4-2-2008

Rejoinder

Alejandro Onofri
Universidad Nacional de La Plata, aonofri@fullzero.com.ar

Lilyan Fulginiti
University of Nebraska, lfulginiti1@unl.edu

J Prod Anal
DOI 10.1007/s11123-008-0094-6

Rejoinder

A. Onofri · L. E. Fulginiti

We thank the reviewers for a number of thoughtful and useful comments on the papers in this session. The instructions and data set received from the organizers were common to the three papers developed for this special session on modeling. Nevertheless, each study reflects the particular idiosyncrasies of the researchers and results on a very different approach to modeling a productive enterprise. The task for reviewers, therefore, is demanding.

Reviewers' suggestions span a broad range of topics. Most comments have to do with modeling issues, some emphasize issues related to the theoretical model, others emphasize issues related to the empirical model, and yet others have suggestions that touch both theory and empirics. In trying to respond to their concerns we find it useful to group our responses accordingly.

1 Theoretical modeling issues.

Førsund raises the most important issues relative to the economic model of choice. He questions the general choice of dynamic cost minimization as a theoretical model of firm behavior. The most important comment has to do with the assumption of this model in terms of firms' output. Dynamic cost minimization does assume that producers expected relative prices, output levels, and public input levels persist indefinitely. Every period, as new prices, output, and public inputs are observed, producers revise their expectations and re-optimize. That is to say that when deciding the optimal investment strategy for this period, the firm knows future input prices and output. We agree with this comment that this is an over simplifying and unrealistic assumption of this type of model of firm behavior. The alternative dual type models would be static cost minimization and dynamic profit maximization. If we were to, instead, use a static cost minimization approach we would be assuming ignorance about next period's output, moreover we would be implying that the firm does not really care about next period output when minimizing cost this period. If we, instead, would choose dynamic profit maximization we would be assuming knowledge of the output price trajectory, instead of the output quantity, in addition to the input prices trajectory when firms decide this period. None of these two alternative approaches are satisfactory either. We did stretch over the static approach to recognize that firm behavior today is affected by information about tomorrow and yesterday. And in fact, in our implementation of this model we do know the output trajectory during the years of analysis. We, nevertheless agree with **Førsund**, and think that dynamic profit maximization accounting for uncertainty and expectation formation would be a worthy next step.

A number of other issues are raised with respect to our modeling of government behavior. **Førsund** as well as **Tsionas** find our approach of a benevolent social planner with exogenous output price (small country assumption) oversimplifying and unrealistic. We agree that a public choice type of approach would represent government behavior more accurately if that were the main objective of the analysis. Our inclusion of the government and our emphasis in the process of production of public goods has

A. Onofri
Department of Economics, Universidad Nacional de La Plata, La Plata, Argentina

L. E. Fulginiti (✉)
Department of Agricultural Economics, University of Nebraska, Lincoln, NE 68583, USA
e-mail: lfulginiti@unl.edu

more to do with the endogenous growth hypothesis that motivates our study than with a close description of actual government behavior in general. We hypothesized endogenous growth for the sector. The sector includes not only the firms involved in using public goods, but the government involved in producing these goods, and then test for endogenous growth. In **Karagiannis** words, we view these public goods as external to the firms but internal to the whole sector, including in the sector the entity that produces these goods, the government. We think that, within this objective, the assumption of a benevolent social planner as a simplified model of government behavior is a start. The small country assumption for this sector (i.e. exogenous output prices) implies that the government can decide separately the optimal provision of public goods for producers and consumers, these decisions are separable. In addition, exogenous prices allow a theoretically consistent modeling of production waste. If we were instead interested in a measure of welfare that includes consumers then we would have to specify a welfare function for society (or an expenditure function) as well as the production structure (the cost structure used in this paper) and a budget constraint for the economy. We think that for the purpose of testing the endogenous growth hypothesis in the US agricultural sector, a focus on productive efficiency is a start as it is a necessary even though it might not be a sufficient condition for welfare maximization. We do agree though that, if one is comfortable with the specification of a social welfare function, including the consumer would lead to a broader measure of welfare that includes efficiency, as well as equity considerations. Producers and consumers decisions then might not be separable and some prices would be endogenous.

In this paragraph we answer some of the remaining theoretical questions raised by **Førsund**. The benevolent social planner uses shadow prices to adjust optimally. At a true steady state, private and public decision makers have reached their optimal levels of stocks and optimal replacement rate. If there are increasing returns to scale over all inputs (private and public), there is only a steady state "perceived" by the firm because it takes public inputs as given. In other words, for the firm there is a steady state for a particular level of G and R. However, if the government provides G and R to increase the returns of the firms, and consequently the firms invest in K, then, at the aggregate, there is no steady state but there is continuous growth.

It is plausible that given the multiple public goods provided by the government to different agents and sectors of the economy, some resources are wasted in the decision making process. At a minimum there will be costs related to the calculations needed for this reallocation of resources. These are part of the government's adjustment costs.

2 Empirical modeling issues

Comments on this section have to do with the data as well as with the estimation approach used and we will address them in this order.

Alvarez, **Thirtle**, **Karagiannis**, and **Førsund** offer comments on issues related to the R&D variable used in the study. We use two different public R&D stock variables, one is constructed from public R&D spending in agriculture reported by Alston and Pardey (1996) and used with the estimation based on the USDA data set. The other is constructed from public R&D spending in agriculture reported by Thirtle et al. (2002) and used in the estimation based on the data set provided by these authors. Both stocks were calculated as a weighted sum of the last 30 years of expenditures with weights following an inverted "V" pattern. A trapezoidal pattern of 35 years was also used with no significance difference in estimation. We do use only spending in agricultural R&D, we do build stocks, but we impose exogenously the length and shape of the lags basing our choice on work for US agriculture by Evenson, Huffman and Evenson, Chavas and Cox, and Alston and Pardey. We agree with **Thirtle** that endogenous determination of the length and shape is desirable and we are in the process of implementing this approach on work that is in process.

The public infrastructure variable represents public capital stocks obtained from the *Survey of Current Business* and includes buildings, highways, streets, sewer structures, etc., excluding military spending. This variable captures infrastructure stock for the whole economy and in this sense it might not be appropriate. More work is needed to identify that portion of public infrastructure that would most directly affect performance of the agricultural sector (for example Huffman and Evenson (1989) has used highway capital stock.) We recognize the problem and we place very little emphasis on estimates related to this variable. We are in the process of redefining this variable to use in new estimations that would also benefit from panel data for 48 US states.

We agree with **Alvarez** on the importance of the Extension variable and we are troubled by the omission as we mentioned in the text. We believe that this variable should not be bundled with public R&D stocks and should enter as a separate public input due to its distinctive nature and the potentially interesting interactions with the other public inputs. Our theoretical model is easily extended to include this additional public input but we cannot say the same of our empirical model. The addition of one more variable adds information but, given the short nature of the time series, the number of extra parameters implied by the flexible form, and the fact that public R&D stocks and Extension are highly collinear, we chose to omit this important variable. We are planning to include this variable in a new study where degrees of freedom have been increased due to the panel nature of the data.

We cannot say the same about the omission, mentioned by **Førsund**, of the private R&D stock in our model. The theoretical model we use only lends itself easily to the inclusion of this effect through prices paid by producers for innovations that are embodied mostly in inputs such a seeds, pesticides, and other chemicals. Private R&D is not a public input and it is produced under different degrees of appropriability and restrictive entry originating rents. If one is interested in modeling the process of private innovations it should be done allowing for an imperfectly competitive market structure as most of the literature has done since these earlier models of endogenous growth. We expect that a model that incorporates an explanation for private R&D stock would look very different than the one used in this paper.

In terms of embodied technical change, we know that USDA's data set has incorporated corrections for input quality changes. An inclusion of a time trend, as suggested by **Alvarez**, to 'pick up' everything that has not been captured by other variables is not easily done given the colinearity that exists between the trend and some of the other variables.

The most important comment on estimation choice is that of **Tsionas**, who comments on the potential rigidity imposed by the algorithm we use in the Bayesian estimation and suggests an alternative. The Bayesian estimates in our study were motivated by our discovery that the unrestricted maximum likelihood estimates did not satisfy the restrictions specified by economic theory. We do not know of any test that would help us decide the appropriate model that would balance the statistical characteristics of the data and the restrictions implied by economic theory. We felt uncomfortable giving a zero weight to the information in the data and of unity to the restrictions derived from economic theory if we were to proceed with restricted maximum likelihood estimates. Although we understand that behavioral restrictions are necessary in modeling the economic behavior of the firm, we decided to complement the maximum likelihood estimates with Bayesian estimates that would provide some sense of how restrictive are these economic requirements on this particular aggregate data set. It is in this spirit that we present the Bayesian estimates. We follow closely the procedures that Griffiths et al. (1999), O'Donnell et al. (1999) and O'Donnell (2002) have used on a similar data set for US agriculture. Other papers have used Gibbs sampling which, according to Chib and Greenberg (1996) is another type of Markov Chain Monte Carlo simulation method. In fact, Chib and Greenberg point out that Gibbs sampling is a special case of the Metropolis-Hasting algorithm. Examples are Atkinson and Dorfman (2005), and Terrel (1996). As a result of the Bayesian estimation, we do get the extra information we were looking for: that the behavioral restrictions are binding. In addition, we obtain a set of Bayesian parameters that could be use as alternative to the maximum likelihood ones. Given the importance we place on economic behavior, we nevertheless proceed to impose behavioral restrictions for the purpose of economic analysis. It is here where **Tsionas** contributions might make a difference as he hypothesizes that these results (i.e. the restrictions are binding) are driven by "the *inherent* inability of the general purpose MCMC algorithms (like the MH) to deal with stiff sets of inequality constraints." The implication would be that with the use of a more appropriate numerical technique we might reverse our conclusion that the restrictions are binding and then proceed to restrict the maximum likelihood estimates or use the alternative Bayesian estimates, but this time with more confidence and without apologies. This certainly would make our results more appealing.

Tsionas goes further and proposes an estimation approach, described in detail in his comments, that he thinks promising "to deal with stiff restrictions in general econometric models." We are curious about the implications for our study and are considering its implementation with improved data. It would certainly be the subject of another study and, if we proceed, we would communicate with Professor Tsionas and acknowledge his contributions.

At this point we would like to remind the reader about the restrictions required by the theory in this study. There are three *sets* of restrictions:

(1) those derived from economic theory of the firm and necessary for a technology to be 'well behaved' (i.e. equality, monotonicity, concavity, homogeneity, etc.);
(2) those derived from a dynamic formulation of producer behavior and necessary for convergence of the dynamic model (i.e. stability, Euler, adjustment cost, etc.); and,
(3) those derived from the hypothesis of endogenous growth for the sector (i.e. nonnegative shadow price of public inputs, public inputs increase steady state stock of private capital, and increasing returns overall factors of production, public and private).

In addition, even when the sector has two types of decision makers (private firms and government); in econometric implementation we were explicit only about private decision makers. Instead of specifying and fitting the value function for the benevolent social planner that nests the one for the private decision makers, we fitted the value function of private decision makers and instrumented the public inputs. This approach created some confusion

(**Karagiannis'** comments) and might have introduced additional problems in estimation.

Given the 'stiffness' of the restrictions, the potential problems in choice of instruments, and the use of aggregate data, before investing too much on alternative numerical algorithms, as suggested by **Tsionas**, we think it would be important to investigate the impact of the maintained hypotheses implied by a dynamic model, endogenous growth, the flexible form chosen, and the choice of instruments.

We realize that by instrumenting the value function of private decision makers instead of fitting the value function of the government (that nests the one of private decisions makers) we might have confused the reader. We agree with **Karagiannis** that public inputs are exogenous to private decision makers (but endogenous to the sector) and are treated that way in estimation. We do open the possibility that weak instruments might have interfered with this approach and that it might be better to explicitly fit the value function of the benevolent decision maker. In this case there would be no gap between theory and application, we would not have problems derived from potentially weak instruments, and it would be transparent that the endogenous growth hypothesis applies to the sector. On the other hand, we would need information on *r*, the rental price of G (see problem (8) in the paper).

This paragraph includes responses to other issues raised by **Tsionas**. We do agree that the draws are dependent by construction. We could use another algorithm that is not random but centered at the maximum likelihood estimator subject to restrictions but this would be appropriate for another study that could center on the importance on different algorithms for sampling. We look at the (non) stationarity properties of the data only as a diagnostic device and because other papers claimed that, given these characteristics, maximum likelihood would not be the appropriate. The analysis was presented in an appendix that is not part of the paper anymore and for purpose of session discussion.

The interactions between public R&D and public infrastructure are estimated as **Karagiannis** suggests. We are including comparison to other studies as **Thirtle** and **Karagiannis** request.

The main objective of the paper was not to estimate derived demands or TFP as **Buccola** seems to suggest, although we get them as byproduct. Our objective is to test a type of growth postulated by the AK model of endogenous growth (as in Romer and Barro). If this hypothesis is not rejected then we have evidence that public policy, through the provision of public inputs, could have had an enduring effect on sectoral growth as opposed to a one-time effect. This would make public inputs an incredibly powerful policy tool. To do so we model the agricultural sector as comprised of two decision makers, one public the other private, that realize that their present choices are influenced by the past, present, and future. In this context we derive specific restrictions (positive shadows of public inputs, a positive effect of public inputs on private capital, and increasing returns in all inputs) consistent with the AK-type of growth that we test. We are unable to reject the hypothesis given the numerous other maintained hypotheses derived from economic theory and empirical implementation that are nested in our problem. We do obtain estimates of derived demands, among other parameters, but these are not central to the paper. The TFP estimate **Buccola** suggests is an interesting and novel extension to this paper, although its actual estimate would be compromised given that the technology we estimate is not 'well behaved.' We proceed then with its derivation and estimation and present a summary of these results here (complete derivations and estimation in Onofri and Fulginiti (2006). Following the procedure in Luh and Stefanou (1991), but allowing here for changes in shadow values, TFP change can be expressed as

$$\widehat{\mathrm{TFP}} = \hat{y} - \Big[F_m + F_K + F_L + F_{JK} + F_{JL} + F_{\dot{K}} + F_{\dot{L}} \cdot \\ + F_G + F_{JG} + F_R + F_{JR}\Big] \qquad (1)$$

where F_m measures the proportional growth in variable factors; F_k and F_L quantify the growth in the stocks of quasi-fixed factors; F_{JK} and F_{JL} represent the weighted changes in the endogenous marginal values of capital and labor; $F_{\dot{K}}$ and $F_{\dot{L}}$ measure the growth in net investments; F_G and F_R measure the proportional growth in public inputs; and F_{JG} and F_{JR} reflect the growth of the shadow values of public inputs. Additionally, we define 'Private Scale' $= (1/\varepsilon_{cy} - 1)\,(F_m + F_K + F_L + F_{JK} + F_{JL} + F_{\dot{K}} + F_{\dot{L}})$ and 'Public Scale' $= (1/\varepsilon_{cy} - 1)\,(F_G + F_{JG} + F_R + F_{JR})$ where ε_{cy} is the elasticity of cost with respect to output. Finally, technical change is obtained residually:

$$\hat{\mathrm{A}} = \widehat{\mathrm{TFP}} - \text{'Private Scale'} - \text{'Public Scale'}$$

Table 1 presents the results for this decomposition of technical change. This table is similar to Table 4 in Luh and Stefanou (1991) and Table 5 in Luh and Stefanou (1993). The difference is that they do not include the components for changes in the shadow values.

The average rate of TFP growth for the whole period is 1.4%. The scale effect on private inputs explains almost 0.6% (40% of the growth rate). Although the average 'public scale' component is negative, the contribution of G and R to output growth is nearly 0.5% and 0.2% per year, respectively (components F_G and F_R). The 'public scale' component of −0.1% is explained by negative estimates for F_{JG} and F_{JR}. These two components represent the

Table 1 Decomposition of TFP change

Period	$\widehat{\text{TFP}}$	'Private Scale'	'Public Scale'	Technical change
Averages by decade				
1926–1930	−0.0011	0.0175	0.0022	−0.0208
1931–1940	0.0157	0.0167	0.0018	−0.0027
1941–1950	−0.0001	0.0525	0.0048	−0.0574
1951–1960	−0.0030	0.0584	0.0053	−0.0668
1961–1970	−0.0352	0.0867	−0.0044	−0.1176
1971–1980	−0.0212	0.0390	−0.0176	−0.0426
1981–1990	0.0703	0.0070	0.0036	0.0597
1926–1990	0.0142	0.0058	−0.0010	0.0094

contribution of changes in the shadow values of G and R due to changes in the quasi-fixed inputs K and L. More detail on theoretical derivation and estimates are found in Onofri and Fulginiti (2006).

References

Alston J, Pardey P (1996) Making science pay. AEI Press

Atkinson S, Dorfman J (2005) Bayesian measurement of productivity and efficiency in the presence of undesirable outputs: crediting electric utilities for reducing air pollution. J Econometr 126:445–468

Chib S, Greenberg E (1996) Markov chain Monte Carlo simulation methods in econometrics. Econometr Theor 12:409–431

Griffiths W, O'Donnell C, Tan Cruz A (1999) Imposing regularity conditions on a system of cost and factor share equations. Working Paper Series in Agricultural Economics, University of New England

Huffman W, Evenson R (1989) Supply and demand functions for multiproduct US cash grain farms: biases caused by research and other policies. Am J Agri Econ 71:761–773

Luh Y, Stefanou S (1993) Learning-by-doing and the sources of productivity growth: a dynamic model with applications to US Agriculture. J Prod Anal 4:353–370

Luh Y, Stefanou S (1991) Productivity growth in US agriculture under dynamic adjustment. Am J Agri Econ 73:1116–1125

O'Donnell C, Shumway R, Ball E (1999) Input demands and inefficiency in US agriculture. Am J Agri Econ 81:865–880

O'Donnell CJ (2002) Parametric estimation of technical and allocative efficiency in US agriculture, chapt 6. In: Ball VE, Norton GW (eds) Agricultural productivity: measurement and sources of growth, KAP

Onofri A, Fulginiti LE (2006) Dynamic TFP index with endogenous growth in US agriculture. Working Paper, Department of Agricultural Economics, University of Nebraska, Lincoln

Terrel D (1996) Incorporating monotonicity and concavity conditions in flexible functional forms. J Appl Econometr 11:179–194

Thirtle C, Schimmelpfenning D, Townsend R (2002) Induced innovation in United States agriculture, 1880–1990: time series tests and an error correction model. Am J Agri Econ 84:598–614