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The Economic Demand for Irrigated Acreage; New Methodology and Some Preliminary Projections, 1954-1980

By

VERNON W. RUTTAN

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This book is well written, well organized, and a noteworthy attempt to apply quantitative methods to problems in the use of natural resources. Such efforts merit encouragement. However, there are a number of difficulties in the present volume which limit its usefulness. (It may be noted that some of the difficulties could be eliminated on the basis of information appearing in the text.)

Professor Ruttan attempts to project irrigated acreage to 1980 using economic optimization criteria, in contrast to projections based on "requirements," obtained by scaling down output projections to input requirements on the basis of an historical ratio of input to output or of trends in that ratio. Certainly, the requirements approach is open to criticism. Ruttan states this well: "Requirements projections implicitly assume that the projected amount of input will be used regardless of the costs of supplying it. . . . Use of the 'requirements' framework . . . involves the implicit assumption that resource combinations and consumption patterns are . . . inelastic with respect to changes in prices. . . ."[1]

In his contrasting "optimization" approach, Ruttan develops projections on the basis of estimated Cobb-Douglas production functions. These functions are fitted by least squares for individual regions on the basis of observations on county aggregates. There are nine western regions and seven eastern regions and only major irrigation counties in each region appear as observations. Agricultural output in dollars (X_0) is written as a function of: all farm workers (X_1), machinery investment (X_2), livestock investment (X_3), irrigated land (X_4), nonirrigated cropland (X_5), and current operating expenses (X_6). For some regions, only a subset of this set of explanatory variables is used in the final version of the fitted function.

The production function estimates are then used in estimating value of marginal product (VMP) for irrigated land and current

1. Pp. 1, 17.

operating expenses, X_4 and X_6, in current use—that is, at average levels. The equations to this point are termed "the productivity model."

The production functions are then used to develop projected irrigated acreage under two alternative approaches termed "the demand model" and "the equilibrium model." Both are profit maximization models; the demand model involves profit maximization under a constraint while the equilibrium model involves maximization without constraint. The constraint in the demand model is that regional output is fixed at a projected level for 1980. The 1980 output projection involves a "dampened trend" approach, which assumes that current trends have played themselves out by 1980.[2] The equilibrium model solution is viewed by Ruttan as "the long-run static equilibrium and is not explicitly associated with a particular date." Further, "it is presented not as a projection but as a spelling out of the implications of the relative factor prices and the estimated production function."[3] However, we are told that results for a specific date (1980 in this case) can be obtained by restricting movement to the equilibrium.[4]

Detailed projections of irrigated acreage by region are presented for both models. (There are complications in the equilibrium model which may confuse the reader. Thus, in Table 14, VMP of X_6 is set at 1.5 its price, apparently on the evidence of earlier studies. There is only limited discussion of the way in which the restricted equilibrium was constructed. And Tables 14, 16, and 17 label all equilibrium solutions as 1980 solutions.) These projections are contrasted with U.S. Department of Agriculture and Bureau of Reclamation projections to 1980.[5] This reveals marked differences for individual regions, particularly between both sets of Ruttan's projections and the agency figures. With respect to total United States acreages, Ruttan's figures are very close to U.S. Department of Agrculture "high" projections. Thus, the index of irrigated acreage in 1980 for both Ruttan and U.S. Department of Agriculture high projections equals about 170 on a base of 1954 equal to 100. The U.S. Department of Agriculture "medium" projection is 124. For the West, we find indexes of about 140 for U.S.D.A. high and

2. Pp. 56, 126.
3. P. 33.
4. P. 59.
5. Table 17, pp. 68-69.

Ruttan projections, an index of 120 for U.S.D.A. medium, and an index of around 180 for the Bureau of Reclamation projection.

In his last chapter, Ruttan considers policy implications and warns against too much and too speedy investment in water facilities, if such investment is not based on economic criteria. This is well stated. Unfortunately, the argument does not square too well with the results presented earlier, which indicate great economic potential for expanded irrigation. (It might be noted that the unrestricted equilibrium solution for the West, assuming current subsidies continue, turns out to be 9.4 times the Bureau of Reclamation projection.)

A number of difficulties in Ruttan's procedures may be documented, working through the demand, equilibrium, and productivity model in turn.

In the demand model, output by region is projected and then VMP of irrigated land is set equal to the price of irrigated land—that is, its cost per year. But this is inappropriate if current operating expense (X_6) is treated as a variable input. (The procedure used in obtaining X_4 must involve this assumption.) The proper maximizing criterion is that VMP divided by factor price must be the *same* ratio for each variable input. By forcing irrigated land VMP to equal its price and neglecting the effect on operating expenses, the VMP of the latter input can be driven well above or below its price. Thus, for the South Pacific region 1954 "projection,"[6] it can be shown that there is a return to X_6 of $12.50 per dollar spent at the margin. If the equal ratio criterion had been employed, projected irrigated acreage would be well below the level presented.

The equilibrium model presents even more serious difficulties. In particular, there is a problem involving returns to scale and an important question on firm versus industry and implications for the equilibrium solution.

Returns to scale is given by the sum of the output elasticities (the fitted coefficients) of the production functions. For the 16 regions of the study, 9 had increasing returns to scale (elasticity sum appreciably above one), and 4 had approximately constant returns to scale (an elasticity sum between 0.98 and 1.02). Maximization is carried out only with respect to X_4 and X_6; yet, in a long-run context, it seems quite reasonable to argue that the other factors

6. Table 14, pp. 54-55.

of production also ought to be treated as variable inputs. But in this event, no determinate levels of input and output are possible for the 13 cases of constant or increasing returns. (Maximization under increasing returns involves indefinite expansion; under constant returns, output is indeterminate. This is a consequence of the second-order conditions for a maximum of the calculus.) One might argue, of course, that as expansion occurred, production functions would change. But that does not help matters here.

The presentation of the equilibrium model as a static, timeless, long-run solution points up a basic difficulty of Ruttan's approach. This involves the treatment of the county as though it were a firm. An equilibrium level for the "average" county is obtained and then multiplied by the number of counties in the region to yield the regional equilibrium.

A static, long-run equilibrium makes sense for an individual firm (under appropriate assumptions, of course); but it does not make much sense for a region. Ruttan's equilibrium would indicate that there is a regional level of output which ought to hold for all time. A particular year projection is obtained by limiting the movement to this equilibrium. But note that the equilibrium is developed on the basis of 1954 conditions. It seems rather peculiar that a 1980 forecast can be had by "imposing restrictions on the rate of adjustment toward the calculated equilibrium levels."[7] On the face of it, this indicates an adjustment lag of some decades. To put it bluntly—why the delay? For 1954 conditions the equilibrium seems applicable to 1954, not to 1980. One would expect regional equilibria to change over time, increasing with population if nothing else. Yet, how are such changes to occur in this model? Further, on a more basic level, there seems to be an internal inconsistency. A long-run equilibrium is *not* a projection into the future. How then can we obtain a 1980 projection by moving part way to this equilibrium? By way of contrast, note that increases in regional output over time is straightforward if the firm and not the county is our unit. We simply increase the number of firms.

In the productivity model (or development of production function estimates), there are some major statistical difficulties. In seven of the regions, the final version of the production function used involved only a subset of inputs. Apparently, this was done when "the coefficient for irrigated cropland is not significant at an acceptable

7. P. 59.

level" in the original equation using all inputs.[8] It appears, then, if a test does not lead to rejection of the hypothesis of a zero coefficient, the author feels justified in omitting one or more variables. But this is very likely to cause omission-of-variables specification error. If the remaining inputs are correlated with the omitted inputs, their coefficient estimates will be biased. If the correlation is positive, the coefficients will tend to be overstated. For five of the seven cases here, the coefficient of irrigated land increased and, consequently, marginal products were probably overstated.

This point holds with most force for the South Pacific region; here, because there were only seven observations, only two inputs were used. The coefficient for irrigated land was quite high, equaling about 10 times the coefficient obtained for combined South Pacific and Colorado observations. The combined South Pacific and Colorado results were not used because of presumed lack of homogeneity between regions. This problem might have been met, however, by the use of a dummy variable for region.

Heterogeneity, noted between regions, may also occur within regions, since it seems quite possible for counties within a large region to be rather diverse. Now, if counties with large amounts of irrigated land are above the average in efficiency and in equilibrium input and output levels, then fitted functions will be biased upwards. More generally, lack of homogeneity among counties can bias estimates. This can be viewed as a variant of the simultaneous equation problem, about which the author seems too sanguine.[9] Finally, it was argued above that individual firm estimates make more sense in projecting regional output than do regional production function estimates. It would be very difficult to argue that the estimated regional coefficients are applicable to individual firms, however, because of what is known in the literature as the "aggregation problem."[10]

There are some additional, though minor, matters of concern. Thus, the dampened trend approach does not command assent; alternative assumptions seem as plausible. Again, Ruttan states that the geometric average yields the "typical" county while the arithmetic average yields an "average" county;[11] the source of this identification is unclear.

8. P. 93.
9. P. 32.
10. *Id.*
11. P. 41.

In the final analysis, what seems to emerge is this: there is some evidence that value of marginal product, for both irrigated land and current expenses, is above factor price—*if* one is willing to accept the county as the unit of observation and to assume a regional production function is representative of the average firm's production function. This may imply that it would be economic to expand use of both inputs. It may also reflect a risk premium. Even for this restricted set of results, then, policy is not obvious.

In evaluating overall methodology, we may note that a number of difficulties discussed above can be corrected. Thus, omission-of-variable bias would be eliminated by using all inputs for all regions. However, a good deal of doubt must remain on the acceptability of the county as unit. The development of individual farm estimates appears more defensible, and work building on Ruttan's efforts might move in this direction.

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