

Comments on Lawrence R. Klein's
Economic Fluctuations in the United States 1921-1941 (Second Draft, June 1947)

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The study attempts to describe and explain economic fluctuations in the U. S. with sufficient precision to make satisfactory predictions possible. Its instrument consists of a system of sixteen equations. That an economy as large and as complex as that of the United States could be so explained would be most remarkable; if the predictions turned out wrong, it should not be surprising. Yet it is an accepted principle of scientific method that an explanation should contain as few variables as possible. We should be justified in constructing a more complex system only after it has been shown that the simpler one was unsatisfactory. This, I believe, should be the fundamental purpose of Klein's study: not to predict, but to see whether predictions based on a relatively simple system of this type can be satisfactorily made. The acceptance of this aim would not require many formal changes. But it would change somewhat the spirit of the study and make its tone more modest. It would also require that its theoretical ground work be made clearer and more explicit.

On the more fundamental question - whether our economy can be satisfactorily described by a system of equations of this type, be their number sixteen, or thirty-two, or three hundred, I don't have much to say. On the whole, I feel sceptical; but scepticism is a rather cheap feeling, and not very useful in the absence of positive suggestions.

These comments, written at the request of Professor Marschak, do not aim at examining all aspects of the study. They are limited to fields in which I felt myself less ignorant than in others.

I. SUPPLY OF LABOR AND CAPITAL

It is surprising that the system has no supply functions of labor and capital, and therefore no concept (in some form or other) of productive capacity. Evidently, any quantity of goods and services demanded can be produced. Under these conditions, it is not clear what determines the general price level, and how the latter can be treated as an endogenous variable. It is interesting and significant that the production process is actually absent in the system. Goods are produced almost by magic. Is this an effect of "The Keynesian Revolution"? Even if this were somewhat the case in the period studied, that is, 1921-41, or at least in 1930-40, a system derived under such conditions can hardly be applied to the present period.

The market adjustment equation (II-44,47) can hardly play the role assigned to it. The random disturbance in the inventory equation cannot be interpreted as measuring undesirable inventory accumulations. This would be the case only if it were claimed that the inventory equation (minus the disturbance) expressed the demand for inventories precisely, and I don't think Klein would claim that any behavior equation could perform such a function. It appears to me that the general price level is determined by the interaction of productive capacity (supply of productive factors) and demand for these factors, and that this fact deserves an explicit recognition.

II. FORMATION OF EXPECTATIONS

It appeared during our staff meeting (and during my discussions with Mr. Cooper) that the reason why expectations take the peculiar form adopted in the study is due to the fact that the economic period for which the decisions are made, happens not to coincide, and, as a matter of fact, is assumed to be much shorter than the period - a year - for which data are available. As a matter of fact, there is a third period here, namely the number of the past planning periods which are taken into consideration when expectations are made. The presence of these three periods and their relative durations deserves, I believe, an explicit explanation.

II-7. What is the justification for assuming future wages to be equal to present ones, while prices are based on a different assumption? It is regrettable that the system contains no wage rates as such. Surely the recent wave of wage increases has had a strong effect on prices. With wage rates being determined by the actions of several large unions and corporations, perhaps these rates should be treated (at least for the time being) as exogenous, but they should certainly have a place in the system.

III-57. 3.3.1a. This equation is really not a part of the system. What is its purpose?

III. THE INVESTMENT FUNCTION

The final result, such as (3.3.2a), is quite reasonable (and, by the way, does it or does it not contain t ?), though not necessarily unique. One wonders, for instance, if there shouldn't be some lag between investment decisions and output of investment goods, and there are other questions which could be discussed, and for which arguments could be found on both sides. But the theory from which the function is derived is most remarkable. If I may be forgiven for saying this, it gives a strong impression that the type of the function was decided upon first, and then considerable effort was made to show that such a function could be actually derived from the profit maximization and production functions.

It is an important economic fact that capital equipment is durable, that it is usually purchased or rented for a considerable length of time, and that its depreciation, both in reality and in accounting practice, is not directly and closely connected with the intensity of its use. The usual accounting practice is to apportion depreciation expense over some time period (uniformly or otherwise), and while its allocation among the various departments of the firm may be connected with output, the actual total expense is usually not. So that if a firm makes plans over a period evidently (as assumed by Klein) considerably shorter than a year, depreciation expense appears as a fixed cost, and therefore hardly finds a place in the profit maximization function. And on IV-4 we find a statement that "Depreciation policies of business firms may be influenced by tax considerations and other matters which have nothing to do with the consumption of fixed capital." I have no doubt that the author is perfectly aware of all the points about depreciation I have made here, and if so, it remains a puzzle why he found it necessary to violate every one of them in his derivation of the investment function. We should also remember that both the stock of capital and its depreciation are extremely vague concepts, and it is rather hazardous to make investment a function of them.

Here are a few more specific comments.

Why isn't the investment function expressed in gross terms in which investment decisions are actually made?

II-3,6. If all capital has the same price, what determines the firm's choice among the various kinds of capital? If new capital is more efficient than old, why doesn't a firm use new capital only, there being no limit (in Klein's system) to its quantity? And if old capital is less efficient than new, it is remarkable that their prices should be identical. Also, should the two kinds appear as one factor in the production function?

II-10 - (2.1.2a*). If \underline{d} indicates the depreciation of the whole capital stock, how can it be identically equal to the depreciation of new investment, the latter being some three or five per cent of the former? And why is net investment a function of its own depreciation? If (2.1.2a*) is solved for \underline{i} in terms of \underline{d} and \underline{K} we get something like

$$\underline{i} = \underline{a}\underline{d} + \underline{b}\underline{K}_{-1} + \underline{c}\underline{n}$$

But \underline{d} is really proportional to \underline{K} . So investment becomes simply a function of the stock of capital.

It is entirely possible that all these criticisms are due to my failure to understand Klein's procedures. If so, I certainly urge a clarification. But if I am at least partly right, then I believe it would be much better to dispense with this derivation of the investment function (a derivation which seems to have only a ritualistic purpose) and treat the investment function as a separate assumption,

which, with the present state of our knowledge, is not derivable from the profit maximization and production functions. Deprived of its mystical powers, the investment function could then be examined, changed, improved, etc. on its own merits, which would be a more honest and scientific procedure than the one used by the author.

IV. THE MONEY EQUATIONS.

The interest rate being completely determined by (3.3.32), the purpose of equations (3.3.19a) and (3.3.31a) remains unexplained. Their derivation was based on highly heroic assumptions regarding the division of the stock of money into idle (time deposits) and active (demand deposits) balances, and since they don't seem to serve any purpose, wouldn't it be better to omit them altogether? The rest of the system will remain unaffected.

Since the interest rate plays such a limited role, affecting only the demand for rental housing (and why not that for owner occupied housing?), and since it is determined to such an extent by the actions of the Federal Reserve Board, why not take it as exogenous and let it go at that? This may not quite apply to the mortgage rate, but Klein does not use the mortgage rate.

V. THE CONSUMPTION FUNCTION

The general objections raised against the derivation of the investment function apply here as well. Again one gets a strong impression that the shape of the consumption function was decided on in advance, and then a rather heroic method was used to show that it does in fact spring from the maximization of utility by the household. This theory, as presented by Klein, is both old-fashioned and unrealistic. It is very doubtful that a household saves in order to buy certain future goods, the prices of which it discounts. It is indeed surprising that Klein, who seems to be a follower of both Marx and Keynes, should accept such a theory. One is ready for the term "abstinences" to complete the picture. And it is also remarkable that the interest rate which appears in this theory presently disappears, though some of the parameters of the consumption function should be not parameters, but functions of the interest rate.

It is also about time to revise the notion that the utility of a household is a function of its income only, and that it is not affected by incomes and expenditures of other households, and by its position in the income distribution. I believe that Duesenberry at Harvard is doing some interesting work along these lines.

I-4. Consumer spending is not entirely limited by the budget. What about credit and particularly installment buying?

II-38. Savings need not be held in the form of liquid assets.

VI. THE HOUSING EQUATIONS

III-32. Aren't the statements that "Every family must have a separate dwelling unit", and that "over long periods of time there will tend to be just as many dwelling units as families", a bit too optimistic, at least for the time being?

III-41. Is income of the last three years a good index of liquid wealth accumulated during that period?

III-44. The speculator builder is more apt to borrow funds for his ventures, rather than worry about their investment in the security market.

III-57. We have here a demand for new houses. What about the supply of them by the construction industry?

III-44. (3.3.14.) Is it legitimate in a study of this type to introduce lags without any explanation, merely as a result of "empirical study"?

III-45. (3.3.15.) The prices of other goods "have never been found to be statistically significant." Any references?

IV-11. When these estimates are made, as well as in the rest of the study, it would be interesting to find out how significant these assumptions are. What about trying a lower and an upper limit?

VII. INVENTORIES

For a discussion of the market adjustment equation, see section I.

II-5. Why is the inventory storage function a quadratic in h ? Shortage of storage space?

III-37. (3.3.3a) It isn't clear why t appears in the inventory equation.

VIII. PROFIT MAXIMIZATION

The use of the profit maximizing function is one of the weak links of this and of many other economic studies. Depending upon the period involved, the maximization of profits may lead to a diametrically opposite decision. The usual assumption of the "long run" is not necessarily meaningful because it can explain anything. Leontieff's use of constant production coefficients instead of profit maximization is certainly an interesting alternative.

Klein essentially has only one firm producing one commodity. Actually, one should investigate how profits are affected by intercompany purchases, differential price movements, valuation of various inventories, etc.

II-3. Should not taxes, particularly excises, appear in the profit maximization equation? (See also III-37). Excise taxes (and even income taxes as well) may affect output, [if uncertainty is taken into consideration]

II-29. Klein's rationalization of the firm's behavior under the rule of thumb is questionable. Presumably anything that the firm does maximizes profits. What is meant in such criticisms is that some other rule rather than a constant markup would maximize profits more effectively. Also, Klein's explanation does not show what determines the size of the markup.

OTHER COMMENTS

I-3. It should be clearly stated that it is tax yields and not tax rates that are made exogenous.

II-22. "Much of the discussion (about effects of taxes on investment) is either confused or wrong." Isn't it only fair to give references?

III-38. Klein's term "business taxes" being used before July 1947, includes corporate income taxes. A note to that effect should be made.

III-56. Government revenues, for the most part, are not fixed in money terms, and a revenue act fixes rates, not yields. As income rises, tax yields increase as well. Klein's assumption regarding the constancy of tax yields should result in an incorrect multiplier. Transfer payments are not entirely exogenous either, though their rates are.

IV-9. For business we should use tax liabilities; for households - taxes actually paid.

I-2. Does Klein imply that as a result of his work economic fluctuations will now be prevented? Greater modesty is certainly called for here.

I-2. It might be better not to call exogenous variables "non-economic." In his model such "non-economic" variables are, among others, corporate savings, prices of capital goods, tax yields, etc.

II-4. Working capital is not the same as inventories.

II-13. "Most demand studies have found that constant-elasticity demand schedules are not out of line with the data." References?

II-14. This is somewhat outside the point, but K does not necessarily indicate the size of the firm; not all its capital is fixed. On the other hand, not all fixed capital belongs to the firm: part may be financed by borrowing. If d indicates how much capital has been used up, it cannot show "how much borrowing will have to be undertaken." Depreciation reserves are set aside, and therefore

(usually) need not be borrowed. Therefore the ratio d/K is simply the depreciation rate and has nothing to do with borrowing.

III-34. (3.3.4.) If stock of producer goods is a function of sales of the latter, we should have here gross and not net investment.

III-35, and others. The reason why "there are no satisfactory data on inventories classified according to producer and consumer goods industries", is simply because such a division of industries cannot be made. Is an inventory of coal or steel a producer or a consumer good? It might be later used for a passenger car, or a truck, for home or for factory heating. The division of the economy into producer and consumer industries is a fallacious anachronism.

III-52. In a closed system the holdings of all securities will not add up to zero because of valuation differences. See the Conference on Wealth and Income (papers by Hart, Goldsmith, and others), January 1948.

III-58. It isn't clear why prices of capital goods are treated as exogenous. Not because an attempt to make them endogenous would raise questions regarding the capacity of certain industries? And how does the author know that "the resulting error is not large"?

III-70. If liquid funds don't affect investment in railroads, this is not a proof that liquidity may not be important elsewhere. For that matter, if I remember correctly, Kuznets made a study of the acceleration principle for the railroad industry and found that it did not work. Would Klein therefore change his investment function?

III-65. "Since these estimates (i.e. 3.3.46a) do not agree with the economic theory, we are led to reject the hypothesis..." Why not investigate the theory instead?

X. SPECIFIC COMMENTS ON MODEL I

This model also lacks the supply of capacity equations and is therefore subject to the same criticisms as Model III.

III-5. Why is the spending of a capitalist subject to a budget constraint? Don't capitalists borrow?

III-6. The insertion of a t into the payroll equation to take into account "the strength of the organized labor movement..." is not quite in accordance with Marxian theory, is it?

III-6. There is evidently a mistake in equation 3.1.13 because \underline{Y} is not net national product.

III-18. What happened to transfer payments? Are they included in the so-called profits?

III-27. On what basis is it decided that the multiplier in (3.1.50) is reasonable? And can it be correct when tax yields instead of tax rates are assumed to be given?

III-34. What is "a stock of producer goods"?

III-18. In actual fitting, what happened to other types of income besides wages and profits, as well as to transfer payments?

IV-14. The author includes all entrepreneurial income (except for farmers' incomes) in profits. If I remember correctly, Kuznets suggested almost the opposite. Wouldn't a compromise be better?