25 percent increase in the stock, there has occurred from 1929 to the Korean outbreak approximately a 75 percent increase in the national product deflated for price changes. The V/O so computed comes close to 2.7 with foreign claims included, as in column 10 of Table 1. Inclusion of the postwar capital formation in the government sector might raise the ratio to perhaps 2.8.[17]

[15] International Association for Research in Income and Wealth, *Income and Wealth in the United States*, Income and Wealth Series II (Cambridge, England: Bowes and Bowes, 1952).

[16] On the basis of the Kuznets series from which the data of columns 9 and 10 of our table were computed.

[17] The ratio approximating 2.7 results from a stock valued at close to

In the *immediate* postwar period, the ratio was much lower because the bulk of the 1929-49 capital formation occurred in the postwar years. For a comparison of the late thirties with the first half of 1950, the $\Delta V/\Delta O$ is about 1.0-1.2, the numerator being \$100-120 billion and the denominator about \$100 billion in prices obtaining immediately prior to the Korean war. Here, again, an upward correction should be made for the postwar capital formation in the government sector. Consequently, 1.2-1.5 would seem to be a reasonable estimate for $\Delta V/\Delta O$.

An aggregative marginal capital-output ratio of such small size implies one of two things. One possible implication is that at the end of the forties there still existed a significant backlog of capital in the American economy. The $\Delta V/\Delta O$ figures obtained from a comparison of capital and output in successive postwar years (or subperiods) during the second half of the forties are very high, because the economy was obviously making up for a backlog at that time. It is quite possible that at the end of the decade there still existed an important backlog and that this explains why the $\Delta V/\Delta O$ ratio computed from the late thirties to mid-1950 is 1.5 or less. This would mean that producers were aiming at higher capital-output ratios than those statistically observable, but that they had had no time to realize these ratios (or to catch up with the output trend, so to speak). They were merely on their way toward more desirable capital-output ratios.

This interpretation could be based on the fact that past experience points to a considerably higher ΔV requirement per unit of ΔO than the $\Delta V/\Delta O$ observable for a direct transition from the late twenties to the end of the forties. Among the $\Delta V/\Delta O$ figures leading into the decade of the twenties, the last figure of column 8 of our table would seem to be most nearly suitable for contrasting with the unitary $\Delta V/\Delta O$ now considered. This figure is 2.3. It is less influenced by the inevitable arbitrariness of price deflation techniques than are the data for the earlier decades, since all data are expressed in 1929 prices. Furthermore, recent Kuznets estimates which relate to various in-

\$620 billion and a net output of about \$230 billion. The 1929-49 capital formation estimate of \$100-120 billion does not include the postwar government (public) investment. In the period 1919-39, between 15 and 20 percent of the total gross capital formation was in the government sector. The share in net capital formation must have been greater, but if this relationship has not become entirely different for the postwar years, the average capital-output ratio so corrected can scarcely exceed about 2.8.

dividual sectors of the economy make it appear likely that the $\Delta V/\Delta O$ figures leading into the twenties are not significantly influenced by changes in the relative weights of these sectors. Therefore, we may say that as we move into the twenties, the observable over-all $\Delta V/\Delta O$ ratios express pretty well the then "normal" tendencies manifesting themselves in the individual sectors of the economy, and that these tendencies would have had to change radically if the more recent ratio of about 1.5 or less were to be interpreted as being "normal" now. If no radical departure from observed *past* tendencies is assumed, the conclusion would be that at the end of the forties the economy was still making up for a capital backlog.

We are inclined to the view that this inference is realistic, but the foregoing reasoning is inconclusive. It is conceivable that the experience of the twenties, and of earlier decades, should be interpreted as indicating a gradual fall in $\Delta V/\Delta O$. This interpretation, too, is compatible with the data of the table. Moreover, it is conceivable that, since the late twenties or thirties, we have been experiencing shifts in the composition of output such as would result in the lowering of the "normal" over-all $\Delta V/\Delta O$, even if the "normal" $\Delta V/\Delta O$ of the individual sectors should not be declining. It is, therefore, not obvious from what has been said so far that past experience points to the likelihood of a $\Delta V/\Delta O$ ratio exceeding 1.5, and, hence, that it points to the existence of a considerable capital backlog at the end of the forties.

Yet we believe that the hypothesis of a backlog is supported, although not proved, by data concerning individual sectors of the economy. Professor Kuznets made these data available to the writer in the form of preliminary estimates. Recently some of these were published.[18] These estimates show a markedly rising long-run tendency for the ratio of fixed capital to output in manufacturing and agriculture, sectors having comparatively low ratios (lower than that applying to the economy as a whole).[19] The data show no appreciable trend for the other sectors with comparatively low ratios, and a markedly falling

[18] International Association for Research in Income and Wealth, *op.cit.*, pp. 117ff.

[19] The estimates for 1938 (*ibid.*, pp. 122 and 127) should probably be disregarded because the corresponding output period (1934-43) includes a period of substantial "overutilization" of the capital stock. However, if not disregarded, the 1938 figures point to a reversal of the trend.

trend for the ratios in several sectors with high ratios (higher than that applying to the economy as a whole). One possible explanation would seem to be that in the high sectors cost saving largely *means* reducing the ratio of capital to output, while in the low sectors innovations may pay even if they go with some increase in the capital-output ratio provided they reduce the labor-output ratio sufficiently. At any rate the ratios for manufacturing and agriculture have been rising. If our aggregative $\Delta V/\Delta O$ ratio were to become stabilized at a "normal" level of 1.5 or less, then our aggregative V/O ratio would also have to tend gradually toward this level. This would imply an unlikely rearrangement by which the ratios for typically high sectors (utilities, transportation, construction) fall below the ratios for manufacturing and agriculture, *unless* of course the rising trend for these low sectors also should give way to a falling trend. At present, these sectors have ratios which exceed 1.5.

In our judgment, the argument pointing to the abnormality of a $\Delta V/\Delta O$ ratio of 1.5 or less—that is, the argument pointing to a pre-Korean capital backlog—is fairly strong. Nevertheless, we shall consider 1.5-1.0 the lower limit of the range of the $\Delta V/\Delta O$ values, the implications of which are still worth examining.

G. THE PROBLEM OF PROJECTION

What conclusions could have been drawn from these data, if in the summer of 1950 the war in Korea had not created a new situation?

Roughly speaking, past experience points to an average yearly increase in man-hour output of about 2.5 percent. Considering that the labor force is rising at a yearly rate of more than 1 percent, but that (aside from acute emergencies) yearly hours of work per man may perhaps be expected to show a mildly declining tendency, let us initially assume an average yearly rise in output of 3 percent. This is a very rough guess, but, with qualifications to be added later, adequate as a point of departure for the present purpose.[20] In the "long run," more pretentiously in-

[20] Preliminary estimates point to the following rates of increase of output per man-hour in the American economy as a whole: from 1890 to 1900, 25 percent; from 1900 to 1920 (20 years), 66 percent; from 1920 to 1930, 30 percent; from 1930 to 1940, 33 percent; from 1940 to 1949 (nine years), 24 percent. In the writer's opinion, preliminary estimates point to the likelihood that hours per week would tend to fall by a smaller percentage

terpreted (in the secular long run, so to speak), the main question would be that of the fundamental continuity of Western socio-economic institutional development and of the continued ability of the economic system to produce the structural and technological changes which have given rise to the observed trends. But, at present, we are concerned with a shorter "long run." We shall assume no break in the basic trends. In fact, some of these have proved very resistant to considerable environmental change.

The GNP (gross national product) of the first half of 1950 was about $270 billion at an annual rate, so that even a $\Delta V/\Delta O$ ratio of 1.0-1.5 would have corresponded to net private capital formation of roughly $10 billion[21] and hence to gross private capital formation of about $30 billion. It would have been reasonable to project government expenditures on goods and services at a yearly rate of about $40 billion (the rate immediately prior to the Korean war), and initially to assume that these will be tax-financed. The experience of the late forties and of the first half of 1950 points to the likelihood that, in such circumstances, reasonably full employment would have required a greater amount of gross private capital formation than the $30 billion resulting from the foregoing calculations. It probably would have required about $10-15 billion more than the figure at which we have arrived.[22]

Crude as this calculation is, it points to the likelihood that, aside from disturbances so far disregarded, the system would have tended to operate at high capacity. Waiving at first the question of whether the initial $\Delta V/\Delta O$ figure is not too low—that is, of whether the figure of 1.0-1.5 should not be replaced by a higher figure *even before* adjustments of the system to

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than that by which the labor force was expected to rise during the next few years.

[21] Being here concerned merely with *private* capital formation, the argument of Section F would make the lower limit fall in the range 1.0-1.2 rather than 1.0-1.5. The figure of $10 billion corresponds to 1.2.

[22] In the inflationary peak year 1948, personal consumption expenditure was $177.9 billion; gross private domestic investment, $42.7 billion; net foreign investment, $1.9 billion; and the government purchase of goods and services, $36.6 billion. In the mild recession year 1949, personal consumption expenditure was $180.2 billion; gross private domestic investment, $33.0 billion; net foreign investment, $0.5 billion; and the government purchase, $43.6 billion. In the first half of 1950, personal consumption expenditure was $186.7 billion at an annual rate; gross private domestic investment, $44.0 billion; net foreign investment, $—1.6 billion; and the government purchase, $40.7 billion.

equilibrium requirements are considered—the conclusion would seem to be that it would have taken a government deficit of perhaps $15 billion to raise total expenditure by the amount of the deficiency. However, if probable internal adjustments are taken into account, the deficit (tax reduction or deficit-financed additional expenditure) required for reasonably full utilization would presumably have been smaller (*perhaps* even zero). In the first place, it seems very unlikely that, under conditions characterized by a tendency toward insufficiency of investment opportunities, the accumulation of undistributed corporate profits would have continued at an unchanging rate. Distribution of all profits might have eliminated much the greater part of a $10-15 billion deficiency, because, in the periods of high activity immediately prior to the Korean war, undistributed corporate profits were accruing at an average yearly rate of $13 billion. Even considerable reduction of the undistributed percentage of all profits would have brought the deficiency below the $10 billion level.

Furthermore, other internal adjustments might also have taken place. Even if the $\Delta V/\Delta O$ ratio of 1.0-1.5 were an adequate point of departure, this marginal ratio might well have shown a tendency to rise in response to unused investment opportunities requiring higher ratios; or the growth-rate of output could have become correspondingly higher, with the reduced capital-requirement per unit of growth. After all, past experience does not point to rigid behavior of this ratio. It points more to mutual adjustments of the variables included in our basic equation. It is impossible to tell whether these various adjustments would have merely reduced a $10 billion deficiency, thus reducing the need for a deficit, or whether they would have practically eliminated it. This would have depended partly on factors such as credit policies, wage policies, and tax policies, and, in general, on how successfully the proper balance would have been reached between the stimulating effect of mass purchasing power (high propensity to consume), on the one hand, and the required incentive effect of inequality, on the other. Assuming reasonably favorable surrounding circumstances, the order of magnitude of the deficiency might well have declined far below $10 billion per year, even with a balanced budget. Moreover, as a consequence of rising productivity, an average yearly budget deficit of about $7 billion might have merely prevented the public debt from falling in relation to the national income; and an average

yearly deficit of somewhat more than this might still not have raised the debt burden in a higher proportion than that in which the tax revenue would have tended to rise at unchanging tax rates and constant prices. This is not to say that stabilizing the proportion of the debt burden to the (rising) national income at its pre-Korean relative level would have been desirable. But it is submitted that even the highly "pessimistic" (in the sense of low) assumption of a $\Delta V/\Delta O$ ratio of 1.0-1.5 would not have pointed to a seriously deflationary long-run problem.

Considering that the relevant data for this argument are percentage rates and ratios, and that their relative magnitudes in question would not seem to depend *much* on the absolute level of output (within reasonable limits), the outcome is not *appreciably* affected by using the initial rather than the terminal data of (say) a five-year period.

In an analysis aiming at numerical precision, this statement would have to be made subject to qualifications of which only one will be explicit here. If, aside from the war in Korea, the aggregate government expenditure had remained numerically stable, or had risen less than output, then, with consumption and private investment accounting for a stable proportion of output, the "deficiency" would have tended to be greater at the end of a longer period than at its beginning. While, on the assumption of balanced budgets, tax revenues like government expenditures would have risen less than output, this in itself would have been an incomplete offset to the decline in government expenditures relative to output because part of the tax saving would have tended to go into private savings rather than consumption. But qualifications of this kind are of small consequence in an appraisal of rough orders of magnitude over a period of (say) five years.

If the initially assumed $\Delta V/\Delta O$ is raised, then it becomes necessary to distinguish between a prospective period of gradually diminishing backlog and a subsequent "normal" period. Even without "internal adjustments," the $10-15 billion initial deficiency resulting from the foregoing calculation is eliminated for a normal period if we assume an over-all $\Delta V/\Delta O$ ratio of about 2.5. On the basis of the past experience here discussed, it is readily conceivable that there has been a tendency in this direction and that, after making up for the backlog, the economy would have operated in such circumstances. If the "normal" $\Delta V/\Delta O$ actually was in this order, then a neatly balanced pic-

ture may be drawn by the aggregative method here employed. But the picture would become balanced only for a more distant period in which the economy would have already made up the backlog. In the meantime, the economy would have had to go through an inflationary period. This is because, on this assumption, the economy did not in the late forties possess the capital normally corresponding to the simultaneous output. It would have had this capital if the net capital formation of the preceding two decades had been about twice as great as was actually the case. The tendency to catch up would have continued well into the fifties. The length of time it would have taken to eliminate the backlog would have depended partly on the nature and the effectiveness of the anti-inflationary policies adopted. These policies might have slowed down the speed of the catching up process. It is also conceivable, however, that the existence of the backlog would have reduced the secular rate of increase in productivity and output, and that more of the capital formation compatible with reasonably stable prices could have been devoted to the gradual elimination of the backlog.

So far we have considered the possibility that the "normal" marginal capital-output ratio was in the order of 1.0-1.5, i.e., that it was equal to the actual ratio computed from a comparison of the late thirties with the late forties, and we have also considered the possibility that the normal ratio was at the level of about 2.5 (the actual ratio of the twenties or slightly higher). On the first assumption, the crude, aggregative method employed led to deflationary initial results, but the deficiency was of a size suggesting the likelihood of gradual internal adjustments (especially with a moderate budgetary deficit). This assumption is definitely pessimistic with respect to the normal $\Delta V/\Delta O$. On the higher assumption concerning this ratio, which might be somewhat "optimistic" in this special sense of the word, the calculations pointed to an inflationary basic tendency over a transitional period of several years and subsequently to a balanced situation. Given the crudeness of these computations, it is not necessary to develop the consequences of specific intermediate assumptions concerning $\Delta V/\Delta O$ or of specific assumptions placing $\Delta V/\Delta O$ beyond 2.5. At some level between the 1.0-1.5 range and the 2.5 level, this kind of analysis would point to a transitionally balanced situation (owing to the backlog), and subsequently to a mildly deflationary initial tendency calling for internal ad-

justments or, in the absence of these, for a deficit. At levels higher than 2.5, these same calculations point to a strongly inflationary tendency during the backlog period and to an inflationary tendency even thereafter. Such inflationary tendencies may also call forth internal adjustments (increased corporate savings, a lowering of the increase in productivity owing to shortages, etc.), and, in the event of the insufficiency of these, they would secularly justify more deflationary monetary and fiscal policies than those on which the initial projection is based.

We do not believe that it is possible to arrive at more definite conclusions without relying heavily on subjective appraisal and judgment. In cautious and general terms, our own appraisal of these results of aggregative saving-investment analysis is that the so-called long-run outlook (over, say, a period of five years) was not deflationary, even aside from the Korean war. More specifically, our own "best guess" would have been that the inflationary period of backlog demands would have continued for some time, and that subsequently the economy might have approached a rather balanced situation. But at any rate, from the materials so far used, *it would be difficult to substantiate a heavily deflationary projection unless consistently "low" assumptions are combined with great pessimism concerning the ability of the system to adjust to rather moderate disturbances.*

Before turning to a brief discussion of the general assumptions which underlie these conclusions, we shall merely raise—rather than analyze—a question and we shall refer to the later context where an analysis follows. Are the foregoing conjectures very much influenced by the size of the depreciation allowances which are implicit in the numerators of our $\Delta V/\Delta O$ ratios? This is an important question because depreciation allowances are inevitably arbitrary. Later, in a discussion of the methods employed by the Council of Economic Advisers, a presumption will be established that the preceding results are not decisively affected by this arbitrariness. But, before considering this matter, we shall first cast a critical eye on the nature of the theorizing in which we have engaged.

It can scarcely be overemphasized that these conclusions share all the shortcomings of trend analysis which disregards the cyclical path "around the trend."[23] They also are subject to the sig-

[23] Although it does not disregard the fact that plans, and hence long-run average realized values (trends), are influenced by cyclical fluctuations and

nificant qualifications of all aggregative, saving-investment analysis which disregards (or hides behind a *ceteris paribus* clause) the problems of specific resource requirements and of relative prices. We shall now briefly turn to the necessary qualifying considerations.

As for the qualifications necessary in consequence of the "abstract" character of the trend concept, these express themselves in the fact that cyclical disturbances may become self-reinforcing and may seriously impede the adjustment of the system to the long-run requirements of balanced development. The kind of analysis contained in the preceding pages assumes, not only that random disturbances become smoothed out into a trend line the basic properties of which are independently meaningful (rather than simply the resultants of cyclical forces), but also that, along the trend line itself, threatening discrepancies and gaps tend to call forth certain adjustments (rather than to grow cumulatively).

One could try to take care of this problem by simply stating that a reasonable cycle policy is implied. To be sure, opinions differ on the details of a reasonable cycle policy. But no one would expect this kind of analysis to try to answer all questions encountered on the way and there exists workable consensus on the desirability of counteracting cumulative inflationary and deflationary movements by means of monetary and fiscal policy. However, the problem now posed is not fully met by such references to reasonable cycle policy. This is mainly because the absence of a reasonable cycle policy in this sense may have appreciably affected our past trend data in some periods. In fact, the reason for treating the data for the 1930's and 1940's on a different footing from those of the preceding decades is precisely that, in the sense here relevant, decade averaging does not smooth out the cycle for the past two decades. The thirties were a decade of chronic excess capacity and the forties a decade dominated by the capacity shortages of the war and postwar years. But in what way was the cycle smoothed out during the earlier decades? Certainly not in the same way in each of these. Do we want to imply that, in future decades, the relationship between trend and cycle will be the same as in any one of the past decades

by uncertainty. What the method does imply is that *long-run* average, realized values approximate the planned magnitudes. In other words "plus and minus" errors are assumed to show a tendency to offset each other, but they are not assumed to leave the plan uninfluenced. Uncertainty affects the plans of the public.

or similar to what it was in certain past decades "on the average"? We scarcely want to imply this. The problem is of considerable significance because the trend and the cycle do not live independent lives. They interact.

If cycle policy should become much more successful than was the case in the past, then this would presumably lower the V/O and $\Delta V/\Delta O$ ratios as compared with the pre-1929 observations. It is reasonable to assume that, on the average, there existed varying degrees of excess capacity during those decades, and that the average degree of excess capacity was not negligible in any one of the decades in question. Moreover, it seems reasonable to assume that, from decade to decade, the additions to required capital were associated with additions to the (absolute) amount of excess capacity, since excesses of this sort should be looked upon in terms of proportions (relative sizes) rather than as absolute magnitudes. Hence, excess capacity probably affects the earlier findings concerning $\Delta V/\Delta O$ as well as V/O. Projecting less excess capacity for the future may imply reducing the projected $\Delta V/\Delta O$. It would be difficult indeed to make "proper allowances" for this.

Moreover, if such allowances were made, it would also become necessary to make allowances for the increased rate of output expansion, which is another likely by-product of successful cycle policy. This other by-product depends very much on the methods of policy. Certain kinds of "stabilizing" policy might weaken incentives and thereby might result in a reduced rate of secular growth. However, in the writer's view, this is not likely to prove true of stabilization policy in general, and *if no specific discouraging effects emanate directly from the methods of interference,* then the consequences of the *results* of these policies—namely of greater stability itself—are likely to be trend-raising (rather than -lowering).[24] In this case, uncertainty is diminished and uncertainty is a factor tending to reduce the rate of expansion. In other words, truly successful stabilization policy might tend to reduce $\Delta V/\Delta O$ and to raise $\Delta O/\Delta t$. Whether it would therefore tend to leave the product of these two terms (the rate of investment) approximately unchanged, or whether

[24] In other words, given the characteristics of past full-employment periods, high-employment expectations without much dispersion are likely to induce more secular growth than strongly fluctuating expectations.

it would tend to change it up or down, is a question about which no definite conclusion is suggested.

Another problem disregarded in our crude aggregative approach is that of specific scarcities and of the cost structure. The statistical technique by which projections may be obtained of the composition of output at rising levels of the aggregate is that of the input-output analysis. Any version of this technique has its inevitable *ceteris paribus* assumptions, as do all other types of predictive analysis in economics. Here, as elsewhere, this calls for corrections based on general information and judgment. But the input-output approach leads *toward* forming an opinion of the specific composition of output while the methods so far considered simply bypass this question. Whether the specific commodity and service requirements of projected aggregate outputs can be met by specific dates is a problem that must be subjected to analysis of its own kind before the results of aggregative analysis can be definitively accepted. The present paper is not concerned with this problem, and consequently the analysis must be made subject to an "if" clause with respect to the availability of specific resources. As viewed from the pre-Korean angle, it seems to us that no difficulty that might have tended to arise in connection with specific resource requirements would have seriously interfered with the normal growth trends of the American economy over several years. But this is a distinctly subjective statement and it is no substitute for detailed analysis.

Relative price and cost structure problems enter not merely as a consequence of possible scarcities in the ordinary sense (natural scarcities), but, also, in view of changing degrees of monopoly and of bargaining power (institutional scarcities). It is a well-known weakness of the contemporary aggregative theories that they have little to say on the interaction between relative prices, on the one hand, and aggregate output and employment, on the other. For example, changes in labor cost per unit of output value probably exert two influences which go in opposite directions, but need not always (or even ordinarily) cancel. They change profit margins per unit of output and thus, *ceteris paribus*, they tend to change the willingness to produce. But the *ceteris paribus* condition is not satisfied, because these same changes also tend to change the propensity to consume in the opposite direction, and the likelihood of high employment depends to some extent on the height of the propensity to consume

(i.e., on what share of the full-employment output would be consumption and what share would have to be investment).[25] It is necessary to be acquainted with the distinctive characteristics of the *ad hoc* situation in which this problem develops to venture even a very tentative judgment on how the intensity of these two opposing effects may compare. The same is true of the effects of changes in the (relative) commodity price structure. The vague implication of the aggregative method is that the factors pertaining to relative costs will continue to affect the aggregates approximately in the same way as was the case in the past. In some respects, this may not be a bad assumption, in view of the rather stable long-run behavior of certain relationships, e.g., of the share of employee compensation in national income. And yet *ad hoc* problems of wage policy, farm price policy, and perhaps especially of tax policy call for current appraisal in terms of the opposing effects just considered. It is easy to describe these effects qualitatively, but they cannot be gauged by scientific methods.

As for the problem of financing the necessary amount of investment, the deviations from the *ceteris paribus* conditions of aggregative projections are in the upward direction. Comparisons of recent liquidity ratios with those pertaining to the pre-1929 period quite generally lead to the conclusion that during the past decade the economy reached a very high degree of liquidity. This is true regardless of whether we examine the ratios of liquid assets to liabilities of enterprise (corporate and other), or whether we turn our attention to ratios of income to private debt. Given the Federal Reserve support-price policy, the ability of the banking system to acquire reserves for additional loans is also a well-known expansionary factor (inflationary or counterdeflationary factor, as the case may be). Furthermore, any projection of undistributed profit ratios resembling those of recent years would lead to the further conclusion that a considerable proportion of the expansion could be currently financed out of profits.

In the foregoing pages we looked at the problem of long-run projection from the vantage point of the pre-Korean scene. An armament program such as that which has emerged under the changed conditions gives rise to problems to which the present approach is inapplicable. The success of an armament program of this sort depends on specific resource requirements, and on the

[25] Consumption is a more stable (dependable, predictable) constituent of output than is investment.

ability of the market forces and of regulations to mobilize the available specific resources. The result expresses itself in aggregate data and aggregative relationships, but there exists no reason to assume that these will bear resemblance to the data derived from the analysis of past long-run trends, or that, in a study of the economic war potential, it is profitable to use long-run aggregative experience concerning capital-output ratios even as a point of departure.

We know from the last war that it was possible to force the capital-output ratios to exceedingly low levels. However, it is scarcely possible to express these results of the war economy in numerical terms. One might argue that the wartime ratios were considerably lower than even the abnormally low postwar ratios, because in no postwar year was the GNP as high in constant prices as at the peak of the war effort, while there was a great deal of net capital formation during the postwar period. But, on the other hand, wartime goods and services are produced partly with the aid of tools and supplies which are not taken into account in the usual estimates of the capital stock. The "productivity" of these does not outlive the wars during which they were used.

H. COMPARISON WITH CONCLUSIONS REACHED BY THE COUNCIL OF ECONOMIC ADVISERS

1. *Similarities and differences*

The *Annual Economic Review* of January 1950 presented a full-employment model for the year 1954. This pioneering study, which was undertaken prior to the Korean crisis, arrived at a GNP figure of $300-310 billion, at consumption expenditures of $210-225 billion, and at gross private domestic investment (excluding inventory accumulation) of $38-43 billion. All dollar figures were expressed in 1949 prices. The model was developed for the year 1954, but it should presumably be interpreted as applying to average conditions over a number of years; or to 1954 itself, on the hypothesis that that year turns out to possess the average characteristics of the period by which it is surrounded. The underlying estimates of the Council "came out right" (i.e., satisfied the internal consistency requirements) at a GNP of $305 billion, with $217.5 billion of personal consumption expenditures, $42.5 billion of gross private domestic investment

(including $2.5 billion of inventory accumulation), $3 billion of net foreign investment, and $42 billion of government purchases of goods and services.

The Council called this a goal based in many respects on hypothetical forecasts. It did not call it simply a forecast, because the Council wished to emphasize the fact that its estimates did not directly lead to these figures satisfying the internal consistency requirements at the prospective full-employment level. The estimates resulted in figures that were several billions lower, and, in the Council's view, called for certain policies by which private investment and consumption could be increased to these prospective full-employment levels, where "full employment" means a frictional, seasonal, etc. unemployment of about 3.5 percent of the labor force. On the assumption that these policies will be adopted, the Council would presumably have been willing to call these projections "forecasts," although with due emphasis on the tentative character of forecasts of this sort. The Council suggested that increasing the amount of residential construction by means of a housing program would be required to bring private investment to the appropriate level of $42-43 billion, and that the propensity to consume would have to be increased by raising the share of employee compensation in national income to bring personal consumption expenditure to the required level of $217-218 billion. The implied rise in the relative share of the compensation of employees is between 3 and 4 percentage points (from 62-63 percent of national income in the period when the computations were made to about 66 percent).

In what respects are these results similar to ours and in what respects are they different?

The main difference is that the Council arrives at a more definite level of future output than we did, thereby also implying definite views on the effect of the recommended policies, which include measures directed at the redistribution of income. In the absence of these policies, the Council expects underutilization. No allowance is made for the possibility of internal adjustments of the system by which this initial tendency might be counteracted or eliminated. The analysis of the present paper, on the other hand, points to the conclusion that the level of the future output which will tend to become established depends largely on whether the projected $\Delta V/\Delta O$ lies nearer the lower or the upper end of a plausible range for this ratio; and that, in the

neighborhood of the lower end (but only in that neighborhood), we get an initial tendency toward underutilization *which may very well become offset by automatic internal adjustments of the economic system.* Nor do we believe that it is possible to make statements of general validity on the total effect of redistribution for a long period ahead. In our view, categorical statements on the relative strengths of the favorable "propensity to consume effect" of redistribution vs. its unfavorable "profit margin (incentive) effect" cannot be adequately supported. Given all characteristics of a specific period, a person may suggest partly intuitive conclusions on the relative weight of these two effects *hic* and *nunc.* Even the qualified validity of statements of this sort is apt to vanish, if they are projected into the more distant future.

The differences so far considered may be expressed by saying that the Council foresaw the necessity of antideflationary, long-range, full-employment policies (quite aside from cycle policies) and placed trust in the effectiveness of the specific kind of long-range policy which it recommended. The present analysis places more emphasis on the possibility of automatic internal adjustments *within the ranges in which these are likely to be required* (if the initial tendency should be toward chronic underutilization). It should be added that the Council does not seem to have felt that there was an appreciable capital backlog at the end of the forties, while we are inclined to believe that there still was a backlog of some significance. However, the *lower end* of our "plausible" range for the future $\Delta V/\Delta O$ implies no backlog. Projections based on these low $\Delta V/\Delta O$ ratios lead to an initial underemployment tendency in our framework, too, and the size of this tendency does not seem to be very different from that which would have been obtained by the Council without its policy recommendations. Our conclusion was that internal adjustments may very well turn out to take care of this tendency, although we did not exclude the possibility of a situation calling for upward trend correction by means of fiscal policy.

The methods of projection used by the Council are different from those applied in the present paper. It is impossible to prove the statement that, aside from its policy recommendations, the Council would have arrived at a gap which is not very different from that obtained in the low ranges of our own projections. Yet this is likely for reasons to be considered.

The Council projected domestic investment by alternative

techniques. One of these was based on the McGraw-Hill survey.[26] The answers obtained from the firms included in the survey made it possible to indicate the planned investment outlays per (planned) percentage point increase in manufacturing capacity, and also the planned replacement and modernization outlays per unit of existing capacity. On the assumption of a 3.2 percent yearly increase in manufacturing capacity (which, according to input-output-study estimates of the Council, corresponds to a 2.4 percent yearly increase in GNP), the sum total of manufacturing gross investment outlays was estimated and it was assumed that the other constituents of nonfarm plant and equipment outlays would rise beyond 1949 in the same proportion as the manufacturing outlays.

Another technique used by the Council was based on a statistical relationship between the *increment* in the cumulated sum of gross nonfarm plant and equipment outlays and the *increment* in privately produced nonfarm GNP (both in 1939 prices). The *ratio* of these two increments seems to have oscillated around a "normal" value during the 1920's. In other words, if the cumulated total of gross nonfarm plant and equipment outlays is measured on the ordinate, and the privately produced nonfarm GNP on the abscissa, then a linear relationship is obtained for the 1920's, and the line connecting the point for 1919 with that for 1929 is very nearly the same line as that expressing the best fit for all points of the decade. (The ratio of increments described in the first sentence of this paragraph is the slope of such a line.) It was found that the points for 1941, 1949, and 1950 lie very close to the line going through 1919 and 1929, while the thirties lie above the line, thereby indicating excess capacity, and the forties (after 1941) below the line, thereby indicating capital shortages. If, for this reason, it is assumed that by the end of the forties the economy had arrived back to the normal relationship and would stay on it in the predictable future, then, for the projected long-run rate of increase in output, an estimate is obtained of gross investment outlays. This estimate is not affected by where (from what year on) we start cumulating the nonfarm plant and equipment outlays to obtain the "normal" line expressing the relationship between cumulated investment outlays and private nonfarm GNP. Nor does this arbitrary de-

[26] *Business Plans for New Plants and Equipment*, published annually in February by Department of Economics, McGraw-Hill Publishing Company.

cision affect the finding that at the end of the forties we were back at the normal line. The relevant conclusions depend merely on the slope of the line and not on the constant in its equation.

The techniques based on the McGraw-Hill surveys and the regression technique just discussed lead to practically identical estimates of yearly nonfarm plant and equipment outlays in 1954. These estimates lie between 22 and 25 billions of 1949 dollars. The total gross private domestic investment requirement for full employment is estimated at around $42-43 billion, a figure which includes residential construction (as well as inventory accumulation and farm investment).[27] The Council believes that the $42-43 billion will be forthcoming in the event of a successful housing program. How much less would be forthcoming in the absence of such a program cannot be read conclusively from the computations. But the computations do show that the residential construction which, according to the Council, is required to reach the total of $42-43 billion is no more than about $3-4 billion greater than the actual yearly residential construction of the late forties. Consequently, we have a strong indication that, in the absence of the housing program, the Council's investment deficiency would be *much* smaller than $10-15 billion (which is our initial deficiency obtained by using the $\Delta V/\Delta O$ ratio corresponding to the lower end of our range).

The total investment requirement of the Council is about the same as ours. We assumed a requirement of about $40-45 billion in the late forties (Section I) and we adopted the working hypothesis that over the years the requirement would rise in the same proportion as output (implying roughly that if government expenditures did not rise in the same proportion, then taxes also would not, and hence private outlays would rise in a higher proportion). The Council's full-employment GNP for 1954 is roughly 15 percent greater in constant prices than was the GNP at the end of the forties. In these circumstances our 1954 requirement is about $45-50 billion. The Council's total investment requirement is $45 billion, including foreign investment. This is a very similar figure. The Council believes that there will be a deficiency as compared with this figure, unless a housing program is adopted, and it is a safe guess that this deficiency is much less than $10-15 billion (perhaps one-half of this figure or less). We

[27] In addition to the nonfarm plant and equipment outlays previously considered.

obtained a \$10-15 billion deficiency with the lowest $\Delta V/\Delta O$, before the possibility of internal adjustments was taken into account. We obtained no initial deficiency when a $\Delta V/\Delta O$ ratio of about 2.5 was assumed. It follows that the techniques of the Council are equivalent to a $\Delta V/\Delta O$ assumption of perhaps 2 *in our terms*. This is somewhat, but not very much, smaller than the full-employment requirement (again, in our terms).

It is true that the Council also states the need for income redistribution to obtain the full-employment level of output in 1954. But this is because, in the Council's model (unlike ours), consumption would otherwise not rise sufficiently to compensate for the failure of government expenditure to rise with output. The explanation is partly that the Council does not let tax revenues fall in relation to output in the same proportion as government expenditures are expected to fall: the Council's goals include a cash surplus of \$2-3 billion for 1954. The total consumption deficiency which, according to the Council, would develop in the absence of redistribution is probably in the general order of \$5 billion, because the Council wants to raise the share of employee compensation in national income from 62-63 percent to about 66 percent, that is to say, by about 3 to 4 percentage points. This implies shifting about \$8 to 10 billion of non-labor income to employees whose propensity to consume is greater. Therefore the total would-be deficiency of the Council (in the absence of offsetting policies) may not be very different from our would-be deficiency (in the absence of internal adjustments), if we use the lowest $\Delta V/\Delta O$ in our calculations. But only part of the Council's would-be deficiency is a deficiency of gross private capital formation, while all of ours is. Judging from the supporting argument, this part is unlikely to be more than (very roughly) \$5 billion. It is likely to be less than this figure. In other words, the Council's techniques of *investment* projection yield a figure that seems to be somewhat lower than the full-employment requirement, but not very much lower; and its results are those which our calculations would have yielded, if we had assumed a $\Delta V/\Delta O$ ratio of somewhat less than 2.

2. *Gross or net capital formation?*

This question possesses interesting implications concerning a variant of the method we used in our previous calculations. As was seen, one of the techniques employed by the Council is similar to

our $\Delta V/\Delta O$ technique, except that ΔV is replaced by the cumulated total of gross investment outlays. In other words, this technique of the Council is like a $\Delta V/\Delta O$ technique, with the additional assumption that depreciation is proportionate to the absolute increase in output (ΔO).

If depreciation were equal to ΔO multiplied by a constant ($D = K \cdot \Delta O$), then a linear relationship between V and O (i.e., constancy of $\Delta V/\Delta O$) would imply constancy of a slope defined as the *ratio* of the increment of cumulated *gross* investment outlays to ΔO. This latter slope is that underlying the Council's method. It differs from $\Delta V/\Delta O$ in that the numerator includes all current depreciation, that is, the Council's slope may be written as $(\Delta V + D)/\Delta O$. But if $D = K \cdot \Delta O$, then this slope is $(\Delta V/\Delta O) + K$, and hence this slope is constant (the underlying relationship is linear) if, and only if, $\Delta V/\Delta O$ is constant. The statement holds vice versa, too. When the questions are raised whether, at the end of the forties, there existed a capital shortage, and what the prospective level of capital formation is for given values of ΔO, the constant K does not influence the answers. Therefore, one would expect that the Council's method and ours would lead to identical results if $D = K \cdot \Delta O$. This is not necessarily a bad assumption for all periods with which we were concerned, because, in some periods, conceivably $D = K_1 V$, $V = K_2 O$, and $\Delta O = K_3 O$, where all K factors are constants (approximately). This would mean that $D = K \cdot \Delta O$. But for a comparison of capital-output relationships in the past two decades, this is not a very adequate assumption.

The implied assumption $D = K \cdot \Delta O$ slants the analysis in the deflationary direction. This is because, during the 1920's, depreciation was a very much higher proportion of gross capital formation than during the preceding decades. In some years of the thirties, there undoubtedly was undermaintenance of capital, with the result that the 1939 stock seems to have been no greater than that of 1929. The yearly *gross* capital formation figures on which the Council's computations are based did not reach the 1929 level (in constant prices) before 1946, and even the average yearly gross capital formation of the entire period 1930-49 is smaller than that of the twenties. Depreciation must, therefore, have accounted for a considerably higher proportion of gross investment outlays in the two recent decades than in the preceding ones. Hence, if from 1929 to 1949 a unit increase in output was as-

sociated with the same amount of gross investment outlay as during the twenties, then this implies that it was associated with a smaller net increment in the capital stock (ΔV). If the "gross method" of the Council points to no appreciable backlog at the end of the forties, then any "net" method would, provided the relationship of the twenties is considered normal (as in the Council's reasoning). For the same reason, if the gross method of the Council points to a future capital formation which is *not quite sufficient* for full employment, then net methods must be expected to show less insufficient or fully sufficient capital formation (or inflation).

Introducing depreciation in the Council's regression analysis (and assuming, as the Council did, that the economy is going to continue on the capital-output slope of the twenties) would have tended to raise the investment projection for 1954, and it would probably have pointed to a shortage as of 1949. The method of the Council actually led to an investment projection which was somewhat too low for full employment in 1954, and the same method led the Council to conclude that there was no appreciable capital shortage in 1949. The analysis of the present paper made it appear likely that there still was a shortage at the end of the decade, and that even without internal adjustments under the influence of a threatening deflationary gap, investment would have been almost sufficient, if we had continued on the $\Delta V/\Delta O$ slope of the 1920's (i.e., on a slope of between 2 and 2.5). Considering that we made substantial allowances for depreciation while the Council's vaguely comparable method made none, one would expect that the two results would deviate *in the direction* in which they actually do. However, *quantitative* reconciliation of the two results is not possible along these lines, because the Council's regression analysis—the second of the two Council techniques here surveyed—applies only to part of the total capital formation, and the results are extended to the total by certain *ceteris paribus* assumptions concerning the relationship between the constituents of the total. The constituent to which the regression analysis is applied—the sum of nonfarm plant and equipment outlays—is a very significant constituent and, hence, one of the two methods of the Council is similar (although not quite identical) to a gross variant of the method used in the present paper.

This comparison of results creates a presumption that the main

conclusions of our analysis, in Section G, are not decisively influenced by the depreciation allowances. These may conceivably be too large in the time series we have used. But if *some* allowances were made, the Council's regression analysis would point to a more sufficient (perhaps fully sufficient) private investment even if automatic internal adjustments were disregarded.

However, the main point to be emphasized is that, in the past, the economic system seems to have had quite a bit of flexibility expressing itself in mutual adjustments of the variables included in our basic equations (Section C-4). The estimate of a future "initial" deficiency of private investment as compared with full-employment requirements, which results from the Council's methods (without adjustments for depreciation), cannot be very different from the "initial" deficiency of about $10-15 billion per year obtained by our analysis if this is based on consistently "low" assumptions. In fact, it is likely to be smaller. We do not believe that an initial deficiency of such magnitude warrants the prediction of underemployment. As was argued in the preceding pages, adjustments in more than one variable may well tend to eliminate initial deficiencies of such size. If, instead of these low assumptions, we make assumptions lying in the higher regions of the plausible range, the conclusion is that, for some time to come, internal adjustments as well as monetary fiscal policy might have to cope with inflationary pressures. Neither our analysis nor the computations of the Council made it appear safe to gear long-run government policy to a deflationary gap.