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FALSIFICATION AND THE PRACTICE OF AGRICULTURAL
PRODUCTION ECONOMISTS: A METHODOLOGICAL ASSESSMENT

by

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Abstract

Falsification and the Practice of Agricultural Production Economists: A Methodological Assessment

Lena Kivanda* and Glenn Fox**

This paper assesses the way in which agricultural economists have employed the neoclassical theory of production. For purposes of appraisal, we adopt the falsificationist perspective of Samuelson (1965), Silberberg (1978) and Blaug (1980). Appelbaum (1978) concluded that neoclassical production theory did not perform well with data from U.S. manufacturing. Our purpose in preparing this paper was to evaluate the empirical performance of production theory in agricultural, fisheries and forestry applications. To this end, we identified every paper that used econometric techniques to estimate cost functions, profit functions or systems of factor demand functions published in the American Journal of Agricultural Economics, the Journal of Agricultural Economics, the Australian Journal of Agricultural Economics, the Canadian Journal of Agricultural Economics, the Review of Agricultural Economics (formerly the North Central Journal of Agricultural Economics), the Southern Journal of Agricultural Economics, the Journal of Agricultural and Resource Economics (formerly the Western Journal of Agricultural Economics), the Agricultural and Resource Economics Review (formerly the Northeastern Journal of Agricultural and Resource Economics) and the Review of Marketing and Agricultural Economics from 1976 to 1991 inclusive. The results of a survey of these articles indicate that agricultural and natural resource economists have not taken falsificationist methodology seriously. In particular, the treatment of the falsifiable hypotheses of cost minimization and profit maximization as assumptions has been widespread. When these hypotheses have been tested, the track record of production theory is not impressive.

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I Introduction

The neoclassical theory of production has played a prominent role in agricultural economics research and teaching from at least the time of John D. Black (1926). In the modern era, Beattie and Taylor (1985) and Chambers (1988) have provided exposition of developments in duality theory with its implications for estimation of profit, cost and factor demand functions. It is difficult to find an issue of any of the leading agricultural economics journals which does not contain at least one article which takes its inspiration from the neoclassical theory of production.

Our purpose in writing this paper is to question the apparent confidence that our profession has in this theory. Our aim is methodological. Our orientation is the version of Popperian Falsification expressed by Samuelson (1948), Blaug (1980) and Silberberg (1978). According to this view, if economic research is to deserve the appellation "Science", it must pass the bar of Popper's Demarcation Principle. In Popper's view, the characteristic quality of science is a thoroughgoing emphasis on the derivation and testing of refutable hypotheses. A refutable hypothesis is a proposition, deduced from the underlying axioms of a theory, which can at least conceivably be contradicted by observation. Blaug concludes that adherence to the methodological doctrine of falsificationism, while it may be difficult for economists to achieve, is, nevertheless, the appropriate goal for researchers. He bemoans the fact that falsification receives widespread but superficial support among economists.

In 1978, Eli Appelbaum published an important but sadly a neglected contribution to the neoclassical theory of production. His paper adopts a falsificationist perspective. The critical test of correspondence in a falsificationist theory appraisal is the test of the falsifiable hypothesis. Appelbaum performed such a test for the neoclassical theory of production using data from U.S. manufacturing. He found the theory wanting. The data used in his study contradicted the refutable

hypothesis of neoclassical production theory. His conclusions are a consistent but cautious expression of Blaug's interpretation of Popper. In particular, Appelbaum states (p.98)

When rejecting the null hypothesis we have in fact rejected the whole set of assumptions identified with the neoclassical theory of production

and later (p. 102)

The main conclusion to be drawn is, therefore, that one should be careful in his interpretations of the empirical results obtained on the basis of neoclassical production theory. While this is not a very constructive conclusion, it is nevertheless worth remembering.

But what about agricultural economics? Perhaps Appelbaum was unlucky! The purposes of this paper are to analyse the extent to which agricultural economists have followed the falsificationist protocol (Figure 1) and to evaluate the extent to which the theory has passed the Popperian test of validation.

II The Refutable Hypotheses

The scope of our review is limited to the static configurations of production theory under conditions of perfect information. Since descriptions of the structure of the theory are available elsewhere (Chambers, 1988, Beattie and Taylor, 1985, Silberberg, 1978) we only wish to highlight the common structure of the theory in each of these configurations. In each of the four cases, the behaviour of the firm is represented as a constrained maximization problem. Samuelson's (1948) insight is that the mathematics of constrained optimization has a logical structure that produces what he called "operationally meaningful theorems". In Figure 1, we list the four generic categories of meaningful theorems, or in Popper's words, falsifiable hypotheses. These categories are;

1. Homogeneity

This hypothesis states that proportional changes in price have no quantity effects. For example, a 10% increase in all factor prices leaves conditional factor demands unchanged and the value of the optimized cost function increases by 10%.

2. Monotonicity

The theory makes unambiguous predictions about the sign of the effects of some price variables on quantities and on the value of the optimized criterion function. For example, in the rubric of the profit function, product supply is not downward sloping in the price of output and profits do not increase if input prices rise.

3. Curvature

Profit functions are weakly convex in prices and cost functions are weakly concave in input prices.

4. Symmetry

Since the optimized values of each of the criterion functions are differentiable and since factor demands and output supplies can be obtained by differentiation with respect to the relevant price, it follows from Young's theorem that cross-price effects of supply and factor demand functions are equal.

These four categories exhaust the necessary refutable hypothesis of the neoclassical theory of production under perfect information. They therefore define the playing field for a falsificationist validation of the theory.

III Procedures and Results

Our data consist of articles published in nine agricultural economics journals from 1976 - 1991. We selected all articles published in the American Journal of Agricultural Economics, the Canadian Journal of Agricultural Economics, the Journal of Agricultural Economics, the Western Journal of Agricultural Economics, the Southern Journal of Agricultural Economics, the North-Eastern Journal

of Agricultural Economics and the North-Central Journal of Agricultural Economics¹, the Australian Journal of Agricultural Economics and the Review of Marketing and Agricultural Economics that used econometric techniques to estimate cost or profit functions or a related system of factor shares or demands. Seventy-one articles were identified. This study adopted Cozzarin's (1989) approach. Each article was examined to determine which of the four relevant hypotheses were tested and to see whether the tested hypotheses were refuted. Information from selected articles was tabulated to show the year of publication, author, output, data and estimation period, estimated function, functional form, estimation procedure and test results.²

Our findings are reported in 5 tables. Table 1 summarizes all 9 journals for the entire time period. Table 2 reports the results for the American Journal of Agricultural Economics, in which 33 of the papers in our survey were published. Tables 3, 4 and 5 summarize, respectively, the results for the other 8 Agricultural Economics Journals, for papers published between 1976 and 1987 and between 1988 and 1991.

Results of the analysis are disturbing. They show a distinct lack of seriousness among agricultural economists in following the falsification doctrine. From Table 1, about 58% of the articles (41 of 71 articles) showed some form of testing. However, only one of the seventy-one articles tested all four hypotheses, 11.3% tested three, 31% tested two and another 14% tested one.

Surprisingly, the extent to which at least one hypotheses was tested was substantially lower in the American Journal of Agricultural Economics (46%, Table 2) than in the other eight journals (68%, Table 3). Among the papers in which hypotheses were tested, most authors tested only 2.

¹ Renamed the Review of Agricultural Economics in 1991, the last year of our survey. No papers satisfying our selection criteria were published in the NCJAE/RAE during this time period, 1976-1991.

² A summary of the tabulations is available from the authors on request. A list of the papers included in our survey is included as an appendix to this essay.

Tables 4 and 5 indicate some improvement in the commitment to test at least one of the refutable hypotheses, in that the percentage of papers in which no hypotheses were tested fell from 47% to 37% in the most recent half of the papers we considered.

IV Discussion

Mark Blaug's (1980, p. 254) contention is that

...the central weakness of modern economics is, indeed, the reluctance to produce the theories that yield unambiguously refutable implications, followed by a general unwillingness to confront those implications with the facts.

The neoclassical theory of production is a case, not acknowledged by Blaug, in which the unambiguously refutable implications have been produced and are well understood by practitioners. Apparently, however, agricultural economists have been as reluctant as other economists in testing the correspondence of the theory of production with the external world. In 71 articles published in agricultural economics journals between 1976 and 1991, only one of the studies even attempted a test of all four of the relevant refutable hypotheses. And this is for a sphere of application of the neoclassical theory of production, agriculture, that economists generally accept as corresponding most closely to the competitive ideal.

Several arguments have been made by agricultural economists in defense of the theory. It is difficult to see these arguments as anything different from Popper's maligned immunizing strategies. One argument is provided by Blaug himself, in a retreat into Lakatos' Methodology of Scientific Research Programs. The claim here is that the theory of the firm is just one element in the Research Program of neoclassical price theory. Theory appraisal should be directed at the program level. The important question, in a Lakatosian lexicon, is whether the program is progressive or degenerating. Blaug himself is caught in a cul-de-sac with the argument, concluding that even if one accepts the

Methodology of Scientific Research Programs as the appropriate framework, it is hard not to conclude that neoclassical price theory is a degenerating research program. Furthermore, if we accept Cozzarin's evidence that consumption and demand theory has also performed poorly on a Falsificationist criterion, it is hard to believe that the research program of neoclassical price theory, being based in the main on production and consumption theory, is thriving.

The most common defense offered by agricultural economists rallying to the aid of the theory is that the data are defective. This is a variation on Duhem's thesis that all hypothesis tests are joint tests of the theory, the data, and the procedures of observation, measurement and analysis. Aggregation is frequently suggested as an aspect in which the data are deficient. According to this argument, the theory has been developed at the level of the single firm or even for a single enterprise or division within a firm. The data are frequently observed at an industry level or for firms which are more organizationally complex than the theory anticipates. But this is a most peculiar argument. Why would an economist, in the process of testing and estimation, use data known a priori to be inadequate for the task at hand? Or was this inadequacy only revealed after the hypotheses tests had been performed? Furthermore, if one accepts this argument, then the basis for the widely accepted practice of imposing the refutable hypotheses as "theoretical restrictions" on models in order to calculate factor demand and product supply elasticities, measures of returns to size, scale and scope and elasticities of substitution must be repudiated. If the data are not good enough to pose an adequate test of the theory, how can they be good enough to provide the basis for these calculations?

Another popular excuse is that the researcher never intended to test the theory, but merely to use it for some less ambitious purpose, such as evaluating the capacity of U.S. agriculture to adjust to an increase in energy prices. Such an approach, which mimics the relationship between, say, the natural sciences and engineering, would be appropriate if the theory being used has been validated by someone else and it has been demonstrated that the present application takes place in

circumstances comparable to those in which validation was achieved. Of course, this is never the basis for this defense.

The final and arguably most pitiable arguments are the "Lack of Suitable Alternatives" and the "But everyone does it" defenses. Anyone who has been a teenager or has raised teenagers has heard the second argument, and is familiar with its rebuttal. The first defense is equally uninspiring. Furthermore, it is, on its face, false. There are alternative models or theories of the firm, and for the researcher, there is always the alternative to search for undiscovered alternatives.

None of the leading production economics textbooks used in North America discuss the methodologist's question of the correspondence of the theory to the external world. Given our findings, this is not surprising, but there is hardly a justification for so serious an omission. There are non-Popperian methodologies in economics (see Gerrard for a survey), but our impression is that agricultural economists generally want to be falsificationists. This creates an awkward dilemma. If we accept a Popperian view of methodology, then we must be much more circumspect in our role as advisors and explainers. If we reject the falsificationist protocol, what shall we use to replace it?

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Figure 1: Overview of the Falsificationist Protocol

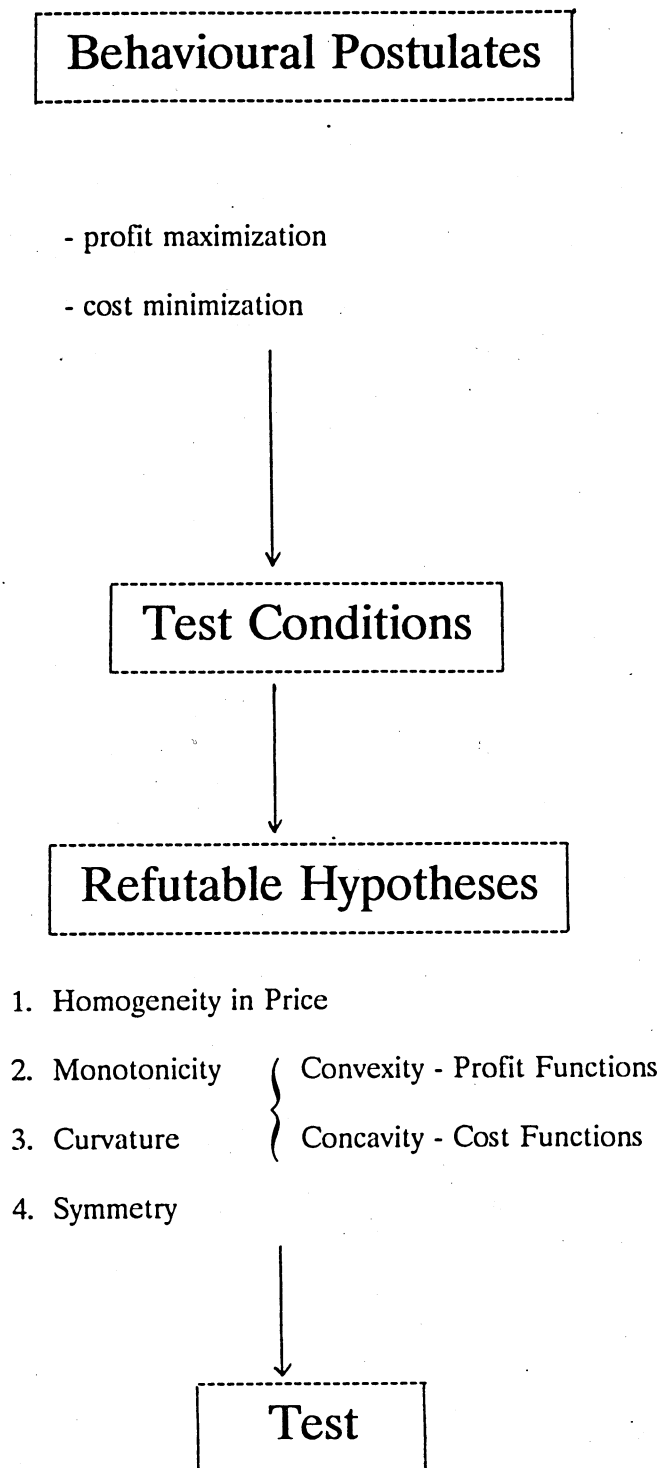


Table 1: Total Sample, 9 Journals, 1976-1991

Category	Number of Articles (percent)	Number of Hypotheses Tested	Results of Tests				
			Four Not Rejected	Three Not Rejected	Two Not Rejected	One Not Rejected	All Rejected
No Hypotheses Tests	30 (42.3%)						
Hypotheses Tested	10 (14%)	1	-	-	-	4	6
	22 (31%)	2	-	-	17	5	0
	8 (11.3%)	3	-	3	2	1	2
	1 (1.4%)	4	1	0	0	0	0
	<u>41</u> (57.7%)	<u> </u>	<u> </u>	<u> </u>	<u> </u>	<u> </u>	<u> </u>
Total	71 (100%)		1 (1.4%)	3 (4.2%)	19 (26.8%)	10 (14.1%)	8 (11.3%)

Table 2: AJAE Articles, 1976-1991

Category	Number of Articles (percent)	Number of Hypotheses Tested	Results of Tests			
			Four Not Rejected	Three Not Rejected	Two Not Rejected	One Not Rejected
No Hypotheses Tests	18 (54.5%)					All Rejected
Hypotheses Tested	5 (15.2%)	1	-	-	2	3
	8 (24.2%)	2	-	-	7	0
	2 (6.1%)	3	-	1	1	0
	0	4	0	0	0	0
	15 (45.5%)					
Total	33 (100%)	0	0	1 (3.0%)	8 (24.2%)	3 (9.1%)

Table 3: Articles From Other Agricultural Economics Journals, 1976-1991

Category	Number of Articles (percent)	Number of Hypotheses Tested	Results of Tests				
			Four Not Rejected	Three Not Rejected	Two Not Rejected	One Not Rejected	All Rejected
No Hypotheses Tests	12 (31.6%)						
Hypotheses Tested	5 (13.16%)	1	-	-	-	2	3
	14 (36.8%)	2	-	-	9	5	0
	5 (13.2%)	3	-	2	2	0	2
	1 (2.6%)	4	1	0	0	0	0
	26 (68.4%)						
Total	38 (100%)		1 (2.6%)	2 (5.3%)	11 (28.9%)	7 (18.4%)	5 (13.2%)

Table 4: Articles From 1976-1987, 9 Journals

Category	Number of Articles (percent)	Number of Hypotheses Tested	Results of Tests				
			Four Not Rejected	Three Not Rejected	Two Not Rejected	One Not Rejected	All Rejected
No Hypotheses Tests	17 (47.22%)						
Hypotheses Tested	3 (8.3%)	1	-	-	-	1	2
	12 (33.3%)	2	-	-	8	4	0
	4 (11.1%)	3	-	2	1	0	1
	0	4	0	0	0	0	0
	19 (52.8%)						
Total	36 (100%)		0	2 (5.5%)	9 (25.0%)	5 (13.9%)	3 (8.3%)

Table 5: Articles From 1988-1991, 9 Journals

Category	Number of Articles (percent)	Number of Hypotheses Tested	Results of Tests				
			Four Not Rejected	Three Not Rejected	Two Not Rejected	One Not Rejected	All Rejected
No Hypotheses Tests	13 (37.1%)						
Hypotheses Tested	7 (20%)	1	-	-	-	3	4
	10 (28.6%)	2	-	-	9	1	0
	4 (11.4%)	3	-	1	1	1	1
	1 (2.9%)	4	1	0	0	0	0
	22 (62.9%)						
Total	35 (100%)		1 (2.9%)	1 (2.9%)	10 (28.6%)	5 (14.3%)	5 (14.3%)

Appendix Articles Included in Survey

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