Scientific Principle and Practice in Agricultural Economics: An Historical Review

Harold F. Breimyer

Ever since their field of learning took its own identity, agricultural economists have seen themselves as dedicated to the scientific principle. In early years that commitment was trusted as a guard against human fallibility, and the more so as the first investigations relied heavily on statistical data and analyses—supposedly bias-free. Thus originated a lasting emphasis on the quantitative. A need for underlying economic principles, recognized at once, was developed in depth beginning in the 1950s. Research taxonomies, outlined in the 1920s, devolved later into an array of research methods. Scientific practice is viewed also as how scientists "behave," and writings about agricultural economists' record are couched in terms of values, objectivity, ethics. Despite just claims to systematic rigor, science responds also to unsystematic elements, among them imagination and hunch. Most engaging in literature are never-ending exchanges about the merits of mathematics and model building.

Key words: research methods, scientific principle.

Agricultural economists are diverse and can be disputatious, but they have one common bond. They see themselves as functioning in the realm of science and dedicated to the scientific principle.

It is, therefore, something of an anomaly that in the annals of their discipline, inquiries into the meaning of that principle and its significance to their field are reported only episodically. It is as though, having embraced the doctrine of science, practitioners in agricultural economics have felt little need to meditate on how their endeavors fit into the schema of learning known as science, or even to ponder what that schema means. Nor do they anguish over what constraints are thereby placed on them.

With a slight literary license the history of agricultural economics can be said to be a sequence of counting, correlation, and econometrics. The early years of collection of data were followed by a decade of fascination with correlation and then by a long dedication to econometrics with all its model building and intricate statistical analysis. There may be whimsy in the lexicography of the three terms, which are of Middle English, Latin, and Greek origins, respectively. Is the profession reverting to its roots? The earliest professional writing in agricultural economics had the simplest of themes. It was to "get the facts," which in most instances were numerical. That focus led to a major emphasis on data gathering, that proved also to be precedent setting as it established a lasting predilection for quantitative analytical techniques.

In 1963 Don Paarlberg scolded agricultural economists for their bent to quantify everything and omit the nonquantifiable (p. 1390). But quantitative studies continued to crowd nonquantitative ones out of the pages of the American Journal of Agricultural Economics, particularly the four quarterly issues.

From the early emphasis on numbers, an inference can be drawn that has never vanished. Subtle and not citable as such in literature, it is a faith that the practices of the scientific world can be so impersonal and trustworthy as virtually to ensure against the perils of human fallibility.

Apparently, during the profession's first years numbers were seen as pure and their message easily read. Statistical data were regarded as self-identifying as to their relevance and the interpretations to be drawn from them. The literature of perhaps the first quarter century reveals little

Harold F. Breimyer is Professor Emeritus of Agricultural Economics, University of Missouri.

Copyright 1991 American Agricultural Economics Association
introspective thinking as to what premises or theories necessarily underlie the design of investigatory research or bear on the analytical technique to be applied. It seems incredible that in the 1021 pages of the Taylors’ grand Story of Agricultural Economics (Henry and Anne Dewees), comments on research method are confined to the briefest of notes on five reports published by the Social Science Research Council. The fragments total scarcely more than a single page (pp. 161, 894, 963–64, 990, 1019–20).

Analytically, during the 1920s the running of averages, frequency distributions, and simple cross-tabulations gave way as the mathematical technique of simple and multiple correlation took the profession by storm. Mordecai Ezekiel’s first writing in the Journal of Farm Economics appeared in 1923 (pp. 198–213). The Journal of the next year carries an article in which Henry Schultz demonstrates the applicability of multiple correlation to a topic that has been copy-book for aspiring agricultural economists from that day to this, namely, the demand for beef (pp. 254–78).

The faith that numbers will tell their own story without risk of economists’ fallibility was extended to the new technique. It was a shock when Elmer Working pointed out, in a now-classic 1927 article published in the Quarterly Journal of Economics, that price-quantity pairings do not even reveal whether a supply or a demand relationship is being described—the identification problem.

Periodically since Working’s warning, someone has reminded that there is an element of treachery to the coefficients that correlations produce so readily. In the 1941 and later editions of his price analysis text, Geoffrey Shepherd inveighs against failing to distinguish between correlation and causation (1941, pp. 137–38). A. N. Halter rings the same bell and alleges that economists generally are unclear about causation (1958, p. 1872). In his textbook Shepherd adds a line, almost of apostasy, that “all a correlation coefficient shows is that if the figure is high the relationship is unlikely to be due to chance” (1941, p. 138). As recently as 1988 the correlation-causation issue resurfaced. Thurman and Fisher draw on barnyard examples of the chicken and the egg to treat, half-facetiously, weighty discourses on (alleged) causality in the macroeconomic world (p. 237).

By the time econometric techniques appeared on the scene, the intellectual climate had changed. To be sure, some of the new enthusiasts with antecedents from early years were quick to pin faith in the miracle-producing power of the new system. But by that time agricultural economists as a genre had become more sophisticated. What followed was a steady flow of exchanges that sometimes dipped into inelegant coinages such as GI–GO (garbage in–garbage out) but more often examined perceptively how numbers-handling or manipulation is constrained by the precepts of science and the scientific method. A vignette from writings about the econometric era is presented as an epilogue to this review.

In pages that follow will be sketched the principal currents of thought as to the scientific principle in agricultural economics, dating from the profession’s earliest years. Sources are primarily the writings in this journal and its predecessors, together with selected other publications.

The Meaning of Science, Place of Theory, and Taxonomy of Research Techniques

The first page of the first issue of the Journal of Farm Economics carries an editorial promise that the new publication will “aim to be a seeker for and an expounder of the scientific facts. . . .” In the third issue, published the same year (1919), Richard T. Ely writes of the need for “an agricultural society whose aim is scientific preparation for sound agricultural legislation” (p. 109). Three years later W. J. Spillman, one of the early seminal thinkers, observes that “a science of farm management [lies] hidden in the field” (p. 99). Thus Spillman and Ely, between them, were concerned for science both in managing farms and enacting laws.

But neither they nor their contemporaries addressed the abstract meaning of science. Before long a number of journal writers began to nibble at the edge of the meaning of pure science. E. W. Allen must be credited with being the first contributor to the Journal of Farm Economics to take up the cudgel of the meaning of science to agricultural economics. “The aim of science is to discover order, something which occurs with regularity, represents a relationship, or may be expressed in a law. . . . Science assumes order and its task is to discover it” (p. 18).

In 1956 Shepherd dipped briefly into the meaning of science and its bearing on research (pp. 8–14). It remained for Halter in 1958, however, to initiate a roundtable-by-rejoinder (a common pattern) on the topic. Halter found it necessary to begin with the definition of three
words, the first of them science, "meaning that type of mental activity" which satisfies the conditions of "orderly or systematic thinking" and a definite subject-matter" (p. 1871). Two years later Halter wrote again, incurring Back's rejection of what he sees as Halter's "purely mechanical theory of human conduct" (1960, p. 1474); whereupon Conklin contends that agricultural economists have overly "abstracted from flesh and blood men" and have done so as they "idolized Newtonian mechanical concepts as the acme of science..." He calls the Newtonian mechanical image unrealistic..." (1960, p. 1475).

Halter and Jack take up the subject later (1961), and Back once again offers a discouraging judgment that "none of the particular philosophies of science currently in existence is sufficient as a philosophical guide to research in all the problems of agricultural economics" (1961, p. 908). Kelso reverts when he oversimplifies, saying, "The essence of science is predictability" (1967, p. 224).

Meanwhile, John Brewster, who came to be known as the leading philosopher among economists, wrote and taught about the ancient and modern meaning of science. In lectures at the Graduate School of the U.S. Department of Agriculture, published in part in A Philosopher Among Economists, be began with a put-down. In a line paraphrased from the author's memory he said, "I don't want to descend into the snakepit of what constitutes science." In reality, in his writings he does not dissent from general statements such as that of Allen cited above. His objection is more nearly a corroboration of Back's denial of the usefulness of philosophies of science. Targeting the "body of logic—principles of valid reasoning" that was so popular about mid-century, Brewster insists that much of what we need to know "simply cannot be caught up in any system of logic" (taken from author's notes). He doubts that scientific acquisition of knowledge can be highly systematic.

The subject is reintroduced in literature in presentations Kenneth Farrell, then AAEA president, scheduled in his 1977 program. Moles addresses "The Creation of 'Truth'...", and Hartman, "Economics of Science and as Culture." Both stress the cultural element in economic research. Moles: "Science and in specific social science" is "one of the many subcultures in western culture." Further: "Culture may be defined as human knowledge" (p. 919). Knowledge, in turn, is reality, seen as arising in human experience and not in the rote recording of phenomena (p. 920). Hartman: "The historical faith of science is in the ultimacy of a certain kind of natural order and the effectiveness of the scientific method to discover that order. . . . [But] all qualitative dimensions of human experience were [historically] precluded from scientific understanding. . . . Therein lies the problems of an inadequate cultural tradition" (p. 926). In other words, we can do better.

Strangely, few of the scholars who contemplate the meaning of science take particular note of the necessary process of abstraction. Not many point out that the scientific method inevitably involves abstracting from the complicated universe, reducing it to a scale human beings can deal with. Moles touches on this, as he writes that "any social science approach is used like all other knowledge systems to reduce the complexity of the environment" (p. 921). Breimyer, contributing appropriately to a volume dedicated to Geoffrey Shepherd, more specifically remarks on the abstractions that researchers employ—he calls them "the images of the universe to which agricultural economists address themselves" (1982, p. 57).

It would be hyperbole to credit agricultural economists with having conceptualized the esoteric notion of science and the scientific principle. But it would be inaccurate to the point of disloyalty to fail to acknowledge that agricultural economists have come a long way in achieving an astuteness of understanding of the scientific process to which they give their allegiance.

The Place of Theory in Research

Swanson in 1960 offers salient comments on the scientific concept, but even more pertinent is his interpretation of the relationship of theory to investigation. (Investigation is the original term, eventually replaced by research.) One assumes a theory, he writes, and goes from it to "observations instead of vice versa..." (p. 1484). This counsel may now seem axiomatic, but early agricultural economists not only saw no need for theory but were skeptical of theoretical formulations. Bushrod Allin quotes an unidentified outlook economist attending USDA's annual outlook conference, who declared, "We do not engage in theoretical research, ours is the job of analyzing the facts..." (p. 409). Even earlier (1940) Theodore Schultz writes that "there are
those among us” (otherwise unidentified) who are alarmed by any mention of “theoretical” and are “filled with sophisticated doubts about that lowly species known as the fact-finding empiricists . . .” (p. 60).

Historically, in 1921 Theodore Macklin recognized a need for an “understanding of the economic forces which underlie the production, marketing and consumption of farm products” (p. 41). E. W. Allen in 1926, however, apparently was the first journal writer to defend categorically the primacy of theory over fact-gathering. His strong language may be appropriate to university courses in research methods today.

My thesis is that a constructive purpose is essential to research; that without it there can be no real research. It is the central idea underlying the method of science, determining all else—the means of approach, the data to be secured, the course of procedure . . . (p. 16).

It is true that science progresses by the determination of facts . . . But isolated facts, however numerous, do not constitute science. To cite the familiar quotation from Poincaré: “Science is built up with facts as a house is with stones, but a collection of facts is no more science than a heap of stones is a house.” And similarly, the indiscriminate accumulation of facts without regard to any central idea or relationship is not in the category of research (p. 17).

Subsequent literature is sprinkled with refinements, such as distinctions between hypothesis and theory. Salter in 1942, after joining others in deploping the practice of compiling “stacks of tables, graphs and maps only to ponder over them unfruitfully,” declares forcefully that “hypotheses are a necessary condition to the sorting of facts . . .” (p. 238).

Halter in 1958 chooses to emphasize “presupposition,” which “refers to the context in which a true or false proposition is stated.” Also, “an absolute presupposition is never subject to the question of truth or falsity as is a proposition” (p. 1871).

In what must be regarded as one of the best journal articles on the role of theory in research, Christensen in 1966 notes that “there has been some misunderstanding . . . regarding the relationship between theory, hypothesis, and model.” Some writers, he explains, “would imply that model and theory are one or that hypothesis and theory are the same.” He continues, “The hypothesis is a conceptual relationship deduced from the formal theory already established and is a deduction derived from a set of known propositions . . .” (p. 138).

A model, incidentally, is seen by Christensen as “a conceptualization of an abstract system of relationships in terms of something more familiar . . .” (p. 138).

The second stage in the application of theory is to draw on the findings of a research study to revise or refine the preexisting theory. Halter would probably call this the forming of a new proposition. Swanson in 1960 touches on this second stage as well. “Any theory is tentative,” he writes, “and subject to further modification with the advent of incompatible evidence . . .” Moreover, he notes the “integration of probabilistic notions . . .” (p. 148).

Swanson, however, does not have many companions. It seems surprising that a profession that ploddingly learned to appreciate the place of abstract hypothesis, presupposition, or theory in design of research, has remained less secure in interpreting the findings other than as verification or rejection. There are exceptions. Christensen touches on the second stage, as does Moles. And Halter, as might be expected, offers cogent observations. The scientist, he writes, “realizes that his laws and theories are conjectures and tentative hypotheses and . . . may be rejected as false on the basis of new evidence” (1962, p. 224). Any reformulation comes next. But Halter is one of the few to write in these terms.

Halter almost certainly had picked up Karl Popper’s thesis about the falsifiability test, which shows up again in 1989 as Just and Rausser cite Popper’s belief in “falsification as the rigorous standard for scientific procedure.” They also quote Kuhn, who “found no support” for Popper’s thesis. Interestingly, they note without endorsing it that “among economists, McCloskey has advanced the view that economic research is basically essays in persuasion” (p. 1179).

All in all, after three quarters of a century of intrepid research into the economics of agriculture, agricultural economists still show considerable insecurity in the final stage of rejecting or reconstructing received economic theory, or composing a wholly new one.

**Taxonomies of Research Technique**

Even though they were tentative about articulating the scientific method, the earliest researchers in agricultural economics were quick to develop patterns in research technique. Owning to the profession’s obsession with numbers,
during at least the first ten years they gave most of their attention to the collection, analysis, and interpretation of statistical data. Among early concerns were those on how to conduct farm management surveys. Authors wrote about how to make telephonic surveys, for example.

*Journal* writers have had difficulties of terminology. Those difficulties persist, even complicating the writing of this review article. Economists use the language of research technique, systems of logic and of analysis, philosophy, research tools. Heady refers to the theorist as "tool maker." But he says the agricultural economist is a "tooluser" (1949, p. 837), implying that the agricultural economist does not ordinarily make his own theoretical tools. This viewpoint of Heady is consistent with the first years' engrossment with assembly of data, noted above.

As an arbitrary choice, the more systematic treatments of research technique found in literature will be reported in those terms. There is also a vast literature addressed to method, often mislabeled methodology. It will be noted briefly later.

The philosophy of science and systems of logic show up among the more classically trained agricultural economists, in terms such as induction versus deduction, and positive versus normative science. Conklin in 1947 reminds that "economics originated as a deductive 'science' . . . [consistently with] the Greek belief that all knowledge is derivative of 'pure reason.'" He adds that "inductive methods have come more slowly to economics than to most other branches of academic endeavor" (p. 926). Induction and deduction are rival concepts for Parsons, who sets forth other pairings, such as pure versus applied economics.

Halter, who rarely failed to get into a philosophical fray, refers to "the myth of induction" (1962, p. 223); and he and Jack introduce other terms from Latin and Greek: a priori versus a posteriori, and analytical versus synthetic. They believe "the key propositions of economic theory should be a posteriori synthetic" (p. 95).

Brewster, the acknowledged philosopher of agricultural research, did not fail to remark on deduction and induction in the scientific method. However, he writes that "the hard part of research is . . . discovering a conflict between prevailing theory and exceptional experiences. . . . Deductive development of well-stated hypotheses and testing their probable truth comes in very late stages of our search" (1970, p. 234).

A pairing of concepts that appears occasion-ally is positive versus normative. In a remark that still surprises, Conklin alleges that "economics began to lose its normative character . . . at the hands of the Mercantilists and the Physiocrats . . ." (1947, p. 926). Inferentially, many early agricultural economists hoped their endeavors could classify as positive, leaving them less subject to challenge in terms of implicit "values."

By any test Glenn Johnson must rank as the foremost thinker in such terms. He elaborates normativism and positivism into pragmatism and variants such as conditional normativism. He writes of "knowledge of values versus prescriptive knowledge" and so on (variably in *Research Methodology for Economists*). In a book reporting a 1985 symposium Tweeten invokes the Johnson credo, dallying with distinctions between normative and positive and then striking comparisons between prescriptive and descriptive (p. 142).

The most structured taxonomies for research technique begin with the monumental study commissioned by the Social Science Research Council referred to above. The report, published in the early year of 1928, touches on virtually all the ideas and terms mentioned thus far in this review: the scientific method (or methods), quantitative versus qualitative, induction versus deduction. The authors classify research technique into five categories: statistical, analogy, case, informal statistical, and experimental.

Leonard Salter, Jr., was a promising young agricultural economist who lost his life in the tragic LaSalle hotel fire in Chicago in June 1946. He still ranks as one of the most systematic thinkers in agricultural economics. In November 1942 he wrote about "cross-sectional and case-grouping procedures" in research, and the taxonomy he set forth in his book, published posthumously in 1948 and reprinted in 1967, features "an outline of inquiry" constituted of problematic situation, formulation of problem, hypothesis, processing of evidence, and a terminal test. The terminal test is as to whether "purposive action is instituted and consonant with consequences" (pp. 68–69). As to research technique (which Salter calls methods), the following are listed: experimental, historical, case, qualitative description, analogical, logical, and statistical (pp. 70–77).

For reasons that are not entirely clear, Salter's field of land economics has lent itself to discourses on research technique in its manifold interpretations. The papers for a symposium, published in a 1966 book edited by W. L. Gib-
son, Jr., R. J. Hildreth, and Gene Wunderlich, essentially captures the thinking of the time.

Several writers have since exhibited their own favorite taxonomies. Alan Randall, addressing resource economics (a successor to land economics in attention getting), reverts to pairings. He recognizes “four major schools of thought” and labels them “institutionalist/land economics; neoclassical/rational planning; public choice/utilitarian; and public choice/individualistic” (1985, p. 1022). Shabman follows by “recasting Randall’s schools . . . into two groups: the mainstream, which includes his N/RP, PC/U, and PC/I, and the institutional, or his I/LE.” Also, he (Randall) does not “distinguish resource economics from the larger discipline” (p. 1030).

If research technique is the most discretely identifiable structuring of research, ideas surrounding research philosophy are the least so. The term, philosophy, shows up randomly, with the single exception that John Brewster virtually built his career on philosophical concepts. The collection of Brewster’s articles, edited by J. Patrick Madden and David E. Brewster, has been noted above. Kenneth Parsons, in 1949, mulls over “what we may call the philosophical frame of reference” (p. 658). Karl Brandt, in 1955, recommends a study of philosophy; and Halter, citing Brandt’s faith that it “would lend clarity to our understanding of the nature of economics and to its research methods,” adds that “this reference to philosophy has gone unnoticed” (1958, p. 1871). Brewster’s star was rising fast at the time, but Halter presumably had not yet taken note of it. As is mentioned above, Halter and Jack later wrote about “a philosophy of science for agricultural economics research.” Back entered the fray in 1961, as did a few other economists later. Generally, though, except for the work of Brewster and just a few others, philosophy as orientation to agricultural economics has proved to be almost ephemeral.

The Catch-all of Method—or “Methodology”

A profession that has preferred to minimize the personal and maximize the procedural or instrumental fills its literature with discourses on operating tactics or procedural patterns, usually tagged by the Mother Hubbard term method or, sometimes and erroneously, “methodology.”

With regard to the term itself, Don Paarlberg is perhaps the most candid in objecting to methodology, as “this employment of the term is out of keeping with scientific usage . . . . Most agricultural economists use the word methodology when they are concerned neither with philosophy nor with logic but simply with method. The three syllables are added to convey prestige” (p. 1386).

Frank H. Knight, famed economist of the University of Chicago, told agricultural economists that he finds fault even with addressing method. “I must begin by confessing a degree of skepticism about the practical value of writing on ‘method’ (a term less pretentious than ‘methodology’) at a highly abstract level” (p. 112).

But the term method is itself ill defined. Perhaps each method can be viewed as a substratum in a research-technique taxonomy. A great many of the methods that are discussed in literature today relate to the three-century-long preoccupation mentioned at the beginning of this paper, namely, that with numbers.

In a major review published in volume two of A Survey of Agricultural Economics Literature, itself anticipated in a 1968 AJAE article, George Judge sketches historical works on what he calls estimation in economics. He puts methods of estimation in the category of measurement, and sees “a systematic use of economic and statistical models, methods, and data” as giving “empirical content to economic theory and practice . . . .” (p. 3).

As it is questionable whether the prolix and diverse writings about method can be tested against scientific principle, all that will be offered here is a sampling of the practices and conceptualizations that can loosely be called method. Veterans will recognize a number that were prominent briefly. Most of them disappeared. Faddism is not absent in agricultural economics.

It all began with methods of collecting data (the counting era). Next we find a panoply of “statistical methods of analysis” ranging from measures of central tendency to the new simple and multiple correlation, followed by simultaneous equations and other more elaborate forms. The particular equilibrium drew a brief focus of attention and methods were applied to it. In 1953 Fred Waugh, asked to review the applicability of developments in methodology, addressed method instead and listed the following, among others: sampling, design of experiments, consumer panels as data source, structural analysis, economic models generally (they were just coming into popularity), linear programming (due to have a long life), theory of games. In the same
issue of the Journal, Richard King introduces activity analysis. Input-output models show up periodically. Marc Nerlove made distributed lags a household word in agricultural economics. The Markov process has been popular intermittently, as has simulation. Dynamic programming, control theory, gini ratios, and a host of other terms and coinages dot the writings in agricultural economics. Some are of lasting merit, others a passing fancy.

In a book reporting a 1985 symposium (published in 1988), Glenn Johnson and Stanley Johnson present a comprehensive inventory of the methods currently at agricultural economists’ disposal. Glenn Johnson calls his list technological innovations, and Stanley specifically names quantitative techniques. A discussant, David Bessler, says Stanley surveys “virtually all interesting (useful) techniques in use today” (p. 199).

The Judge inventory of methods in estimation is another useful compendium (1977).

The almost countless designs for conceiving and carrying out agricultural economic inquiries, primarily quantitative in nature and commonly classified under the general term method, are a part of the profession’s fabric and yet are peripheral to the most signal tenets of the scientific principle. For this reason they are given only this serial mention.

On Behaving as a Scientist

It ought to have been evident even to the first economists who called themselves agricultural that it is not possible, or even desirable, to carry out agricultural economic studies so impersonally as to neutralize the human factor—converting, in Conklin’s language, the investigators into mechanics (1960, p. 1475). The role of the scientist is endemic to all scientific inquiry. As recently as 1989, when the prestigious National Academy of Sciences commissioned a thoughtful piece on “the nature of scientific research” the opening question, “Is there a scientific method?” was answered in terms that match the thesis of this review, that dedication to scientific principle does not lead to a singular system of scientific practice. It is significant that the resulting publication carries the title, On Being a Scientist.

Throughout the history of science, some philosophers and scientists have sought to describe a single systematic method that can be used to generate sci-
avoid and that continue to be unattractive to some members of the profession. It suggests that science and practices under its banner are partly a matter of the individual scientist's stance—his outlook, his Weltanschauung. A number of agricultural economists have been not only aware of this opposite side of science's coin but have pondered it and written about it. Again, the reference points have varied but have embraced concern for values, objectivity, morality, ethics.

The historical record shows several episodes of preoccupation with values. In 1949 Parsons touched on "the value problem in economic behavior" (p. 679). Frank Knight, reviewing Parsons, writes of "the ambiguous nature and true role of values" (p. 120). He sees two meanings to the word, value. "When used in economic theory, this term refers only to individual or subjective desire, as expressed in choice among alternatives presented" (p. 120). "In contrast," though, "economic policy, and all discussion of any common policy, inevitably refers to . . . some kind of 'objectivity,' some degree of right-and-wrong, capable of being argued." Further, "It involves a distinction between desire and choice as facts versus what they ought to be . . ." (p. 120).

In 1961 Earl Heady as director of the Center for Agricultural and Economic Adjustment at Iowa State University doubtless was instrumental in staging a seminar at that university on "Goals and Values in Agricultural Policy." The papers comprise a book bearing the title.

Many agricultural economists remained diffident about the idea of values, apparently satisfied with vague notions of cultural influences on a researcher. But in 1954 L. John Kutish demonstrated that "the value question" could generate the heat of spirited exchange.

Brewster, as would be expected, touched on values frequently: in "The Cultural Crisis of our Time" (A Philosopher Among Economists, pp. 7-65); in "Society Values and Goals in Respect to Agriculture" (Goals and Values in Agricultural Policy, pp. 114-37); and in "Beliefs, Values, and Economic Development" (Journal of Farm Economics, November 1961).

In all his writings Brewster rarely bothered to define the term, value. He regarded values, redundantly, as "judgments as to what is valuable," and he had society's judgments in mind (p. 158). He implicitly expected the agricultural economist as scientist to accept and be faithful to society's values, and not substitute his own.

Values remain a part of the vocabulary of agricultural economics, but the word has given way before a related concept that may entail even more sensitive responses, that of objectivity. As Heady suggests, objectivity itself can be regarded as a value; in a seeming contradiction, it adjoins the value-steeped economist not only to set aside his values (other than possibly the most elevated ones) as he addresses economic issues, but also to resist all enticements to bias or adulteration.

It may be supposed that agricultural economists generally subscribe, if only subliminally, to the principle of objectivity, yet are not eager for questions to be raised. In 1950 the over-stimulating Frank Knight, not one of the clan, called attention to the principle in relation to "fact" and "generalization" (p. 113). "Nature," he writes with more than a little whimsy, "is 'honest.'" But "in human conduct it is necessary to allow for unpredictability as 'affected by error' [his italics]," in several meanings, and by ignorance and prejudice; and where social relations are involved, "outright deception is a fundamental fact" (p. 114).

Harsh language.

When Breimyer in 1967 opened up the subject, he was more guarded or even gentle as he presented his stand that "if agricultural economics is to be a science . . . the precepts of scientific objectivity must be adhered to" (p. 340). He then diplomatically suggests that "the most persistent of all blocks to the objectivity test . . . [may be] the human tendency to avoid areas of controversy . . ." (p. 347). Even so, he does not fail to note "the most recent of all threats . . . [which] arises when an economist divides his time and allegiance so as to multiply his income" (p. 348). And he leaves no doubt that in his mind the test of objectivity must be met if agricultural economics is to "wear the esteemed toga of a science" (p. 350).

Whether the Breimyer literary fist was gloved or mailed, it incited responses. Grove rebuts that "most agricultural economists pretend to an objectivity they do not possess" (p. 153), and the only recourse is subjective dissent (p. 155). Schmitt and Timmerman concur in the judgment of nonattainability of objectivity (p. 921).

Castle, in another follow-up, admits that objectivity is hard to come by and lists a number of threats to it including desire for approval (pp. 810-11).

Alan Randall, the next economist to consider the behavioral paradigm of the scientist, examines "the moral responsibilities of the scientist and his host institution. . . ." He "explicitly acknowledges the role of power in economic interaction." The scientist and his in-
stitution are confronted with "the necessity of making distinct moral choices . . ." (1974, p. 227).
If Randall in 1974 selected the language of morality, in 1983 Maurice Kelso chose another variation on the theme as he set up at the AAEA annual meeting a session on "Ethical Issues in Resource Economics" (cf., e.g., the discussion papers by Glenn Johnson and Raymond Anderson).

And so, during forty years of journal exchanges among some of the leading lights in agricultural economics, the bearing of economists' behavior patterns on the scientific quality of the profession's output is examined in variable language but with consistent purpose. And in 1989 Just and Rausser reintroduce Heady's original term of objectivity. They call "insistence on objectivity . . . one of the dominant characteristics of the profession." As did Grove twenty-one years earlier, they see the best protection "in the clash of individual subjectivities" (p. 1179). Only if some individuals are objective and declare themselves can the majority be expected to be so.

The Unsystematic or Uncodified Component to Investigations in Agricultural Economics

In a striking contrast, agricultural economists' testimonials to science and formal scientific practice are mixed with occasional enjoiners to the opposite. Surely, systematic rigor is to be extolled and even mandated. But there is a place also for imagination, inspiration, even perhaps for day-dreaming; and for anecdotal evidence. A number of writers have so declared, sometimes insistently.

Swanson senses the place of intuition. "It is tempting to speculate on the future of the relative emphasis within our profession on the role of the informal predicting expert who uses judgment, intuition and wisdom and that of the researcher methodically seeking to explain phenomena" (p. 1485).

George Ladd carries the argument further, in trenchant language. "We pay little attention . . . to the research tools that are the most versatile and frequently used of all . . . [namely] subconscious mental processes (imagination, intuition, hunch), chance (including serendipity), and writing" (1979, p. 1). In his book, Imagination in Research, he explains further that "we need imagination to provide the insights leading to theories that fit the facts. And we rely on intuitions for the postulates that support our rules of logic" (p. 134). Ladd even goes so far as to note a place for doubt and tension as contributing to imagination for research.

Brewster, nevertheless, ranks as the foremost exponent of the unsystematic ingredient to research that may otherwise class as scientific: "Yet . . . there is much to the research quest . . . that cannot be reduced to systematic rules of right reasoning. No rule can be given that will induce one to perceive conflicts between prevailing generalizations and exceptional observations . . . [and] will lead one to induce . . . new hypotheses. . . . In this important sense, research includes far more than mere logic, whether deductive or inductive. It includes insight, genius, groping, pondering—"sense" which can't be boxed up in any formalized procedures. The logic we can teach; the art we cannot . . . (1970, pp. 234–35).

An Epilogue: The Game of Lambasting and Defending Mathematics and Model-building

For longer than a generation, agricultural economists have sparred over what its critics regard as an obsession with mathematics and the constructing of models, often intricate ones. Most of the exchanges have been good-spirited. There is substance to the debates, and a sampling is presented here.

This feature of the historical record is nevertheless presented as an addendum or epilogue because it is not fundamental to an account of the place of science and the scientific principle in agricultural economics.

Most of the verbal tussling has related to the third of the three stages of agricultural economics' preoccupation with numbers, that is, econometrics. During the years of counting, the lightest touch was to admonish field surveyors to phrase their questions carefully. "If the farmer can't answer, look to how you ask the question!"

A few ripostes followed the introduction of the exciting new correlation techniques. Don Paarlberg, for example, says we went crazy and "correlated everything with everything else . . ." (p. 1390).

The early beginning of arguments about the place of mathematics in agricultural economics is attested to by Thompson's pooh-poohing them (1937). "Those who make too strong a claim for the mathematical method appear to believe that this method can of itself yield valid conclusions
. . .” Not so: “there is no logical difference between reasoning without mathematics and that which uses higher mathematics. . . .” Then a pox on the other house: “The opposite view that the mathematical method is completely useless for economics usually rests on the mistaken identification of this method with measurement” (p. 718).

Arguments waxed warmer as research moved more fully into econometrics with all its intricate wizardry. In 1961 Karl Fox set up, at the winter association meeting, a session on “Contribution of Econometrics to Farm Price and Income Policy.” Breinweber in a discussion paper offers a judgment consistent with Thompson many years before, one that also anticipates the barrage of evaluations that fill the journals of recent years. “In the magic of electronics the limiting factor is not capacity of computation but expertise of programming. Moreover, neither the electronic machines nor mathematical concepts can compensate for any deficiency in underlying economic theory and understanding . . .” (p. 383).

Paalberg in 1963 calls it a delusion to think “that the new methods, being mathematical, shelter us from the hazards of human judgment . . .” (p. 1390). Two years later Norris Pritchard pleads for recognizing the limitations of mathematical methods (pp. 152–53). Thereupon Hildreth was moved to ask, at the 1965 annual meeting, “Have we gone too far?” He cites (pp. 1497–98) Paalberg’s further warning that “no method can produce rational results from erroneous data” (p. 1390) and Kelso’s harsher charge that “the elegance, the tidiness” surrounding mathematical models may only lead us to be “wrong in a more elegant manner” (1965, p. 11). For balance he quotes Waugh’s judgment that “sophisticated techniques are necessary to analyze difficult problems in our complex world” (1964, p. 866). And Heady: quantitative tools have greatly improved the agricultural economist’s ability to handle the masses of data necessary for analysis (1963, pp. 120–22).

In the February 1972 issue of the Journal, Oscar Burt and William Martin exchange barbs that are germane to the adequacy-of-data versus ingeniousness-of-technique dispute. Burt had written an article on the economics of investment in specified range improvements. In Journal pages the two economists combat as Burt defends his adapting a dynamic programming model to data of acknowledged shortcomings, leading Martin to charge that the exercise amounts to redefining a problem to fit “the specifications of the tool.” “Elegance,” he added (Kelso’s word again), “is the medium . . .” and has become the message (p. 134). Burt then alleges that Martin confuses “elegance with rigor” (p. 135).

Has much changed since George Warren sought to get better data to put into the farm management studies then being developed? Perhaps not. Warren, however, did not pretend to elegance.

The issue has stayed joined since Warren’s day, yet an emerging consensus may be sensed. If so, it is likely to be consistent with Hildreth’s judgment in 1965 that we have not gone too far in quantitative dexterity (p. 1501) and the real need is “for better training in economics and methodology . . .” (p. 1503).

A consensus judgment may be that employment of mathematical methods does not substitute for rigorous theoretical formulations in agricultural economics. Rather, it itself requires more precise understanding of the constructs of which the discipline is constituted.

Inasmuch as rhetorical exchanges among agricultural economists tend to be cyclical, a new outpouring of challenge and response on the practices attending the econometric stage in agricultural economics can be anticipated in years to come.

All of which introduces a jingle Frederick Waugh attributes to Herman Southworth (1953, p. 706):

Our economic methodology
is full of fine epistemology.
But when we come to problems practical
our theories are too didactical.
If economics is a science,
it needs to foster the alliance
of theorist and statistician,
with manager and prognosticist;
To tie the work of mathematicist
to problems of the market strategist.

[Received August 1990; final revision received November 1990.]

References


—. “Are Agricultural Economists Becoming Mechanists?” J. Farm Econ. 42(1960):1475–86.


King, Richard A. “Some Applications of Activity Analysis in Agricultural Economics.” J. Farm Econ. 35(1953):823–33.

Knight, Frank H. “Comment on Professor Parsons’ Article.” J. Farm Econ. 32(1950):112–22.


Macklin, Theodore. “Report of Committee on Farm Economic Investigational Work, the American Farm Economic Association.” J. Farm Econ. 3(1921):41–44.


Waugh, Frederick V. “Applicability of Recent Developments in Methodology to Agricultural Economics.” *J. Farm Econ.* 35(1953):692–706.
