Journal of Agricultural and Resource Economics, 20(1):82–95

A Protocol or a Set of Standards to Guide Agricultural Economics Research

David L. Debertin, E. Jane Luzar, and Orlando D. Chambers

This article examines some difficult decisions that agricultural economists must confront in doing research. Over many years, rules and standards have developed in agricultural economics that guide researchers, providing an underlying framework for research methods. This article deals with applying these seldom discussed guidelines to specific research situations confronted by agricultural economists. With this article, we hope to stimulate a dialogue among agricultural economists about the need for additional, appropriate methodological guidelines in agricultural economics research.

Key words: ethics, impartiality, methodology, replication, research standards, scientific method

Introduction

> It would be absurd to claim that this institutionalized mechanism [of scientific protocol] for sifting warranted beliefs has operated or is likely to operate in social inquiry as effectively as it has in the natural sciences. But it would be no less absurd to conclude that reliable knowledge of human affairs is unattainable.
>
> (Ernest Nagel, *The Structure of Science*, p. 490)

Just and Rausser depict agricultural economics as a "club" that must examine rules guiding the behavior of its members and adapt or become extinct (p. 1177). The acceptance of common values and beliefs defines a subculture or "club" and facilitates communication among its members (Boulding). To be ethical, members of a club must adhere to certain rules or standards that govern their conduct in accordance with the principles of the club. In this respect, agricultural economists are no different from members of any other club. However, agricultural economists vary in academic backgrounds, skills, and interests. Furthermore, not all agricultural economics researchers agree on a common protocol to guide research. Other disciplines, including some in the social sciences, have developed explicit rules to guide research and the behavior of researchers. For example, the American Sociological Association has a lengthy code of ethics that provides specific rules and standards for conducting research, and also includes items related to teaching and the treatment of employees.

The objective of this study is to examine some of the difficult decisions that individuals face while doing agricultural economics research and to suggest needed changes in our research protocol.[1] Agricultural economics organizations have never attempted to develop a code of ethics to guide research behavior to the same degree that the American Sociological

---

David L. Debertin is a professor of agricultural economics at the University of Kentucky; E. Jane Luzar is an associate professor of agricultural economics at Louisiana State University; and Orlando D. Chambers is a graduate research assistant at the University of Kentucky. The views in this article are those of the authors.

The authors express appreciation to Alan J. Randall, Eldon D. Smith, and a number of anonymous reviewers who have provided helpful comments on various drafts of this paper.

Association has, and they may not wish to now. However, we believe that it is important to engage in a profession-wide discussion of the rules and standards that guide the conduct of agricultural economics research. We hope to stimulate thinking about current rules and standards, and we shall suggest some practical changes in the way agricultural economists perform and report research.

We believe that in any discipline, including agricultural economics, established guidelines, rules, or standards facilitate communication among researchers and provide a fundamental foundation for the procedures employed in conducting research. These rules play a role in the decisions made by agricultural economists while conducting research and in reporting research results. Since these rules are not part of an explicit code of ethics for agricultural economics, many agricultural economists may not be fully aware of their function and importance. These rules provide the foundation upon which much of the behavior of individual researchers ultimately is based. The credibility of an individual researcher is based upon the adherence to guidelines and procedures for obtaining and assessing evidence accepted and used by the discipline[2] as a whole.

The issues we discuss here are not new and have been addressed in some manner by other authors (Castle 1972; Randall 1974; Barry; Breimyer; Tomek). But these issues need additional consideration and discussion among agricultural economists. The following statements outline our basic thesis.

1. Although agricultural economists may not be able to agree upon as comprehensive and detailed a code of ethics as that of the American Sociological Association, we believe that a written statement should be developed outlining the generally accepted guidelines and research standards under which all agricultural economics researchers should operate.
2. There is a need for an additional journal or section of a journal in agricultural economics that focuses on research which seeks to confirm the results obtained from previous empirical research. Too often, empirical studies in agricultural economics attempting to confirm the work of previous authors are rejected simply because they offer nothing "new." However, Tomek (p. 6) notes that "attempts to confirm prior work provides a depth of understanding that is otherwise unattainable."
3. New computer technologies, including the ability to rapidly transfer large programs and data sets from one institution to another, provide important new opportunities for accessing data and programs used by other researchers. This will make it possible for researchers interested in what Tomek calls confirmation-based research to proceed with greater ease. While some associations and journals, including the Western Agricultural Economics Association, have in the past made it possible for researchers to obtain the data used to conduct research, it should now also be possible to provide other researchers with the author's computer programs, such as simulation models.

[1]The dictionary (*Webster*, definition 4) defines protocol as "the plan for scientific experiment or treatment." As used in this paper, research protocol is a set of rules that guide how the specific research technique is applied or how particular research results are interpreted and evaluated, not the choice of the particular quantitative technique or empirical method to be used.

[2]Many would not consider agricultural economics to be a single discipline but rather a combination of many disciplines. Certainly there are differences in the criteria, peers, procedures, and kind of knowledge generated between multidisciplinary and disciplinary work. Under definitions such as those provided by Johnson, most of the research done on agricultural economics would not be termed disciplinary. However, the term discipline is still used here to describe research done within agricultural economics.

The Code of Ethics of the American Sociological Association is a lengthy document that includes material not strongly linked to research methodology, such as dealing with other professionals and with students, confidentiality of survey data, the treatment of human subjects in research, obligations of journal editors, and even employment practices. However, some of the items in this document are concerned with issues in research methodology and might be adapted to agricultural economics with only slight modification. Examples we might use with slight modification as taken directly from the document (p. 2) include the following:

1. In presenting their work, sociologists are obligated to report their findings fully and should not misrepresent the findings of their research. When work is presented, they are obligated to report their findings fully and without omission of significant data. To the best of their ability, sociologists should also disclose details of their theories, methods, and research designs that might bear upon interpretations of research findings.
2. Consistent with the spirit of full disclosure of method and analysis, sociologists, after they have completed their own analyses, should cooperate in efforts to make raw data and other pertinent documentation collected and prepared at public expense available to other social scientists, at reasonable costs . . . .
3. Sociologists must not accept grants, contracts or research assignments that appear likely to require violation of the principles enunciated in this Code . . . .
4. Sociologists should take particular care to state all significant qualifications on the findings and interpretation of their research.

We believe that agricultural economists should make an effort to develop a written document that outlines widely accepted rules. However, because agricultural economists have widely varying backgrounds and interests, and as a result, their research goals are also diverse, reaching a consensus on a written statement following a format such as this could prove difficult.

Research Protocol and Institutions

Consider two agricultural economists. The first works as a policy analyst and seeks to shape local, state, or federal public policy toward agriculture and rural America. The second is an academic researcher who seeks to develop and advance economic theory applicable to agriculture. These two agricultural economists may have very different goals and interests, but the same written statement would need to apply to both individuals as they seek to conduct and use research.

Faculty in the so-called "wet-lab" sciences, including the biological scientists, who are the majority of faculty in most colleges of agriculture, may have it somewhat easier than agricultural economists do. Unlike the social sciences, the biological sciences place great emphasis on reproducing exactly the work of other scientists in an effort to confirm or disconfirm past research results. This search for "truth" about reality—the essence of most biological and physical sciences—provides the foundation for research conduct.

In the non-social sciences, such research usually involves setting up a laboratory or field experiment in an effort to make another attempt to collect similar data. In the social sciences, however, comparatively few studies involve the collection of primary data sets, and most rely on secondary data series not collected by the researchers themselves. Ideally, two agricultural economics researchers that employ the same data set and the same quantitative technique should be able to obtain exactly the same results. In reality, this may not always be the case.

Differences in results obtained by two different agricultural economics researchers who are attempting to apply the same model and quantitative technique to the same data set could

include (*a*) unintended errors made by one or both of the researchers in implementing the quantitative technique (Dewald, Thursby, and Anderson, p. 587); (*b*) data entry or other errors in the data files themselves; (*c*) differences in secondary data sets as a result of updates and revisions by the collecting agency; and (*d*) differences in how calculations are performed within different statistical packages implementing the same quantitative technique. In the University of Kentucky agricultural economics department, instances have already occurred in which the widely publicized floating point error in the Pentium® chip led to noticeably different results in a spreadsheet program when results were compared with calculations done on a computer without that particular chip error. These kinds of problems are analogous to the errors and contamination problems that frequently occur in laboratory experiments. Without attempts by other researchers to exactly replicate a study, these kinds of errors may frequently go undetected.

Controversy still exists when scientists argue in favor of a particular explanation for an observed phenomenon, but through efforts conducted by other scientists, advocates of explanations inconsistent with the "truth" are eventually discovered and are ultimately discarded. Testing hypotheses in the physical and biological sciences usually requires laboratory experiments conducted such that another researcher doing the same test should be able to obtain similar results. If the goal of science is explanation, then such research authenticates the analysis and confirms its validity. The controversy surrounding the inability to confirm results from the Utah "cold fusion" research provides insight into how the process normally works in the physical sciences. If a laboratory experiment cannot confirm a result, then the original research is appropriately questioned.

Some biological research is not easily replicated. Instances where biological scientists have deliberately falsified results of laboratory experiments have made newspaper headlines, but only after other scientists made confirmation attempts. Unlike our "wet-lab" counterparts, social scientists seldom, if ever, discard a theory, no matter how many times it fails in an empirical setting. Indeed, it is often argued that theories in economics are never tested in an empirical setting: it is merely the *applicability* of a particular theory to a specific empirical problem that is being tested.

Most social science data are very different from that collected through controlled experiments. While the biological and physical sciences frequently conduct laboratory or field experiments to obtain data, agricultural economists (and other social scientists) frequently use data they did not collect and make simplifying assumptions that allow complex problems to be analyzed. Seldom is the social scientist explicitly concerned with the issue of whether or not the exact same results can be obtained by other researchers employing the same data set and technique.

Popper's basic approach of scientific inquiry based upon falsification through critical processes is thought by many agricultural economists to be the basis for research within agricultural economics. Popper's approach applies fairly well to most biological science research conducted within agricultural experiment stations. The difficulty in applying Popper's views to agricultural economics is that he dismisses many of the problems encountered by agricultural economists and other social scientists as not science, that is, inappropriate for scientific inquiry (p. 158). Popper likes hypothesis testing under laboratory conditions where the collection of the data is controlled by the researcher, not hypothesis testing employing secondary data outside of the researcher's control.

The misapplication and inadequacy of the positivism advocated by Popper in social science research have increased the appeal of "alternatives to Popper" in the philosophy of science and methodology of economics, leading to books on methodology for economists such as those by Blaug, Caldwell, and Johnson. Also, a new book by Gebremedhin and

Tweeten focuses on research method issues in the social sciences. There is no consensus among these writers on the "right" method for approaching economic research. Many methodology writers now advocate an eclectic approach toward research in the social sciences embodying various styles and approaches along with a less rigid adherence to falsification principles advocated by Popper (McCloskey; Caldwell; Hausman).

The eclectic approach to research methodology as advocated by these recent authors calls for an assessment of conduct and reporting within agricultural economics research. Does the eclectic approach suggest that there are no longer any specific rules to be followed, and that any methodological approach for social science research is warranted? This would be a form of methodological anarchy as advocated by Feyerabend. Most agricultural economists would probably not yet subscribe to this view, nor perhaps should they! Empirical analyses that use sophisticated leading-edge quantitative techniques are becoming increasingly prevalent in agricultural economics research, to the exclusion of other approaches (Debertin and Pagoulatos). The application of these sophisticated tools may make it appear, at least to a non-economist, that somehow agricultural economics research is being transformed into something more like a laboratory science with ever greater emphasis on numerical findings. Agricultural economists know better than that. No matter the sophistication of the quantitative method, a social science cannot be transformed into a hard science.

Of course, some researchers might argue that application of these increasingly sophisticated techniques may ultimately make economic research more testable and therefore more applicable to Popperian falsification. The intent may be to prove a hypothesis through an empirical test, but much of the empirical analysis in agricultural economics is actually a demonstration that the novel quantitative technique works with a real data set rather than a serious attempt to falsify testable predictions. Blaug argues that much empirical economic research "is like playing tennis with the net down: instead of attempting to refute testable predictions, modern economists all too frequently are satisfied to demonstrate that the real world conforms to their predictions, thus replacing falsification, which is difficult, with verification, which is easy" (p. 256).

Research using mathematical programming and computer simulation models that do not involve formal hypothesis testing (as econometric research does) raises additional difficult questions regarding the appropriateness of applying Popper's falsification principles. Johnson and Rausser note that the common use of paradigms as maintained hypotheses orients research towards propositions which may not, in the Popperian sense, be testable (p. 169). Many social scientists deny that testing a research model which requires the manipulation of quantitative data is even possible and argue that economists should not confine themselves to such an approach. Others believe this approach has limited creativity and has led to mathematical tinkering rather than addressing new problems (Just and Rausser, p. 1179; Barry, p. 2).

Standards for Behavior

Although a few agricultural economists have called for less dominance of mathematical techniques in agricultural economics research, the incentive for publishing research with novel quantitative techniques is high. In 1990, over 92% of the articles published in our *American Journal of Agricultural Economics* used some type of quantitative method compared to just 52% in 1965 (Debertin and Pagoulatos, p. 4). Are the methodological foundations of these empirical articles based on Popperian falsification, or is any thought at all given to the methodological foundations of applied agricultural economics research? This

question may be especially relevant for graduate students and other agricultural economists whose methodological approach to research is in its formative stage.

Castle (1989) points out that the limited time in a graduate program may prohibit the learning of alternative theories, lead to repetitive use of the same theory, and result in a narrow-minded view of research methodology (p. 5). A research environment in which increasingly sophisticated mathematical techniques and applications are employed, methodological conditions are changing rapidly, and journal editors and reviewers increasingly desire articles that employ new approaches and data, supports the need for a set of agreed upon guidelines to direct and enhance the credibility of agricultural economics research. As Hausman suggests, "the normative role of methodology is unavoidable; whether methodological rules are garnered from imitation, methodological asides, or systematic methodological treatises, there is no doing economics without some standards or norms. Furthermore, if economics is to make any rational claim to guide policy, these standards or norms cannot be arbitrary" (p. 123). There must be limits to the laissez-faire approach to research.

Suppose, for example, that an agricultural economist developed a new and promising theoretical model representing economic phenomena of interest, but needed empirical evidence to support the formulation. The data set needed to do this may exist. However, upon initial estimation of model parameters, the researcher discovers that the data do not provide parameter estimates consistent with a thorough and rigorously developed model specification. Further analysis traces the problem to one or two outliers in the data set, and removal of these outliers provides parameter estimates that, while perhaps not perfect, are largely consistent with the theoretical arguments.

Each alternative approach for dealing with this problem involves decisions by the individual researcher, but each alternative also has implications for the system that guides research. For example, an attempt might be made to publish the theoretical model without the empirical evidence, particularly if the research is very well done. But without the empirical evidence, the probability of acceptance by a journal is reduced. The empirical results could be presented without alteration of the original data set, and the researcher could show that the empirical results are inconsistent with the theoretical development. Again, the probability of acceptance may be reduced, given the incompatibility between the theoretical development and the empirical results. Alternatively, the agricultural economist might simply discard the outliers and publish only the results from a truncated data set, without discussing the research procedure used. Or the researcher might indicate the research procedure used, but attempt to publish only the results from the truncated data set. Another possibility is to show two sets of equations, explaining fully the impact of the outliers on the parameter estimates.

This illustration is used not because there is a single correct alternative that is "proper" scientific behavior by the individual. Rather, the case illustrates that it is sometimes difficult for the researcher to make the right decision even when confronting ordinary research problems. Furthermore, the "proper" decision by the researcher in borderline cases can be even more difficult to identify. The dividing line between acceptable versus unacceptable research practices is not always clear. For many agricultural economists, an adherence to scientific method might continue even when at variance with professional self-interest—that is, getting the journal article published.

Most agricultural economists attempt to accurately report the findings of their research because they believe that accurate reporting is proper. Further, if the researcher were dishonest and reported results that are improved by changing signs on coefficients or increasing a *t*-ratio, there is always the (admittedly small) danger that the changes will be discovered either within the review process or after publication. Most important, however,

is that the confidence of the researcher in the research findings is rooted in a belief that the findings *could* be reproduced by other researchers in the discipline, even though it is unlikely that another researcher will ever attempt to do so.

Several examples further illustrate situations in which such guidelines should play a critical role in guiding applied research in the social sciences. First, social scientists often report only the best empirical results. This applies not only to agricultural economics analyses using statistics, but also to studies which make use of mathematical programming, simulation, or other quantitative techniques.

Social scientists, including agricultural economists, often rework theoretical models based on initial empirical results to ensure that theoretical and empirical models are consistent. Specifications of equations may be adjusted based on initial empirical evidence, and nonsignificant variables may be eliminated from initial model specifications. Missing values may be inserted into a data series by interpolating between known values or by inserting the means of the other observations already in the data set.

Some of these examples may appear simply to be normal research procedure within many social sciences. For example, choosing the best of the available research evidence for reporting in a journal article is normal behavior for researchers both in and out of the social sciences. The burden of providing disconfirming evidence is placed on peers. Few applied researchers have not made at least small changes in model specifications based on initial empirical evidence contained in the data. Certainly the data provide a basis for changing the hypothesis, but theoretical arguments are sometimes formalized after initial relationships have been identified within the data, thus ensuring that the empirical evidence will not conflict with the theoretical arguments.

Some of the common decisions a researcher makes may deviate from standard research rules depending on the specifics of the case involved. For example, removal of outliers from data sets might be supportable if the researcher substantiates that the data for these observations are inaccurate, but offending data and a rationale for rejection could be presented. When this strategy is used as a means of coloring or modifying the results, there is concern. A social scientist may have little choice but to fill in missing observations if research is to proceed. However, failure to report that the procedure was used and its possible implications on the outcome is a serious issue.

The problem of specification bias resulting from the use of stepwise regression models has been documented elsewhere (Freund and Debertin). Excluding variables that do not behave as anticipated can lead to serious specification-error problems. Research articles in which such techniques are used to develop initial model specifications may sometimes be identifiable, for they may lack a rigorous derivation of the theoretical model that forms the basis for the specification. However, researchers are becoming much more adept at disguising that a conceptual section was written last. In addition, encouragement by both reviewers and editors to present equations with significant t-ratios can lead a researcher to use such approaches even when they conflict with best judgment.

Furthermore, if a researcher were dishonest, a coefficient with a different sign or a smaller standard error than was estimated could usually be reported with little chance of it ever being found. Deliberate dishonesty is somewhat different from the other cases and represents a violation of personal ethics and scientific research ethics. While it is difficult to determine the frequency of such abuses, behavior similar to this by agricultural economics researchers is probably not widespread.

Tangney has done research on this issue. Although her study involved behavioral and social scientists, respondents did not include agricultural economists. Tangney (1987a, b, c) conducted a survey designed to assess scientists' attitudes toward and perception of scientific

fraud. The survey was sent to 1,100 scientists at a major university. Of the 245 respondents, 41% were behavioral and social scientists, 40% biological scientists, and 19% physical scientists. The survey response indicated that 32% of the respondents suspected a colleague in their field of some form of scientific fraud, but most took no action against the suspected colleague. Tangney is quick to point out that measuring the perception of scientific fraud is not the same thing as measuring the actual prevalence of scientific fraud.

The results of Tangney's survey suggest, however, that scientific fraud is a problem worthy of concern and attention among scientists both in and out of the social sciences. Other than replication by another researcher, there is little to prevent deliberate dishonesty from occurring. In this instance, the governing regulations of the university are the protocol, and proof of deliberate dishonesty is grounds for dismissal at many universities.

Quality Control through Confirmation

In agricultural economics, particular emphasis needs to be placed on maintaining and upgrading the quality-control system that is supposed to discourage scientific fraud. For example, within agricultural economics there has usually been only a limited emphasis on confirmation as a path to publication, in comparison with the emphasis placed on the publishing of studies that confirm earlier results within the biological and physical sciences and even within some of the other social sciences. Tomek makes an excellent case in support of the need for agricultural economists to devote additional effort toward publication of confirmation research. The problem is that much economic research is heavily geared toward the publication of new findings. The economist must show that the research is innovative, for creativity is one mark of a scholar. If the research is a new and more promising approach to a problem, it will not use the same data or quantitative technique. If the same quantitative technique and a similar data set are used, this may be interpreted as a lack of trust between researchers. Although creativity carries risk, agricultural economics research which represents a new and innovative approach to a problem, particularly an approach that takes advantage of a quantitative technique that has not previously been used, is generally well received and is sometimes published even with substantive technical flaws in the empirical component.

When an agricultural economist has a serious disagreement with the findings of an earlier study, an attempt might be made to reproduce earlier results—but such papers generally are not publishable if they simply repeat the work of peers and reach similar conclusions, even if the data set has been revised or contains additional observations. In the biological and physical sciences, the ability to reproduce findings provides additional evidence and will generally warrant publication. Thus, the burden of proof in these sciences is on the journal editors and peer reviewers to find sufficient flaws in a study to warrant a negative publication decision. A colleague in the biological sciences argues that failure to publish an article within the biological sciences that meets minimum technical standards with regard to the conduct of the research constitutes a withholding of information, and most journal editors within the biological sciences would not want to do this.

Within agricultural economics, the publication decision by editors often rests heavily on whether or not the technique is novel. Within the social sciences, the burden is therefore on the author to prove that the research makes a significant contribution to the progress of the discipline. Social science journal editors and reviewers need to agree that findings are new enough and important enough to warrant a positive publication decision.

As a consequence, acceptance rates for journals in agricultural disciplines outside the social sciences are often much higher than within the social sciences (Lacy and Busch). The likely primary reason editors reject articles in the social sciences is that the technique or results are deemed insufficiently new or novel, not because of a basic flaw in the procedures or data used in conducting the research. In the biological and physical sciences, rejected articles likely consist primarily of those in which some basic flaw in the experimental procedure was discovered, not because the problem being studied had been studied by other researchers, or because the experimental procedure was similar to that used by other researchers.

In a brief symposium paper in 1955, Elmer Working, a pioneer in demand analysis, stated that "statistical studies of demand increased apace [prior to World War II] but relatively few were published" (p. 970). This limited publication was (in part) "because of unresolved doubts on the meaning and reliability of the results. The studies were never considered finished or suitable for publication by those who conducted them." Scientific perfectionism as described by Working can be incompatible with the six-year promotion rule present at many universities. The social sciences may now require more tangible evidence of scholarly progress than before. Because of this, the performance of the quality-control institutions that police the various social science disciplines is likely to be more severely tested.

Impartiality, the Protocol, and Favorable Research Results

One controversial view, currently particularly fashionable among some researchers in the biological and physical sciences, is that objectivity or research impartiality by a researcher is neither necessary nor sufficient for good research. This view maintains that science is not science only because researchers are objective individuals. Many excellent scientists have strong expectations regarding relationships. Further, the driving force behind experiments, both in and out of the social sciences, is a hunch or even stronger conviction by the researcher that certain relationships must hold.

This view also holds that scientists generally do not devise experiments in an effort to show that the null hypothesis is accepted. Rather, virtually all scientists (including biological, physical, *and* social scientists) select from their findings the best results for publication, and in so doing, expose only the best results to other scientists. This is the practice of agricultural economists, when the regression equation with the correct signs on all the parameter estimates, significant *t*-ratios, and a high coefficient of determination is the one finally chosen for inclusion in a journal article.

Robert Millikan, a noted physicist, often selected only the best results for publication. In the margins of his laboratory notebook, intended for the eyes of no one else, were comments. These margin notes included statements such as "this is a very good result, publish it," or in other instances, "this result is inconsistent with what should have happened. Examine the equipment and procedure that was used to obtain this result more carefully. Don't publish." (Goodstein).

Reexamination of Gregor Mendel's original genetic experiments by R.A. Fisher, a statistician, and Sewall Wright, a geneticist, using a chi-squared analysis, revealed that Mendel's results were in fact "too good to be true." Tempering charges that deliberate falsification had occurred at the monastery, Wright instead explained, "I am afraid that it must be concluded that Mendel made occasional subconscious errors in favor of expectations" (Hodges, Krech, and Crutchfield).[3] Mendel's "laws" (probabilities of inheritance of dominant and recessive traits) have guided genetic research for many years, and only recently

molecular biologists have learned that the rules governing the inheritance of many genetic traits are far more subtle and complicated than Mendel believed. One might question whether research progress in the biological sciences would have proceeded at a faster pace if Mendel had completely disclosed his findings, including the occasional inconsistent and disconfirming results, rather than reporting only those results that tended to entirely support his expected probabilities.

Most scientists are not unlike Mendel in their passion for their own theories. In much of science, it is the researcher's passion and persuasion, not objectivity, that is the real force pushing scientific research forward both in and out of the social sciences. Furthermore, experimental procedures are always more carefully examined in instances where the outcome is inconsistent with the researcher's expectations. Moreover, this lack of objectivity by the researcher, or for that matter, the more careful scrutiny of the experimental design and procedures (for agricultural economists, mathematical programming, simulation and econometric models, coefficients and data) when the results are inconsistent with expectations does not necessarily result in "bad" research. Social scientists are not the only researchers who are neither impartial nor objective. *No* good researcher is completely impartial and objective. It is this lack of impartiality and strong belief in one's own theories that drives researchers to solve problems.

According to those who advocate this position, what makes for good research is the researcher's fundamental belief that the results obtained can and will be substantiated by the work of other researchers. They argue that many excellent researchers publish only their best results, and in so doing, the researcher is not inherently involved in a violation of scientific rules and standards. Furthermore, they suggest that a researcher who publishes only the best results is not cheating, or acting at variance with accepted scientific procedures, if the researcher truly believes that these best results can be substantiated through careful effort by other researchers within the discipline. Among other researchers may be those whose values and, hence, expectations strongly differ. In other words, what separates a good researcher from one who is attempting to cheat is that the good researcher believes the findings can be confirmed, even by researchers who disagree with the results.

The ability to confirm results obtained by other scientists and the willingness by other scientists to do this are key elements for scientific progress within a discipline. If we were to apply this view to researchers in agricultural economics, we would conclude that having many agricultural economists who have strong values and lack objectivity is not what leads to "bad" research. What leads to bad research is a failure to recognize the contribution to scientific progress made by a researcher within agricultural economics who is able to confirm the results of a previous study.

Tangney's survey results would not be so disturbing if we knew that other researchers' evidence would eventually "weed out" studies (and eventually destroy reputations of researchers) with misreported findings either by mistake or fraud. If such a view were widely adopted by agricultural economists, research would be restructured such that far more effort would be devoted to attempts at confirming (or disconfirming) the work of our peers, along the lines that Tomek suggests.

---

[3]These comments regarding Mendel's reporting of research results are particularly illuminating in that the rules developed by Mendel that explain inheritance of traits are undergoing increasing scrutiny. Since the rigid rules governing inheritance from dominant and recessive genes are no longer thought to apply in all instances (Begley, pp. 77–78), perhaps some of the inconsistencies between anticipated and actual results that Gregor Mendel had observed and not reported because they "must have been due to laboratory error" were instances in which genetics was not entirely behaving according to the rules that Mendel developed and strongly believed.

The potential for abuse of scientific credibility exists when sponsoring research institutions maintain a set of values which, in part, ensures the ongoing existence of the institution itself. Public and professional perception of the dilemma resulting from the potentially compromising linkage between funding sources and research impartiality has created what Castle (1980) calls a great need for universities to enhance their credibility. Nonimpartiality in the hypothesis-testing process, undampened by a guiding set of institutionalized rules and standards, can create an atmosphere conducive to the manipulation and publication of self-serving research results. For example, if the applied social sciences have become enamored with the need for favorable research findings (evidence for the existence of a relationship), the normal review process will be biased in that direction.

Blaug argues that "all scientific hypotheses have philosophical, social, and even political undertones, which may prejudice scientists in evaluating the evidence for and against a particular hypothesis. Ideological biases and special pleading of all kinds are a universal feature of scientific work for which the only remedy is the public criticism of other scientists relying on the shared professional standards of the subject" (p. 153). Problems arise if a researcher must improve the results of a study to make it publishable, and in doing so, the researcher must violate the rules of scientific method.

A Call for Action

Agricultural economists need to discuss the issues in this paper. In the past, many of these issues have been treated as if they were topics that should not properly be raised in polite circles of social scientists. It is time to evaluate the research standards that guide agricultural economists and assess the effectiveness of our quality-control institutions, such as the refereeing process. For example, should we continue to place such heavy emphasis on innovation as the path to publication rather than research that seeks to confirm or refute past studies? If such research is to be valued (published), both researchers and referees will need to rethink their positions on what constitutes research progress in agricultural economics. This suggests the need to develop additional externally sanctioned guidelines for research specifically aimed at confirming or refuting previous research.

While many unstated but generally understood guidelines are commonly followed in agricultural economics, there are no specific standards for ethical research behavior by agricultural economists. The Code of Ethics of the American Sociological Association establishes some practical requirements for the ethical behavior of social scientists and provides a methodological foundation for doing social science research. We are fascinated that an entire discipline agreed to comply with a specific set of guidelines. Could agricultural economists ever agree upon the content of a written code that deals with issues in research methodology? A comparatively simple statement that focuses on a few important methodological issues that are frequently encountered in agricultural economics research might adequately serve our needs. Discussions among agricultural economists about the specific items to be included in such a statement may prove fruitful in highlighting important issues. Such discussions could create a new awareness of many of the issues raised in this article and stimulate thinking about the importance of research methodology in conducting agricultural economics research.

A few journals that publish research in economics have made an effort to confront some of the issues raised in this paper. For example, a special arrangement was instituted by the *Journal of Econometrics* and the *Review of Public Data Use*. These journals jointly

established a publication program that gives authors a preferential opportunity to publish complementary articles in the *Review* relating data used in applied journal articles. One purpose of this arrangement is to encourage a uniformly high quality of research methodology (Renfro). An approach such as this could be effectively used in other social science disciplines as well.

A complementary action in the applied areas involves the creation of a discipline-wide computerized information system through Internet with data and programs supporting research published in the major professional journals, and involving efforts of the editors of all the agricultural economics journals that publish applied research employing quantitative methods. The *Journal of Business and Economic Statistics* and the *Journal of Applied Econometrics* now place data on the Internet. Search algorithms such as Gopher and simplified procedures for transfer of data from host computers to the researcher via File Transfer Protocol (FTP) make it technologically feasible for researchers to quickly gain access to virtually any public-domain computer program or data set.

Thus, the Internet might provide ready access not only to data used in econometric investigations, but also to the software constructed by other researchers that was used for estimating model parameters, as well as copies of researcher-constructed simulation and mathematical programming models. Ready availability of the data and programs through this computerized information network could form the foundation for a system of checks and balances now lacking in agricultural economics. The additional cost of creating and maintaining such a database would, of course, be a concern.

It is our belief that the difficulty in simply transferring computer programs and data from one institution to another has discouraged much of the research called for in this study. Furthermore, lack of ready access to the models used by other researchers has made it difficult for agricultural economics scientists to build and improve upon the published work of other scientists to the extent that this occurs in many other disciplines. What a computerized information network such as this would permit is easy access to *any* of the quantitative models and data published in an agricultural economics journal. Benefits to the profession in terms of increased scientific credibility could be great. The fact that other researchers have access to a model means that they might attempt to experiment with it, further improve its specification, or correct other "obvious" flaws. If serious errors inadvertently occurred, these might eventually be uncovered as researchers attempt to reestimate the model and improve upon it. Furthermore, knowing that others in the profession would have ready access to data and programs should further deter researchers from fabricating or doctoring results.

Besides the need for accessing data and programs, King has suggested that the actual documentation of the data in the reporting system is incomplete and often of an inappropriate form. A profession-wide effort within agricultural economics to standardize proper documentation besides making data and programs readily available through a computerized information network would lead to wider acceptance of results and greater credibility (p. 846).

The inclusion of data sets has long been essential in economic research. Frisch, in the first issue of *Econometrica*, indicated that "in statistical and other numerical work presented in *Econometrica* the original raw data will, as a rule, be published, unless their volume is excessive. This is important in order to stimulate criticism, control, and further studies" (p. 3). Dewald, Thursby, and Anderson found that requiring authors to submit their programs and data with manuscripts significantly reduced the frequency and magnitude of errors by revealing ambiguities, errors, and oversights which otherwise would be undetected (p. 589).

The *Journal of Agricultural and Resource Economics* requires authors to fully document data, as well as model specifications and estimation procedures. In addition, authors must

provide data, at cost, for five years. In some cases, this has presented conflicts because data are considered confidential. Copyrighted data or expensive scanner data are examples that raise difficult questions about confidentiality versus disclosure. Such issues lend additional support for specific guidelines.

Another action that focuses on the reporting of econometric results has been proposed by Cooley and Leroy. Because economic theory generates incompletely specified statistical tests, they, too, argue that selective reporting and advocacy of certain values are common. They offer in place of the accepted reporting style an alternative format developed by Chamberlain and Leamer which clearly identifies explanatory variables in a regression as "focus variables" and "doubtful variables." Extreme values of the focus variables would be reported over a defined region of the parameter space, which includes coefficients of doubtful and focus variables. A more rigorously specified reporting procedure (such as that proposed by Chamberlain and Leamer) would limit opportunities for selective reporting and perhaps more clearly identify research advocacy.

Creation of outlets for research aimed at confirming (or disconfirming) results of previous studies, formation of a program and data bank, and modifications in reporting styles involve major revisions in the reporting system. The social sciences must place greater emphasis on this reporting system as an institutionalized form of checks and balances. With improvements in reporting requirements, much of the needed information could be made available to other researchers who wish to attempt to reproduce the findings of a study. The ability of a discipline to weed out questionable research is aided by rigorous reporting requirements.

Editors of agricultural economics journals need to be keenly aware of the problems that arise when cliques of authors, reviewers, and editorial boards favor rules that are consistent with the underlying philosophic orientations of members of the clique. Research that could be important to the progress of the discipline may apply different rules and standards. This research might be reviewed unfavorably by members of the group, since it does not conform to the group's "rules." As a result, important results may never be published.

While improvements in the institutions that police the social science disciplines are a necessary step, they are not sufficient to completely prevent abuses by some. Opportunities exist for violations that might only be detected by the individual conducting the research. Great care in conducting studies is required to ensure that results presented to the profession represent good science in that quantitative methods were appropriately applied and results were accurately reported and carefully interpreted. The relationships between research methodology and contemporary quantitative methods should be a formal component of study within the social science disciplines. As Tomek points out, "changes in current research protocols will not be easy" (p. 6). It is time to recognize that changes are needed and can be accomplished.

[*Received February 1990; final version received March 1995.*]

References

Barry, P. J. "Publishing in Professional Journals." *J. Agr. Econ. Res.* 41(1989):2–3.

Begley, S. "A New Genetic Code." *Newsweek* (2 November 1992):77–78.

Blaug, M. *The Methodology of Economics or How Economists Explain.* Cambridge: Cambridge University Press, 1980.

Boulding, K. E. "Economics as a Moral Science." *Amer. Econ. Rev.* 59(1969):1–12.

Breimyer, H. F. "Scientific Principle and Practice in Agricultural Economics: An Historical Review." *Amer. J. Agr. Econ.* 73(1991):243–54.

Caldwell, B. *Beyond Positivism: Economic Methodology in the Twentieth Century*. Boston: George Allen and Unwin, 1982.

Castle, E. N. "On Scientific Objectivity." *Amer. J. Agr. Econ.* 50(1972):557–69.

———. "Agricultural Education and Research: Academic Crown Jewels or Country Cousins?" Kellogg Foundation Lecture to the National Association of State Universities and Land Grant Colleges. Atlanta GA, 8 November 1980.

———. "Economic Theory in Agricultural Economics Research." *J. Agr. Econ. Res.* 3(1989):3–7.

Chamberlain, G., and E. Leamer. "Matrix Weighted Averages and Posterior Bounds." *J. Royal Statis. Soc., Series B.* 1(1976):78–84.

Code of Ethics. American Sociological Association, N. Street NW, Washington DC, 19 August 1989.

Cooley, T. F., and S. F. Leroy. "Identification and Estimation of Money Demand." *Amer. Econ. Rev.* 71(1981):825-44.

Debertin, D. L., and A. Pagoulatos. "Research in Agricultural Economics 1919–90: Seventy-two Years of Change." *Rev. Agr. Econ.* 14(1992):1–22.

Dewald, W. G., J. G. Thursby, and R. G. Anderson. "Replication in Empirical Economics: The Journal of Money, Credit and Banking Project." *Amer. Econ. Rev.* 76(1986):587–603.

Feyerabend, P. *Against Method: Outline of an Anarchistic Theory of Knowledge*. London: Verso Books, 1975.

Freund, R. J. and D. L. Debertin. "Variable Selection and Statistical Significance: A Sampling Experiment." *Amer. J. Agr. Econ.* 47(1975):721–22.

Frisch, R. "Editorial." *Econometrica* 1(1933):1–4.

Gebremedhin, T. G., and L. G. Tweeten. *Research Methods and Communication in the Social Sciences*. Westport CT: Praeger, 1994.

Goodstein, D. L. *The Mechanical Universe*. Videocassette. California Institute of Technology, Pasadena CA, 1984.

Hausman, D. M. "Economic Methodology in a Nutshell." *J. Econ. Perspectives* 3(1989):115–27.

Hodges, J. L., D. Krech, and R. Crutchfield. *Statlab: An Introduction to Empirical Statistics*. New York: McGraw Hill, 1975.

Johnson, G. L. *Research Methodology for Economists*. New York: Macmillan, 1986.

Johnson, S. R., and G. C. Rausser. "System Analysis and Simulation: A Survey of Applications in Agricultural and Resource Economics." In *A Survey of Agricultural Economics Literature*, Vol. 2, G. G. Judge et al., pp. 157-301. Minneapolis: University of Minnesota Press, 1977.

Just, R. E., and G. C. Rausser. "An Assessment of the Agricultural Economics Profession." *Amer. J. Agr. Econ.* 71(1989):1177–190.

King, R. A. "Choices and Consequences." *Amer. J. Agr. Econ.* 61(1979):838–39.

Lacy, W. B., and L. Busch. "Guardians of Science: Journals and Journal Editors in the Agricultural Social Sciences." *Rural Sociology* 47(1982):429–48.

McCloskey, D. "The Rhetoric of Economics." *J. Econ. Lit.* 21(1983):481–517.

Nagel, E. J. *The Structure of Science*, chapter 13. New York: Harcourt, Brace and World, Inc.,1961.

Popper, K. "Philosophy of Science: A Personal Report." In Mace, C. S., *British Philosophy at Mid-Century*. ed., C. S. Mace, pp. 155–87. London: George Allen and Unwin, 1957.

Randall, A. "Methodology, Ideology and the Economics of Policy: Why Resource Economists Disagree." *Amer. J. Agr. Econ.* 67(1985):1022–29.

———. "Information, Power and Academic Responsibility." *Amer. J. Agr. Econ.* 56(1974):227–34.

Renfro, C. "Production, Distribution and Use of Data: An Editorial." *Rev. Public Data Use* 8(1980):295–306.

Tangney, J. P. "Factors Inhibiting Self Correction in Science." Unpub. paper presented at the 95th Annual Convention of the American Psychological Association, New York NY, August 1987a.

———. "Fraud Will Out—Or Will It." *New Scientist* 155 No. 1572 (1987b):62–63.

———. Personal correspondence, 1 October 1987c.

Tomek, W. G., "Confirmation and Replication in Empirical Econometrics: A Step Toward Improved Scholarship." *Amer. J. Agr. Econ.* 75(1993):6–14.

Webster's II New Riverside University Dictionary, s.v. "protocol."

Working, E. J. "How Much Progress Has Been Made in the Study of the Demand for Farm Products." *J. Farm Econ.* 37(1955):968–72.