Discussion of the Evans Paper

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While the political discussion in the United States has suddenly focused on the so-called "supply-side effects," this is not a new discovery in the literature of economics. I don't believe any one has denied the theoretical possibility that labor supply may depend on the real wage rate, and that personal savings may depend on the real after-tax rate of interest. The question has always been about the empirical order of magnitudes of these responses. In the case of savings, there are two further questions: whether or not an increase in savings will necessarily lead to correspondingly larger investment in capital goods, and how much the additional investment will contribute to potential and actual output.

Evans appears to claim in his summary (Evans, 1981) that he has resolved all these empirical questions, and his new model is now capable of predicting major effects of macro and micro policies aimed at supplies of productive factors. A detailed appraisal of his claims is difficult because they are embedded into a large model, and the model in question is not laid out for easy understanding.

I therefore propose to look at one critical group of equations in Evans' model as a representative of the model. Since Evans himself says that the equation explaining productivity plays the central role in his model, let us look at this equation as the starter. It is given in his summary paper (Evans, 1981) and (Evans, 1980, pp. 7.88–7.89).

First of all, we have to presume that Evans, when he defines PRD as private nonfarm business productivity, means by this variable output per manhour in this sector. The dependent variable in this equation is the rate of change of PRD. We may dispute the choice of variables that Evans introduces to the right-hand side of this equation. In order to concentrate our attention on less controversial issues, however, let us accept his choice as appropriate. There remains the question of the form of this equation.

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The most curious thing about this equation is the lack of correspondence of dimensions among variables, and consequent implausible steady state characteristics associated with it. As I indicated before, the dependent variable of this equation is the *rate of change of* productivity per manhour. Yet some of the independent variables, notably the ratio of the number of secondary workers to total employment, and direct federal government expenditures on regulations *in current dollars*, are level variables.

To understand clearly the nature of absurd results that follow from this setup, let us consider the situation in which all independent variables, including the two variables mentioned above, remain constant for a while, generating a constant rate of growth of productivity. Now suppose that the proportion of secondary workers in total employment increases by some fixed amount, say 1%, and remains at the new level thereafter. Then, the *rate of growth* of productivity (not the level of productivity) declines by a fixed amount. (If I believe in the definitions and numerical values reported in Evans' paper, it does so by .84% per year. However, this is too large an effect for me to accept for the first year, the only period in which this equation makes any sense, and I suspect that there may be some misprint and/or errors of units in the definitions.) Consequently, a once-and-for-all increase in the proportion of secondary workers to total employment will, according to the Evans equation, lead to a continual decline in the level of productivity relative to the reference path. Taking Evans' equation literally, if the ratio of the secondary workers to total employment rises 1%, say from 40% to 41%, and remains at the new level thereafter, the level of productivity will fall by .84% the first year, 8.7% during the first 10 years, and 18.3% during the first 20 years, and will continue to decline forever.

The level of federal government expenditure on regulation is even more absurd. The variable entered is total expenditure *in current dollars*. Thus, if the total expenditure in current dollars rises slowly for whatever reason, perhaps because of inflation, perhaps because the scale of the economy increases, the rate of increase of productivity must fall even if the federal government expenditure on regulation is becoming smaller and smaller relative to total GNP in current dollars. (As an illustration, suppose that GNP in current dollars is growing at 7% per year, while the government expenditure on regulation in current dollars grows at 4% per year and the inflation rate is 5% per year. The rate of growth of productivity will still continue to decline, according to Evans'

equation.) Since no other variable on the right hand side of this equation is an extensive variable that grows with the growth of the economy as a whole, the presence of this variable, the level of direct federal government expenditures on regulation in current dollars, must eventually make the rate of increase of the productivity negative, even though this expenditure as a proportion of GNP in current dollars becomes smaller and smaller.

Even some of the more reasonable-looking variables have their troubles. The ratio of business fixed investment to gross national product sounds like a reasonable candidate for influencing the rate of growth of productivity. But anyone who has worked with models of growth will soon realize that this is not really a sensible variable. The variable of this sort that can be fairly readily accommodated in this context is the rate of growth of capital stock per employee net of depreciation, not the gross investment—gross output ratio.

His statement that the relevant ratio is investment in constant dollars to the gross output in constant dollars, and not the ratio of current dollar values is also a serious suspect. The only theory bearing on this point in a multi-goods model that I am aware of is my own (Ando, 1964); the conclusion in that theory was that the only aggregate ratio that could be interpreted meaningfully was the ratio of the value of capital goods to the value of output, not the ratio between implicitly deflated figures in national income accounts. But that proposition was in the context of a specific, well-defined model, and here we are dealing with an assertion by Evans, which is apparently not based on any coherent view of the world.

On a basis of these observations, I conclude that Evans' equation explaining the rate of growth of productivity, the equation which, in Evans' own words, reflects the main thrust of his model (Evans, 1981), is not worthy of our further attention.

Even though Evans imputes great importance to the equation for the productivity discussed above, the output of this equation feeds into only two places in Evans' model, and it is probably worth extending our review of Evans' model to cover these two additional groups of equations.

The first group of equations in which output of the productivity equation plays a role is the equation expressing total manhours as a function, among other things, of total output and productivity. One typical such equation in Evans' model is given as the third equation in Braun's discussion (Braun, 1981), also (Evans, 1980, p. 7.69). Since productivity, PRD, is defined as output per manhour, if all

definitions are assured of consistency everywhere, then the manhours equation must be an identity, namely

$$\text{EHMFG40} = \text{XIPMS} \cdot \frac{1}{\text{PRD}}$$

where

EHMFG40: manhours in manufacturing
XIPMS: index of industrial production, manufacturing
PRD: productivity per manhour, private, nonfarm, business sector

The identity does not hold because EHMFG40 and XIPMS refer to manhours and output in manufacturing industries while PRD refers to productivity per manhour in private nonfarm business sector, XIPMS is an index of production rather than total volume of production, and for a host of other definitional discrepancies. But I do not see anywhere in Evans' writing or in his handling of these equations any indication that there are any important conceptual reasons why the above identity should not hold. Yet, the manhours equation Evans actually estimates and reports is basically of the form

$$\text{EHMFG40} = \text{constant} + \alpha_1 \text{XIPMS} + \alpha_2 \frac{\Delta \text{PRD}}{\text{PRD}} + \ldots\ldots$$

where dots represent additional terms in the equation which are not related to output or productivity. In other words, Evans has substituted for the level of productivity, PRD, in the identity the rate of change of productivity, linearized the equation, and then introduced a host of other variables. I see absolutely no justification for this substitution or for linearization. That it has disastrous consequences should not come as a surprise to us.

For instance, given a level of output and a rate of growth of productivity (not the level of productivity), other things equal, the manhours needed to produce this output remains the same. To put it another way, if output remained the same from year 0 to year 10, while productivity (output per manhour) increased at the constant rate of 3% per year, then the manhours required to produce this same output in year zero and in year ten are nevertheless the same. If this statement sounds contradictory, it nevertheless accurately reflects the statement embodied in the equation.

Clearly, such a characteristic of the equation cannot be reconciled with data, and something else must enter this equation to help

reduce the manhour requirement per unit of output over time. The only variable introduced by Evans into this equation with the type of time trend for performing this function is, of all things, the sum of average personal tax rate. (This rate, computed by Evans, has a strong positive trend over time. Whether or not this is a reasonable concept is another matter, since one could also compute the average rate which does not have as much trend). It is therefore not at all surprising that the average personal tax rate acquires a strong negative coefficient.

Evans seems to suggest that the definitional identity among manhours, output and the productivity does not apply here because manhours and output measures that enter the manhours equation reflect short-run, cyclical movements of these variables while PRD reflects the longer-run, secular trend of the productivity. This excuse does not wash because PRD is simply calculated as the ratio of output to manhours in each year, and to reflect this fact, the equation explaining the rate of change of PRD has explanatory variables that are strictly related to short-run, cyclical variation in productivity, such as the rate of change of GNP and the index of capacity utilization.

I must conclude, therefore, that Evans' formulation of the manhours equation makes no sense, that its fit against data is purely accidental, and that the large negative coefficient for the sum of average personal income tax rates estimated in this equation is at best due to a combination of vagaries of the pattern of time series data and of serious misspecifications of the equation form.

I would like to repeat here a curious feature of this manhours equation observed by Steve Braun (Braun, 1981). Since output and the personal income tax rate enter separately as independent variables in this equation given the level of output, an increase in the average personal income tax rate will reduce manhours. That is, the higher the average personal income tax rate, the higher the productivity per manhour. I am sure those who are interested in supply responses to a change in the tax structure are interested in getting an explanation for this phenomena.

The only other place where the variable PRD plays a role is in the equation defining the maximum production. It is a definition rather than an estimated equation, and takes the form (Evans, 1980, p. 11.11)

$$XIPC = (EM^*)^{2/3}(K)^{1/3}e^{PRODQ}$$

where

XIPC: index of maximum production in the manufacturing sector 1967 = 100.0
EM*: "full employment" supply of labor in manhours
K: "stock" of capital goods, somehow measured

We shall not discuss the serious problem of how EM* and K are measured by Evans, since the focus of our discussion here is how the measure of productivity is utilized in the model. Evans says that PRODQ is the annual change in private nonfarm business productivity. Evans could not mean what he says, since if we take him literally, it makes no sense, and I don't believe that he could have generated the data reported by him preceding the specification of this definition (Evans, 1980, p. 11.10). I therefore assume that PRODQ is something that does make a minimum of sense, say, the accumulated value of the rate of change of productivity starting from some initial period, with the initial value of it coordinated with the constant term in the definition so as to fit the data.

Even then, this equation makes no sense. If PRODQ is some concept such as the one I suggested above, and in any case it is based on the measure of productivity per manhour, then anyone who has ever worked with growth models based on homogeneous production functions, particularly the Cobb-Douglas function, will know that the productivity measure cannot be introduced into the production function unmodified. This is because productivity per manhour already reflects the contribution of an increase in the capital-labor ratio, as Evans' equation explaining the rate of change of PRD tries to describe. Therefore its introduction together with the capital stock into the production function without the proper restriction is a double counting. One possible, though rather naive and unrealistic way to handle this problem is to replace the term e^{PRODQ} by $e^{\frac{1}{3}PRODQ}$ in the above equation defining XIPC (assuming, always, that my reinterpretation of Evans' PRODQ is basically correct). At least, this will make the structure logically consistent.

Even if PRODQ is introduced correctly into a Cobb-Douglas production function, it is most doubtful that such a formulation will be adequate for estimating the maximum productive capacity of the U.S. economy. On a year-by-year basis, at least some of capital goods are not malleable. Hence, it is a doubtful procedure to utilize any production function for the whole economy (or a large segment of it) incorporating the concept of the aggregate capital stock in order to describe the production possibilities in the sense that Evans uses the concept of capacity or maximum output. Moreover, the

depreciation or abandonment of capital goods may very well depend on movements of relative prices. But this is really taking us too far afield away from the subject at hand, namely, the most obvious defects of Evans' model.

In this note, I have so far limited myself to discussing the explanation of productivity in Evans' model and two sets of his equations in which the productivity so explained is a critical input. I have, however, looked at the large, 850 page document (Evans, 1980), which is an explanation of his model, and I must report that everywhere I turned, every equation that I have examined, I have objections rather similar in nature to the ones I have been discussing. Very few of his equations make "good sense" as this convenient term is normally understood by most of us economists, and most of them imply what I would consider rather absurd behavior of the dependent variable when one of its explanatory variables is changed from one level to another while all other explanatory variables are held constant. That is, most of his equations have what may be called "unacceptable steady state properties."

In his oral discussion, Evans took the position that he did not care what properties individual equations possessed, so long as the whole system generated dynamic behavior in simulation that appeared reasonable. Although Evans is not alone in taking this position[1], I for one do not consider this position a tenable one in building econometric models. Some misspecifications in short-run, dynamic behavior of some subsidiary equation might be tolerated, after a careful examination to make sure that such a misspecification did not affect the overall behavior of the system, either for good or for bad. The requirement that the whole system behave in an understandable, reasonable manner under a variety of shocks is a useful criteria in judging the quality and acceptability of any econometric model, but is a criteria in addition to, and not in place of, the traditional one that each equation in the system be sensible.

My review of Evans' two papers (Evans, 1980 and 1981), then, convinces me that the whole model does not make much sense, and I cannot have any confidence in his model nor in any analysis based on his model. I have seen many errors and bad judgments in many econometric studies, including my own. Seldom have I seen,

[1] I recollect that Jay Forrester tended to take a position somewhat similar to this in his writings in *Industrial Dynamics*, but I am unable to find a specific reference at the present time.

however, a large-scale work such as this one of Evans, undertaken by a reputable and experienced econometrician, where the pattern of such major defects have dominated so large a part of the entire work.

This is really too bad, because the case for rationalizing the tax and transfer payment structure of the United States seems to me to be quite strong. The shift from the personal income tax to the expenditure tax originally proposed by Kaldor has its appeal, provided that is is combined with adequate taxation of estates. I believe such a shift will make it much easier to handle the vexing problem of capital gains, to cope with inflation and indexing of the tax base, and may possibly stimulate savings. A great deal of work is beginning to be done in this area. I believe the rationalization of depreciation allowances should be pursued, and the immediate and complete write-off of capital good purchases as cost should be considered as one possible alternative, more in the case of producers' equipment than in the case of structures. Going beyond that, some form of integration of corporate profit tax, personal income tax, and the social security tax would be worth analyzing. An even more difficult problem is the coordination of taxation by the federal, state, and local governments. On the transfer side, any movement to make payments less dependent on income of the recipient is likely to be helpful. The aim is, as it always has been, to design the tax and transfer payment system which raises the needed revenue, approach the desired redistribution of income as closely as possible while minimizing the distortion of relative prices.

There are many careful studies of these possibilities, although they are all quite incomplete, and further research on them as well as open public discussion of these issues should prove helpful in formulating our economic policies in the coming decades. A work such as Evans' new model, undertaken at public expense, and well publicized, claiming so much and yet so misleading, is likely to divert the attention of both economists and the public away from basic issues and focus it on questionable gimmicks, raising false expectations in the process. I fear that it will, in the end, retard rather than advance the cause of fundamental reform of our tax and transfer payment structure. I hope that I am wrong in this premonition.

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