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Balancing these considerations, it was estimated that absorption of a million workers in agriculture and in making good undermanning in several nonagricultural industries would result in a 2 percent decline in output per manhour, and that output per manhour would fall an additional 4 percent due to the reduction in munitions output and to disturbances associated with the transition to peacetime production and a peacetime labor force. The combined effect of these factors is to reduce civilian output per manhour 6 percent from the first half of 1945 to the third quarter of 1946.

Weekly hours of work in nonagricultural private employment were assumed to fall 10 percent from the first half of 1945 to the fourth quarter of 1946, or from 44.6 to 40 hours per week.

A quarterly index of output per worker, constructed on the basis of these judgments, was applied to the estimate of output per worker in the first half of 1945, obtained by dividing gross national product, minus wage payments to the armed forces and interest on the public debt, by the estimated civilian employment. The resulting estimate of output per worker for each quarter was divided into the adjusted gross national product for the same quarter to give estimated civilian employment.

The number of persons in the armed forces was estimated on the basis of demobilization schedules submitted by the military departments.

The estimated change in the labor force was based on judgment concerning the speed with which the wartime extras would leave the labor market. Unemployment is then a residual figure, though in estimating the speed with which wartime 'extra' workers will leave the labor force, the level of unemployment was taken into account.

### COMMENT

W. S. WOYTINSKY

#### I

Mr. Colm's definition of 'full employment' as a situation in which there are at least as many unfilled work vacancies as applicants for jobs is not satisfactory.<sup>1</sup> According to this definition,

<sup>1</sup> For Mr. Colm's definition, presented originally in discussion, see his reply below.

employment is 'full' if there are 10 million workers out of work and as many unfilled vacancies; it is not 'full' if there are 2.5 million unemployed and 250,000 vacancies. The concept of 'full employment' is, in this way, divorced from the usual connotation of 'good' employment. Fullness of employment is measured not by the size of unemployment or its relation to the labor force, but by the ratio of unemployment to job vacancies. However, in a country as large and heterogeneous as the United States there may be, simultaneously, many workers out of work and many vacancies; for example, mass unemployment among factory workers and shortage of labor in agriculture and forestry; or mass unemployment in the South and shortage of labor in the West. It is not clear why such a maladjustment between the demand for labor and the supply of labor force should be described as 'full employment'.

The postulate of a balance between applications for jobs and unfilled vacancies rests obviously on the assumption that the higher the ratio between applications and vacancies the easier it is for an unemployed worker to find work. From this point of view, the situation should look gloomy when there are 250,000 vacancies for 2.5 million applicants, and highly satisfactory if there are 10 million persons out of work and 10 million vacancies. However, a closer analysis of the turnover of employment and unemployment shows that the number of 'unfilled vacancies' — whatever this term may mean — has little to do with the opportunity an unemployed worker has of finding work. What counts is the number of accessions, i.e., actual hirings per unit of time, say per week. If there were  $U$  workers out of work at the beginning of the week and the number of separations and accessions during the week are designated by  $s$  and  $a$  respectively, the chance an unemployed worker has of finding a job

during that week may be measured roughly as  $h = \frac{a}{U + s}$ . The

number of unfilled vacancies does not appear in this formula.

'Full' employment should be used synonymously with 'good', 'high', or 'satisfactory' employment. All these terms express simply our appraisal of the situation. As a guide for appraisal, a maximum of tolerable frictional unemployment may be established and combined with a measure of the velocity of turnover

of unemployment or a frequency distribution of jobless workers by duration of idleness.

Moreover, for the purpose of studies of the Nation's Budget, another definition of 'full' employment may be preferable. We may start from the widely recognized fact that both a depression and a boom cause the labor force to expand. In the event of a deep depression and mass unemployment, members of families in which the main breadwinner is idle begin to look for jobs, and even if they find no work they join the labor force as unemployed additional workers. In the event of a boom — as during the war — additional workers are recruited among boys and girls who, under normal conditions, would have been in school; married women; persons who had retired from the active labor force because of superannuation; and the like. Additional workers are not the same in the two cases but the effect of their influx into the labor market is similar: the labor force increases with declining employment during a depression as well as with rising employment during a boom. In mathematical terms the active labor force is a U-shaped function of the demand for labor.

According to this theory, there must be a state of employment when the labor force is at a low point; that is, when additional workers are fewer than they would have been at a higher or lower level of employment. Characteristic of this employment level is the largest school attendance, the largest number of married women who can afford to give all their time to home-making, the largest number of retired workers. This level may be described as *normal full employment*, and is defined by the postulate: under full employment the active labor force is at a minimum.

From this point of view, the number of unfilled vacancies is irrelevant. On the other hand, it is not unlikely that the labor force will be at a low point when unemployment reaches a certain level, neither too high nor too low. It remains to future research to determine whether this level is 2 million, 2.5 million, or higher.

The question is of paramount significance in planning for full employment. The goal is not to *maximize* jobs but to determine their number in such a way as to *minimize* the active labor force.

## II

The merits and shortcomings of the Nation's Budget method exemplified by Mr. Hagen's paper should be appraised in the light of experience.

The aim is to estimate future unemployment; computations of single items of gross national product are merely steps preliminary to arriving at the unemployment figures shown at the bottom of Tables 2 and 3. To appraise the method it is enough to compare the figures in these tables for the fourth quarter of 1945 with the actual level of unemployment as revealed by the Bureau of the Census monthly surveys. Hagen predicts for October–December 1945 unemployment averaging 6.3 million under both favorable and unfavorable conditions. The Bureau of the Census recorded 1,520,000 persons out of work in October and there is no indication that the tide was rising in November or December. Consequently, the actual level of unemployment in the fourth quarter of 1945 was probably close to 1.5 million. For the first quarter of 1946 Hagen predicted unemployment of 8.1 million in both the 'more' and 'less' favorable projections. What the Census will show remains to be seen, but many observers think that labor market conditions in 1946 will be about the same as in 1945. Even with an allowance for understatement in the Census figures, Hagen's forecasts seem to be 200 to 300 percent off the beam. Indeed, they suggest mass unemployment at a time when there is an acute shortage of labor, depression when there is a boom, deflationary contraction when the entire economic system is dominated by inflationary forces. In brief, the prediction appears wrong on all counts and useless for any practical purpose.

Moreover, if this is the degree of accuracy of the method for the next quarter or two, its range of error for the more remote future must be still larger. The complete failure of the forecast at the first real test makes pointless further discussion of the applicability of the Nation's Budget method to estimates of future unemployment. Only the causes of this fiasco remain to be investigated.

I think that the errors in the projections have two sources. First, Mr. Hagen, like other partisans of the Nation's Budget method, began his computation with a definite conception of

our economic perspectives. He knew — or believed he knew — that the nation was heading toward a deep depression and mass unemployment; regression lines, as they always do, confirmed his apprehension.

The second source of error is in the method itself. Future unemployment is calculated as the difference between labor force and employment. A moderate margin of error — say of  $\pm 3$  percent — in the estimate of the labor force means that the future supply of labor might be estimated with a probable error of  $\pm 2$  million. The margin of error in the estimate of future employment is probably much wider. For gross national product the margin of error is hardly less than  $\pm 5$  percent, and an equal or larger margin should be allowed for the projection of productivity of labor and weekly hours of work. The cumulative margin of error in the estimate of employment is therefore not less than  $\pm 10$  percent, or  $\pm 6$  million jobs.

Since the errors in the estimate of the labor force and those in the estimate of employment are not necessarily compensatory, the total margin of error in the forecast of unemployment by the Nation's Budget method may be as large as  $\pm 8$  million. In short, a fairly narrow margin of error in the preliminary operations means that the final forecast may be 500 or 700 percent out of the way.

Hagen's errors are within this comfortable margin: in fact, his estimates were 250 percent, or 4.5 million, too high for the fourth quarter of 1945; they will probably be 400 percent, or 6.1 million, too high in the first quarter of 1946; and even the prediction of 9.3 million unemployed in spring 1947 may not be more than 8 million too high.

The method is so inadequate that the most fantastic mistake comes within its legitimate margin of error.

Much better projections of unemployment might be obtained by direct analysis of future economic conditions, emphasizing qualitative characteristics of prevailing trends. Such projections developed more than two years ago indicated that because of deferred demand for consumer goods, piled up purchasing power, postponed investments, and huge business reserves, there would be sellers' markets in the United States during the reconversion and for a considerable period after. This general appraisal of the economic scene implied that manufacturers

would have a good chance of selling, with a reasonable profit, as much merchandise as they could deliver. It was fairly certain that as long as merchandise was generally short production would tend to expand, and despite reductions in government expenditures, the situation would be inflationary. Long before the end of the war a direct analysis of economic conditions showed that the labor force set free by the cancellation of war contracts and demobilization of the armed forces would be readily absorbed and there would be no mass unemployment either during the reconversion or in the early phase of the post-war economy.

This conclusion is confirmed by historical experience: after each major war, the labor force has been the main bottleneck of reconversion and economic expansion. In the next year or two, temporary unemployment may be large in certain strategic areas and frictional unemployment appreciable in the rest of the nation, but expectations of mass unemployment like that during periods of economic stagnation are utterly unrealistic. Only runaway inflation, social turmoil, or the imminent threat of a new war could reverse the situation and push unemployment up. In that event, however, unemployment would be high for reasons that have nothing to do with the factors taken into account by the Nation's Budget method.

If experience is of any use in appraising projection methods, this is the record: economists who used direct economic analysis for their projections were able to foresee, several years ahead, the actual pattern of employment and unemployment in the transition period; those who used the Nation's Budget method, on the other hand, were responsible for a series of predictions that turned out to be wrong and misleading.

#### L. R. NIENSTAEDT

Mr. Woytinsky's remarks about the general approach of the papers presented by Messrs. Colm and Hagen seemed to me very much to the point. When going from detail to detail it is difficult to see one's direction, and as Mr. Woytinsky suggests, it is the direction that is important. In a forest one sees only the trees; to see the forest the first thing to do is to get out of it. As far as the economic system is concerned, that to me means

to attempt an orientation of the 'system' in relation to something that is 'not system'. This requires first of all a clear definition of what belongs to the system and what does not. Such a definition must evidently establish delimitations outside of which there is no economic activity. In *space* the delimitation is a geographical border. In *economic functions* the delimitation is the place where and the moment when some physical substance resting in nature begins to move into the system for some economic reason. If there is to be no economic activity outside the limits set, trading (movement of economic values) across geographical borders is excluded from consideration; in other words, the system is self-contained and can maintain itself only by production and trade inside its own geographical boundaries. Its relations with the outside consist in the utilization of raw materials from nature for productive purposes and the return of these same materials to nature when the useful objects they were part of are discarded. From such a point of view the economic process is a circular process resembling the metabolism of a living organism, and the system as defined is self-contained only in the sense that it is unrelated to any other economic system.

It is not unrelated, however, to something that is not system — natural resources, for instance. And a little consideration and observation will demonstrate that although the system is an economic system it must be under a definite influence of factors from the outside that are not 'economic' in the usual sense — length of the harvest period, length of day and night, etc. Further consideration may prove that relations between some factors inside the system may eventually be determined by these outside factors. These relations may be truly causal and very stable because the outside factors persist and are not influenced by any reactions from the inside factors. They are given facts to which the system must adapt itself even in an economic sense. This adaptation may determine trends that override all short term phenomena, i.e., determine the direction the system is moving in.

This approach, which allows a clear distinction between truly primary factors outside the system, or inherent in it, and induced factors, both spontaneous and conscious, inside the system, has been developed in my book, *Economic Equilibrium*,

*Employment and Natural Resources* (Principia Press, Bloomington, Ind., 1942).

As regards the full employment bill, I fear the term 'full employment' as now used is much too ambiguous. Sixty million jobs is the goal. But what is a job? It is so and so many hours of work for one year by an individual paid at such and such a price. Since, in a modern economy, everyone's work and income depends upon everyone else's work and income, full employment becomes a primary concern of 'general welfare'. On the other hand, respect for the freedom of the individual must admit the right to refuse the job offered (or 'guaranteed'). Clearly here is a dilemma of far-reaching consequences. As long as the right is exercised by only a few individuals at a time, there is no threat to the general welfare. However, if large segments of the population all at once exercise their right because they do not want to accept the jobs offered or 'guaranteed' all guarantees become absurd. At one extreme of the dilemma the right of refusal annihilates the possibility of a job for every citizen, while at the other extreme the right to a job for every citizen annihilates the right to strike and the fundamental freedom of the individual. There was no right to strike in Germany but everyone was supposed to have a job.

How can this contradiction be solved? What exactly is it possible to guarantee? Since it will be the economists who have to answer these questions, they are the first to be concerned. In view of the fundamental nature of the dilemma, it seems to me that it would clarify matters considerably if a concept of employment could be introduced that implies a freedom of choice between clearly defined consequences. At the same time such a concept must allow exact quantitative definition and be susceptible to mathematical manipulation in order exactly to determine what it is possible to guarantee under such and such conditions. I believe these requirements are met by what I would call 'the average chance of finding employment' meaning by 'employment' a definite number of manhours (a full time equivalent) any individual in a population of such and such a size could expect to be employed during a year if he accepted such and such an average wage rate, etc. For example, if a full time equivalent is 2,000 hours, an average chance of finding employment of 1 in 5 means that 20,000,000 in a population of

100,000,000 may work 2,000 hours annually on an average at such and such an income, or that 40,000,000 may work 1,000 hours at half that income.

Can such a term be defined? And can its dependence on other primary factors be established in a truly one-way causal fashion? I think so; at least as far as physical production is concerned. This was attempted in my book where it is shown, for instance, that the average chance of finding employment (3,510 manhours per annum) in physical production in the American economy declined from 1 in 5 to 1 in 8 between 1901 and 1929. This result is deduced independently from observation of interrelations between primary factors, then verified by statistical criteria. So far nothing has been done on the chances of employment in trade and services.

#### CLARK WARBURTON

The methodology currently used by econometric model-builders for estimating the future size of gross national product is essentially that of comparing the size of the parts of a whole with the size of the whole. The logic seems to be:

The whole is made up of its parts — in the case under discussion, three principal parts;

Certain relations have been found by experience between the size of the whole and the size of certain parts;

The size of the remaining parts has been found by experience to be related to the size of the whole and to the size of other parts;

Therefore, if we know the size of certain parts we can estimate the size of the whole;

Therefore, the size of the whole causally depends upon the observed relations between certain parts and the whole;

Ergo, by swelling the size of certain parts, corresponding changes are produced in the size of the whole.

This basic methodology has as much applicability to scientific problems in a wide range of fields as to the problem of forecasting the value of national output. One field in which it has been widely applied is that of anthropology, the specific problem being estimation of the size of a body from the length or dimensions of one or a few bones. However, this anthropological

application involves only the first four steps in the foregoing sequence. I wonder what the anthropologists would think of the fifth and sixth steps.

Let me take another analogy. If we measure a large number of plants we will probably find, for each species, typical relations between the lengths of the longest root, the stalk, and the head — the sum of the three being the total length of the plant. As long as these relations hold, if we can find some hormone — I believe such a hormone has in fact been found — to make the roots grow longer, we can expect to have taller plants. But when we turn to the real world and want to know why crops in Oklahoma were not as tall in 1934 and 1935 as in previous years, is this kind of analysis helpful? Rainfall statistics might conceivably be more significant. Or, if we want to guard against recurrence of such a situation, an irrigation project or a rain controlling device (if we have the 'know-how') might be of more practical use than a hormone factory.

The Budget Bureau and Mr. Hagen do not lean as heavily as other economic forecasters on the practice of stimulating growth by means of hormones. From current data they develop forecasts of the major constituents of gross national product by noting the short-run trends in the relative size of roots, stalks, and heads of the plants that have been grown in the fields during the last year or two or are still growing (a method essentially similar to that of the Department of Agriculture in forecasting crops in a given year). They give some attention also to the methods of cultivation and kinds of fertilizer that have been applied to the fields and appraise their probable effects. This methodology, nevertheless, is based upon essentially the same logic as that of the model-builders. The Budget Bureau and the Office of War Mobilization and Reconversion apparently assume that they are faced with the problem of deciding upon the strength of the hormone concentrate that should be applied in order that the crops this year or next will be up to standard. They ignore, however, the possibility that the probable rainfall during the next year and the water table resulting from the rainfall of the past few years may have sufficient relevance to the problem to be given specific consideration.

Also, I would like to point out that, even though we were not sure that we knew how to do so, in 1913 (with some improve-

ments in 1917) we built a rain-controlling device that has proved to be exceedingly effective — so effective indeed that in the early 1930's it gave us and the whole world the greatest economic drought recorded in history and during the last two years has given us greatly excessive moisture. More attention should be devoted to the current operations of this rain-controlling device, which have a great deal of relevance to the problems toward which the analyses of Mr. Hagen and the Bureau of the Budget are directed.

Finally, I would like to make two suggestions regarding research on the variability of consumer and business spending. First, if the mathematically-inclined makers of economic models would combine with the classical general-equilibrium theory of a static economy the equally traditional, respectable, and unrefuted theory of unneutral money, they would have a theory of a dynamic or moving equilibrium that might give them a good exercise in applied mathematics and would provide the theoretical basis for the construction of models with some resemblance to reality.

Second, if the economists who are interested in full employment under a system of private enterprise would look at the cases in the past where relatively full employment has been achieved for a few years and then lost, they might discover the conditions essential for the maintenance of full employment without government deficits. If, for example, they would look carefully at the data for the 1920's and early 1930's, they would find that during the 1920's, when we had a relatively high level of production and a moderately stable price level, the average rate of growth in the money supply was about 5 percent per year, but that this growth was stopped in the early part of 1928 and after two or three years of irregular slight decline was followed by a precipitate contraction. Such facts as these might lead to the conclusion that it was impossible for business and individuals to maintain their expenditures in line with increasing productive capacity without drastic changes in monetary habits, rather than to the conclusion — embraced by the Keynesians but repugnant to common sense — that the depression was ushered in by a change in monetary habits in the form of a sudden and great reluctance of business and individuals to spend the money in their possession. The facts regarding the

stability or erraticism of monetary habits, I suggest, have considerable relevance to the problem of estimating the adequacy of demand for potential postwar output and to the governmental policies that are necessary and desirable for full employment in an economy of free consumer choice and private enterprise.

MORRIS A. COPELAND

Mr. Colm concludes that our present economic understanding and statistical proficiency enable us to forecast business conditions with sufficient accuracy to implement the Murray Bill. While he does not make this clear, it would seem that his conclusion is meant to apply to forecasts of business conditions for a year and a half ahead. Thus Hagen's forecast extends forward for seven quarters from August 1945.

Two questions not really discussed by Mr. Colm or Mr. Hagen would seem pertinent to such a conclusion: What economic expedients to maintain full employment does the Murray Bill contemplate and what types of forecast does each call for? Among these various types of forecast, which are we able to do most effectively today?

It is suggested that the expedients contemplated by the Murray Bill call for the following types of forecast, among others:

(1) At how much, if any, below a full employment level is our economy likely to be operating during the next eighteen months unless specific new measures to raise the level are taken? (This type of forecast is presumably called for annually.)

(2) Is there any likelihood that with the types of private pecuniary motivation now in force and in the absence of a large public expenditure program we will be able to achieve and maintain approximately full employment during any, say, four successive years after about 1950? (A single-time official plus-or-minus forecast of this sort might implement the private-sphere provisions of the Murray Bill. However, any major actual or prospective change in the structure of private pecuniary motivation would be an appropriate occasion for a revised forecast.)

(3) Assuming that a 'shelf' of expenditure programs is to be accumulated, what are the dimensions of the prospective deficit

in the national production and expenditure budget it should be designed to meet?<sup>1</sup> That is, how sharp, how deep, and how prolonged a depression should we be prepared for? Such a forecast is needed in answering questions such as how large should the shelf be? How large a volume of expenditure programs could be put into operation in how short a time and how nearly ready-to-go should projects be before admitting them to the shelf? What should be the composition of the shelf and the type of arrangements for activating included projects? (While such a forecast would need revision from time to time, it is not clear that annual revision is called for.)

(4) Are business prospects for the next nine months such as to make checks on further expansion or business stimulants advisable? (This plus-or-minus type of forecast is presumably needed on a quarterly or more frequent basis and separately for each of several major segments of business.)

We may apply our second question first — which of these four types of forecast are we able to do most effectively today? Type (2) is the easiest forecast in its absolute form. Model analysis is clearly pertinent broadly to this type of forecast, but this plus-and-minus variety of forecast in its absolute form might equally be made on the simple basis that depressions are likely to recur unless we take steps to prevent them. However, when it comes to forecasting whether some proposed structural change will eliminate depressions, the difficulty of forecast is increased; if the proposed change is a major one, very greatly increased.

Type (3) is certainly a forecast we can do today. Although quantitative, it calls only for fixing limits on the various dimensions of the prospective deficit against which a shelf is to be accumulated: a safe minimum annual rate of shelf-project expenditure to provide for; a safe minimum period for which to provide this rate; and a safe minimum time in which to achieve

<sup>1</sup> By a 'shelf' is here meant a file of lawful projects, public and private, such that (a) the activation of each can, within limits known to a central authority, be materially delayed until the go-ahead signal is given or be materially advanced, and (b) each can be counted upon to get into actual production according to a schedule beginning after a certain interval subsequent to notice of activation. The shorter the interval the shorter the forecast of Type 4 needed for drawing upon the file. If the interval is to be known by the activating authority, an advance commitment of funds will probably be necessary.

this rate. The chief problem is how to compromise between limits generous enough to be statistically fairly safe and limits modest enough so that it will be politically expedient and administratively feasible to achieve the shelf.

Type (4) is not properly a simple plus-or-minus forecast, as the term 'steering-wheel policy' suggests; checks may be needed in some directions and stimulants in others at the same time so that several plus-or-minus forecasts are needed for each quarter. Nonetheless, it is much easier than Type (1); partly because it is a plus-or-minus forecast for each significant direction. But the fact that nine months instead of eighteen will suffice for most purposes is also important. Colm has drawn a distinction between 'primary' and 'induced' components in a *GNP* projection. It seems clear that the primary components would ordinarily constitute a larger proportion of the total *GNP* for a nine month than for an eighteen month projection. Some components that should be regarded as 'induced' for purposes of a longer-term forecast should be regarded as 'primary' for purposes of a shorter-term forecast. Hagen does not seem to have taken full advantage of this fact, perhaps because the problem of doing so was complicated by the special conditions of the transition period. It seems fair to say that in general we are far better able today to do a Type (4) than a Type (1) forecast. Usually an accurate and useful Type (4) forecast is easy. The real difficulty comes in 'calling the turns'. And if the turns cannot always be called nine months in advance, to call one three or four months in advance is far better than to wait until the turn has taken place.

Now as to our first question: for what economic expedients is each type of forecast useful? Type (2) is essential in connection with the adoption of any proposed structural change to promote full employment. It is essential also in connection with the adoption of any flexible policy, whether flexible tax rates, flexible credit policy, flexible expenditures, or what-have-you. Type (4) is essential in connection with the proper operation of any flexible policy, and Type (3) in connection with the proper operation of a flexible expenditure program. Type (1) is intriguing from the viewpoint of economic theory, and in conjunction with other features of the Murray Bill has certain political advantages. But its appropriateness to any specific

economic expedient is by no means clear. The most difficult and least needed type of forecast seems to have received an undue share of attention.

As a corollary of the above considerations it may be urged that it is advisable both to adapt our efforts at forecasting to the requirements of a full employment program and to adapt our full employment program to what is known regarding our present capacities for various types of forecasting.

#### MORDECAI EZEKIEL

Mr. Colm's statement concerning the lack of basis for economic forecasting was apparently directed at efforts to forecast the general business situation as a whole. As is well known, past efforts at general business forecasting — such as those made by the Harvard Economic Service for many years — have had 'batting averages' of about 50 percent; that is, the forecasts were little, if any, better than would have been obtained by tossing a coin.

Other types of economic forecasting, such as forecasting developments, commodity by commodity, in the agricultural field, have given better hope for success. They are based upon the large number of agricultural economic studies of factors determining price, supply, and demand for individual commodities, that have been made since 1915. For more than twenty years the Department of Agriculture has been publishing an annual 'Agricultural Outlook Report' in which forecasts are given of the prospects for individual agricultural commodities a year or two ahead — forecasts covering prospective shifts in production, consumption, demand, and prices. As far as they indicate the general future direction, up or down, their 'batting average' has been 80–90 percent right.<sup>1</sup>

The success of this commodity-by-commodity forecasting service has made it valuable as an economic service to farmers on which to base their future production programs. In turn, this realistic and proved knowledge of how the economic factors work in various agricultural industries made it possible to carry through the various operations under the Agricultural Adjust-

<sup>1</sup> Oris V. Wells, 'A Comparison of Outlook Statements with Subsequent Events' (U. S. Department of Agriculture, Bureau of Agricultural Economics, mimeographed, Jan. 24, 1930).

ment Act and subsequent agricultural control legislation in the light of what the controlling economic elements really were.

Although, as indicated, it is more difficult to forecast the general business situation as a whole, many factors of the Nation's Budget could be forecast by a similar industry-by-industry analysis. Output of housing and construction, automobiles, and many other capital goods are determined in part by inherent cycles or other characteristics of the industries themselves. Their production and sales, therefore, are not entirely dependent upon the levels of national income as a whole. To the extent that these individual items in the national budget can be forecast from their own internal industry situations, the accuracy of the national forecast can be increased, and the projections of the Nation's Budget given somewhat the same degree of reliability that the projections of agricultural situations have had in the past.

### REPLY

MR. COLM

*The Concept of Full Employment.* The concept of 'full employment' has been challenged. In response to a question from the floor I defined 'full employment opportunities' as a condition in which the number of vacancies equals the number of job seekers. Mr. Woytinsky objects to that definition. He says we may have millions of job seekers and millions of vacancies, but that would still not be 'full' or 'good' employment if job vacancies and job seekers do not fit each other because they are in different regions or of different types.

Woytinsky proposes to measure the status of employment by measuring the chances of an unemployed worker to find a job,

and presents a formula:  $h = \frac{a}{U + s}$ ;  $a$  and  $s$  designate the number of accessions and separations in a unit of time;  $U$ , the number of unemployed workers at the beginning of the unit of time. Let us test his formula by some hypothetical figures.

Assume that, in case 1, of a 60 million labor force 10 million are unemployed and 5 million are fired and rehired during the period. The chance of an unemployed worker finding a job is

then expressed by Woytinsky's formula as:  $h_1 = \frac{5}{10 + 5} = 1:3$ .

In case 2 the number of unemployed is the same, but we assume a complete turnover within the employed labor force, namely, 50 million accessions and 50 million separations. The formula

then gives:  $h_2 = \frac{50}{10 + 50} = 1:1.2$ . In case 3 the number of un-

employed is only 1 million and the number of accessions and separations is assumed to be the same as in case 1, that is,

5 million each. In this case then:  $h_3 = \frac{5}{1 + 5} = 1:1.2$ .

This measurement shows an equal approximation to 'full' or 'good' employment in cases 2 and 3 although the unemployed number 10 million in the one and 1 million in the other. It seems to me that Woytinsky's formula measures the unemployed worker's chance of finding a job but neglects to measure the employed worker's chance of being fired. In this respect I think his approach fails to measure 'full' as well as 'good' employment.

Nienstaedt proposes to measure 'the average chance of finding employment', expressing employment by a certain number of manhours as an equivalent for an individual employed full time. I think in all our estimates we have actually been using manyears in measuring the number of employed and unemployed rather than the number of individuals in or out of a job. Consequently, no one will quarrel with Nienstaedt's proposal in this respect.

Nienstaedt's formula, if I understand it correctly, merely expresses employment, measured in terms of manyears, as a ratio to the population as a whole. I can see its value for depicting a trend, as he does in his book, but I fail to see that his formula can help in solving the problems he raises with respect to the purposes of a full employment policy.

Nienstaedt, however, tends to oversimplify our problem when he assumes that primary physical factors provide a simple causal relation from which we can determine the quantities of employment and unemployment. The problem he struggles with is fundamental to any consideration of the means whereby full employment can be attained. Physical environmental factors are of great importance to our society and should be considered in interpreting the significance of statistics, particularly

of dollar figures. However, if physical factors were the primary and controlling determinants of the level of economic activity, there would be no need for a full employment bill.

Nienstaedt is also worried about another problem. He believes that any government assurance of full employment is incompatible with 'the right to strike and the fundamental freedom of the individual'.

I believe that Woytinsky's as well as Nienstaedt's doubts can be met by distinguishing, as I suggested, between full employment *opportunities* and actual full employment. Full employment opportunity means that there is a job for each person in the labor force. Some persons in the labor force may still not actually get a job because of regional discrepancies, discrepancies in skill and training, differences in wage scales offered and wages demanded, or for other reasons. 'Good' employment, which Woytinsky wishes to measure, should comprehend all factors that affect job opportunities as well as actual employment. For purposes of analysis, and even more important, for purposes of government policies, it is, however, of utmost importance to distinguish between employment opportunities and actual employment. There are opportunities even though some workers must migrate or retrain or settle their conflict with management before the opportunity can materialize.

If I correctly understand the purposes of the 'full employment' bill, it is intended to assure the creation of sufficient job opportunities but is not designed to guarantee actual employment for everybody. Limiting the assurance of the government that full employment opportunities will exist does not mean, however, that the government does not also have the responsibility of helping to overcome the discrepancies between jobs offered and sought and labor-management conflicts. It is, I believe, desirable to develop methods to measure employment opportunities as well as actual employment. There is probably not any one single set of figures that can adequately measure both employment opportunities and actual employment.

*Various Types of Projection.* Copeland emphasizes quite correctly that various types of policy formulation require various types of projection, covering short, intermediate, and long periods. In addition — and this is very significant — he suggests that possible patterns of economic development be

sketched as a basis for preparing programs for actual use when needed.

Copeland's remarks can help to clarify some aspects of the discussion that was initiated by Hagen's presentation of a set of estimates. These estimates, which were prepared immediately after VJ Day, were, I believe, valuable for illustrating methodological problems and for demonstrating possible patterns of economic development in the reconversion and post-reconversion period. The projections showed that in the period of restocking and reequipping, inflationary pressures may prevail although current incomes are expected to decline. These projections suggest the need for preparing policies to deal with this particular type of inflation as well as prepare plans for meeting deflationary tendencies after the end of the restocking and reequipping boom.

The fact that the number of unemployed estimated for the reconversion period proved to be exaggerated should not be minimized and should teach a valuable lesson. I do not believe, however, that the errors in the short run prove that the patterns of economic development indicated for the intermediate and long run periods are necessarily wrong. Woytinsky questions the method presented by Hagen, the patterns of economic development, and the policy conclusions suggested by these projections. He may or may not be right. He believes that "to appraise the method it is enough to compare the figures in these tables for the fourth quarter of 1945 with the actual level of unemployment as revealed by the Bureau of the Census monthly surveys". This, I believe, is not a valid conclusion. The fact that the unemployment estimates of these projections were exaggerated for the first part of the transition period proves very little for the real controversy concerning the method of projections and the probable patterns of development. This experience does prove, however, the validity of Copeland's point that the significance and limitation of each projection with respect to permissible policy conclusions should be emphasized.

MR. HAGEN

If two football teams meet during the first postwar season, with many changes in their line-ups caused by 'reconversion' of the

college population, and if two individuals bet upon the outcome of the game, one will be more nearly correct than the other, barring a tie. It would be unwise, however, to conclude that this individual possesses a superior procedure for judging the results of all future games between the two teams.

Similarly, Mr. Woytinsky's conclusions concerning the Nation's Budget method of forecasting economic conditions constitute a drastic jump from limited premises. The evidence consists of a non-random sample of one forecast. To draw conclusions concerning a population consisting of all future forecasts — as Mr. Woytinsky does — is unwarranted.

One specific point in Mr. Woytinsky's analysis should be noted before his general criticism is commented on. I do not understand his statement that regression lines necessarily — Mr. Woytinsky says "always" — indicate that the nation is heading toward deep depression and mass unemployment. A given set of regression lines may indicate this. Another may not. Almost any systematic relationship can appropriately be expressed by a regression equation. To deny the validity or relevance of regression equations is substantially to deny that systematic relationships exist between the variables. I think that Mr. Woytinsky would not deny that relationships exist, for instance, between consumer disposable income and consumer expenditures. He has repeatedly used regression lines to relate these variables.<sup>1</sup> Since he therefore obviously does believe in the use of regression equations for purposes relevant to the present discussion, his statement here seems meaningless. It is worth while noting that as it stands it is obviously in error.

Mr. Woytinsky begins his criticism of the logic of the Nation's Budget method by stating that by it "future unemployment is calculated as the difference between labor force and employment" and therefore is subject to wide errors. The statement is correct — but I do not know of any other method of forecasting unemployment.

The margin of error in any forecast of unemployment is due not to the use of a particular method but to the nature of unemployment as a small final residual in a chain of causation.

<sup>1</sup> See, e.g., W. S. Woytinsky, 'Relationship between Consumer Expenditures, Savings, and Disposable Income', *Review of Economic Statistics*, XXVIII, 1, Feb. 1946.

Unemployment arises because job opportunities are less numerous than job seekers. The number of job opportunities depends upon the level of output. Small variations in forecasting the level of output, plus small errors in forecasting the level of employment that will be associated with any given level of output may result in percentage errors in forecasting employment that will be far greater percentage errors in forecasting unemployment. A forecast of unemployment by no matter what method is subject to large percentage error for this reason. Since Mr. Woytinsky himself bases his forecast of unemployment on one of output and therefore implicitly of employment (see the last page of his criticism above), there would seem to be no valid basis for his criticism of this aspect of the Nation's Budget method.

The Nation's Budget method of forecasting is simply a systematic procedure for adjusting estimates of consumer expenditures, government purchases of goods and services, and components of private capital formation, so that they will be mutually consistent. Obviously, the level of government expenditures and revenues and the level of private capital formation are partial determinants of the level of total output and income. Equally obviously, the level of consumer expenditures depends upon the level of total income, though not upon that alone. Again, the levels of various components of private capital formation depend in part upon consumer expenditures. The Nation's Budget method is merely a simple method of making clear the relation between component estimates, so that an explicit basis exists for judging their mutual consistency. Following a procedure that makes this possible is in my judgment an important improvement in forecasting techniques.

However, it remains true that the method determines the levels of output, income, employment, and unemployment which are forecast, only so far as it reveals inconsistencies. The estimates depend in the first instance upon judgments and data that are independent of the method used.

Mr. Woytinsky would reject this systematic procedure for checking inconsistencies, and would substitute a "direct analysis of future economic conditions, emphasizing qualitative characteristics of prevailing trends". I do not understand what he means. He clearly does not propose estimating unemploy-

ment or employment directly, without regard for the level of output. I hardly think that he means that each component of output should be estimated independently of every other component. It therefore seems probable that his statement constitutes merely an expression of the opinion that his *judgment* at VJ Day concerning coming conditions was better than that of persons who were using the Nation's Budget method. Concerning this I have no comment to offer. I would merely note that it is not relevant to the question whether the Nation's Budget *method* is a useful one, and therefore that Mr. Woytinsky's criticism in the main is irrelevant.