Reflections on Relevance of Professional Journals

Lindon J. Robison and Dale Colyer*

Abstract

It appears the major private benefit from peer reviewed journals such as the *Review of Agricultural Economics* (*RAE*) is certification. To maintain public support for our journals, increased efforts are needed to demonstrate the social benefits from peer reviewed publications. Research cost considerations have led agricultural economists to emphasize applied disciplinary work using secondary data and to ignore the important work of careful data collection and reporting. Moreover, pressures to publish have led to more isolated research efforts ignoring other disciplines. Recommendations to improve the relevance of journal publications include more active efforts by journal editors to make applied journals such as *RAE* more accessible to the public.

Key Words: certification, confirmation studies, private goods, public goods, relevance

Introduction

This is intended to be a reflective look at our experience editing the *Review of Agricultural Economics* (*RAE*) for the 1991-93 period. Based on the number of manuscripts submitted and journal subscriptions, 1991-93 was a productive period for *RAE*. Manuscript submissions increased from 48 in 1990 to 98 and 90 in 1991 and 1992, respectively. Subscriptions increased from 541 in 1991 to 602 and 581 in 1992 and 1993. Moderating the interest in subscribing to *RAE* in 1993 may have been a subscription rate increase of from $15 to $20 that occurred in 1992.

Two significant changes may have influenced the increased number of articles submitted. First, the name of the journal was changed to reflect its national and international orientation; the *North Central Journal of Agricultural Economics* (*NCJAE*) became the *Review of Agricultural Economics*. The name change reflected our view that the interests of our authors and subscribers extended beyond the North Central region of the United States. In retrospect, this appeal to a broader set of authors and subscribers was justified. During the 1991-93 period, 58 percent of the first authors who published articles in *RAE* were **not** associated with the 12 North Central departments that sponsored *NCJAE*. Table 1 describes the institutions represented by first authors who published in *RAE* for the 1991-93 period.

In addition to changing its name, *RAE* altered its format and included in each issue an invited paper. Having the opportunity to invite a paper for each issue gave us modest control over the journal's content and the opportunity to identify types of articles we hoped would be submitted more often. The invited papers were of the type we believed our subscribers wanted to find in their journal but which few authors submit. Consistent with Castle's recommendation, the

*The authors are professors of agricultural economics at Michigan State University and West Virginia University, respectively. They thank James Bonnen, Lester V. Manderscheid, Robert Myers, and Virgil Norton for helpful comments on an earlier draft of this paper.

J. Agr. and Applied Econ. 26 (1), July, 1994: 19-34

Table 1. Institutions of First Authors Who Published 90 Manuscripts in *RAE* During 1991-1993

University	Number of Articles	University	Number of Articles
Federal Government	9	Brigham Young U.	1
Kansas State U.	8	Louisiana State U.	1
Purdue U.	5	Nicholls State U.	1
State Government	4	N. Dakota State U.	1
Texas A&M U.	4	N.E. Missouri State	1
Iowa State U.	4	Oklahoma State U.	1
Ohio State U.	4	Oregon State U.	1
Cornell U.	3	Pennsylvania State U.	1
U. of Georgia	3	Santa Clara U.	1
U. of Illinois	3	Tuskegee Inst.	1
U. of Kentucky	3	U. of Arkansas	1
U. of Minnesota	3	U. of Arizona	1
U. of Wisconsin	3	U. of Exeter	1
Auburn U.	2	U. of Florida	1
Hebrew U.	2	U. of Haifa-Israel	1
Montana State U.	2	U. of Hawaii	1
N. Carolina State U.	2	U. of Saskatchewan	1
U. of Missouri	2	Utah State U.	1
U. of Nebraska	2	Virginia Poly. Inst.	1
U. of Wyoming	2	W. Virginia U.	1

invited papers were integrative, literature reviews, or forward looking.

RAE income was generated from page charges and subscription fees. Page charges paid by the institutions supporting the authors accounted for roughly 71 percent of *RAE*'s total income during the 1991-93 period. However, the cost of publishing *RAE* exceeded its income and required subsidies from its host institutions, Michigan State University and West Virginia University. If the private benefits of articles published in *RAE* are reflected by its income, then authors and their supporting institutions were the major beneficiary of the publication enterprise.

At first glance it seems a strange enterprise that charges suppliers to supply their product. This manner of funding leads to the question: why should the public pay for a product when the suppliers receive the major private benefits? One justification for public support of journals such as *RAE* is that the page charge system is an efficient way of organizing public support for publications that produce significant social benefits. This argument has some merit since public funds supporting departments and faculty are the ultimate source of support. Yet, we have little evidence demonstrating the social benefit from our publication efforts.[1] In addition, if the willingness of the public to support our journals through page charges is evidence of the public good nature of the product, then one might argue that the public believes the social benefit derived from such publications is decreasing.

Our observations about journal funding lead us to reflect on the purpose of professional journals such as *RAE* and ask if new directions are needed. As part of our reflections, we ask and try to answer several questions such as: What is *RAE*'s purpose? Who publishes and why?

Who reads *RAE*? What factors influence the type of articles published? Are theory and applied work properly balanced? And are new directions needed?

Incentives for Publishing

Consider the important question: Why do authors submit articles to peer reviewed journals such as *RAE*? Authors submit articles because it is in their self-interest to do so. Publications serve as evidence of an author's productivity and ability to do scholarly work. Thus, publications are used by administrators to award raises and argue for promotion of their faculty. Evidence that publications produce financial rewards for authors was provided by Broder and Ziemer. They found that an additional *AJAE* publication adds $184 to the average agricultural economist's annual salary (p. 303). In 1992 dollars, that increase is translated to $356. Moreover, the present value of the increase over 20 years discounted at 7 percent equals $3,767.

The peer review process is also the profession's way of certifying each other's work. Because of the importance of this certification, we infer those without it are the most anxious to publish. Supporting this view are survey results of VanTassell, McLemore, and Roberts (VMR). They found that assistant and associate professors felt significantly more pressure to publish than did full professors.

Publications in peer reviewed journals can be used to certify the relevance of departments in much the same way they certify individual faculty. Peer reviewed publications are evidence that faculty members are current in their knowledge of tools and issues and are involved in a professional dialogue.

Faculty publishing in peer reviewed journals are also advertising their department to those outside the department. Students often select graduate schools based on their recognition of the faculty employed at the schools. This recognition is often obtained by reading the faculty's published work or having the faculty member's work referred to in textbooks or by the students' professors. A department's ability to attract high quality faculty also depends on the professional reputation of members of the department, achieved in part through the publication record of members of the department. Professional awards that call attention to departments and members of departments are often selected from peer reviewed articles. Contractors and grant providers when faced with applied problems in which conception and measurement gaps exist may use journal publication to determine the qualifications of those with whom they contract to resolve the gaps. Finally, published articles are likely to be studied in graduate schools and influence the kinds of research produced in the future by new faculty. Thus, publications in professional journals likely influence the direction and research methods used in the future.

The benefits of peer reviewed publications for individual faculty and their supporting departments are clear. Summarizing these benefits, Just and Rausser quote one unidentified respondent to their survey as saying: "After over thirty years of observing the academic process it appears that most scholarly societies have become agents to establish professional credentials for tenure, promotion or a job offer" (p. 1189).

The question more frequently being asked these days is not how do authors benefit from peer reviewed publications but: How does the public who support our efforts benefit? Most authors of peer reviewed articles would argue that private incentives to publish (such as certification) are necessary to enhance productivity; but, the ultimate goal of the research process is to generate significant positive social returns. Yet, we have little evidence that supports the view that our publications produce significant social returns. In fact, none of the articles we published attempted to document how the public has benefitted from articles published in *RAE* or similar journals. It appears likely that we will continue to depend on public funding for our journals. For the public to continue this support, more efforts will be required to demonstrate research and journal publication activities have social benefits that outweigh social costs.

Who Publishes?

Consistent with the hypothesis that one of the journal's private goods is certification, 48 percent of *RAE*'s first authors during 1991-93 were assistant professors (32 percent) or research assistants (16 percent). Assistant professors and graduate assistants appeared as first authors in the *American Journal of Agricultural Economics* (*AJAE*) only 34 percent of the time.[2] Associate and Full Professors appeared as first authors 39 percent of the time in *RAE* and 47 percent of the time in *AJAE* (see panel A of table 2). Assistant Professors and Research Assistants were listed as first authors at a higher rate than their membership in AAEA would indicate if all ranks published equally. However, the most under-represented group were the "Others" who were first authors for only 19 percent of the articles, but who are 45 percent of the membership.[3]

Assistant professors and research assistants were listed 30 percent of the time as second authors in *RAE* and only 22 percent of the time in *AJAE*. Meanwhile, associate and full professors were listed as second authors 59 percent of the time in *RAE* and 53 percent in *AJAE* (see panel B of table 2).

We also observed that those who publish in *RAE* represent a very narrow group. All first authors were agricultural economists or economists. In addition, nearly all held research and teaching appointments in Land Grant Universities. When Leontief (1971) congratulated our profession for its proper balance between theory and empirical analysis, it was in part because of our working relationships with other scientists. He wrote:

> "When they [agricultural economists] speak crop rotation, fertilizer, or alternative harvesting techniques, they usually know, sometimes from personal experience, what they are talking about. Preoccupation with the standard of living of rural population has led agricultural economists into collaboration with home economists and sociologists, that is, with social scientists of the 'softer' kind" (p. 5).

It seems clear that based on articles we published in *RAE*, we no longer merit Leontief's accolade. There was not a single first author and only 8 co-authors from our sister disciplines who published in *RAE* during our period of editorship. Nor could we find significant number of references to publications from our sister disciplines. To us, this result signals our increasing isolation.

What We Published

In analyzing what we published in *RAE* during the past three years, we find different ways of classification helpful. Johnson described the research process as creating a continuum of knowledge divided into three categories: disciplinary, subject-matter, and problem-solving (see figure 1). Disciplinary knowledge is created by basic research and the other two by applied research from multiple disciplines with problem solving research more problem specific (less general) than subject matter research. Thus, the classification of any particular article is somewhat subjective.

The research published by *RAE* during the last three years tends to fit mostly in between the applied subject-matter research and theoretical or disciplinary research. Thus, while only one or two articles would be considered purely theoretical (basic research), substantially more (some 20-25 percent) have made contributions to disciplinary knowledge as defined by Johnson, "...research to develop and improve economic theories, quantitative techniques for economists, and the measurement of basic economic phenomena and parameters such as supply and demand elasticities, multiplier effects, and the gross national product" (p. 12). These have helped to advance our understanding of economic theory and/or measurement techniques (econometrics, programming, control theory, etc.), thereby contributing to the capacity of our basic discipline, economics.

Table 2. Academic Ranking of *RAE* and *AJAE* Authors

Panel A: Percentage of First Authors by Rank Publishing in *RAE*, 1991-93, and a Sample of *AJAE* Issues for the Same Period

	RAE	*AJAE*
Research Assistants and Assistant Professors	48	34
Associate and Full Professors	39	47
Others	13	19

Panel B: Percentage of Second Authors by Rank Publishing in *RAE*, 1991-93, and a Sample of *AJAE* Issues for the Same Period

	RAE	*AJAE*
Research Assistants and Assistant Professors	30	22
Associate and Full Professors	59	53
Others	11	25

Figure 1. A Continuum of Knowledge

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(Most RAE articles)
Disciplinary.....Subject Matter.....Problem Solving
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Most of the research published in *RAE* (perhaps two-thirds or more) is directed toward a largely unidentified generic group called decision makers, a few of whom were further identified as public policy decision makers. One justification for directing our research toward such a generalized population is that public money should not be used for research activities useful primarily for the benefit of small groups. Such narrow groups should be serviced by paid consultants.

We might also classify the type of articles we published based on their appeal to readers. *RAE* advertises itself as publishing "articles that communicate applied and empirical findings of interest to agricultural economists engaged in research, teaching, extension, business and government service" (inside back cover, any issue). During the past three years, we mostly published articles of interest to agricultural economists engaged in research.[4] Only one or two articles each were strictly in the extension, teaching, and opinion categories. A sizeable number of articles were about the profession of agricultural economics, but most of these were applied, empirical studies. The invited papers, one in each issue, were more general, usually reviews of important topics.

Debertin and Pagoulatos examined the changing mix of articles appearing in *AJAE* during the 1919-1990 period. They found the percentage of nonquantitative articles has consistently dropped over the period. For example, during the 1936-38 period, 100 percent of the articles were nonquantitative; moreover, during the 1919-1950 period, only five times did the percentage of nonquantitative articles published as a percentage of the total fall below 90 percent. During 1980-1990, however, nonquantitative articles published averaged only 9 percent of the total.

We found that 95 percent of the articles published in *RAE* during our tenure were, according to Debertin and Pagoulatos' classification, quantitative (see table 3). The quantitative focus of the articles tended to be about equally divided between econometric applications and other mathematical models (programming, simulation, control theory, etc.) with slightly more econometric models published in the last three issues. Thus, *RAE*'s publication of quantitative and nonquantitative articles is consistent with Debertin and Pagoulatos' findings.

Just and Rausser (1989), in an evaluation of professional media (*AJAE*, *Choices*, and annual meetings), claim an overemphasis on *ex post* analysis of historical secondary data using formal frameworks. They associate this trend with efforts of agricultural economists to adopt methods consistent with Poppers' notions of observable phenomena and falsification as the standard for science. They ask for, instead, more forward-looking articles, case studies, problem definitions, and heuristic application of economic principles based on understanding and experience.

The materials published in *RAE* during 1991-93 seem to fit the Just and Rausser historical, standardized framework category. At least the vast majority of the articles utilized models which were tested or analyzed using historical secondary data. Relatively few were strictly forward-looking, i.e., attempted to determine the shape of future events, although many certainly had implications for the future, i.e., had results that could be utilized in making policy or entrepreneurial decisions. The limitation of such efforts, Just and Rausser assert, is that we are unlikely to anticipate future events.

Castle, in the 75th anniversary issue of the *AJAE*, called for more review type articles, those that integrate the scattered bits of research encompassed in published articles. He cites the articles published in the *Journal of Economic Literature* as good examples and recommends that both commissioned and unsolicited integrative papers be encouraged. *RAE* has approached this issue through its use of an invited paper in each issue of our journal. These generally are reviewed and should help meet the need for integrating research.

Just and Rausser (1993) discussed the importance of the Land Grant University system broadening its research focus beyond the issues of concern to production agriculture that represent only 2 percent of the total populations. They recommended that at least research address the concern of those 18 percent of the population who work in the food and fiber industries. They also emphasized the need for research in those areas that have a public goods aspect where the private sector cannot conduct and capture the benefits of the research.

Few of the articles published in *RAE* during our tenure would have satisfied Just and Rausser. Nearly all the articles were directed at farm problems, i.e., appeared to be for the benefit of the 2 percent who operate farms. Only a few of the issues addressed apply specifically to the other 18 percent or so involved in the agricultural industry; many more, however, had important implications for the 100 percent who are consumers of agricultural products.

Why We Publish What We Do

Editors are generally limited in what they publish to what is submitted for review, and from among those, the ones the reviewers find acceptable. So the question is: What determines the kinds of papers that are submitted for review and hopefully to be published? This question is especially relevant to agricultural economists in land grant universities who in the past have enjoyed considerable freedom in what they research and submit for consideration. Current trends, however, suggest that in the future, constraints on financial resources will limit this freedom.[5] Then, we will need to ask: Will we be supported if we continue to publish the same kinds of articles in the future as we have done in the past?

We suggest that authors are pointed to research that is disciplinary in scope because research published in national journals must appeal to national audiences. Specialized

Table 3. Comparative Classification of Refereed Articles Published in *RAE* and *AJAE*[a]

Quantitative Method Used	*RAE* (1991-93) Percent	*AJAE* (1985-90) Percent
Econometrics	52	55
Programming	6	5
Dynamics	22	14
Other Quantitative	19	9
Nonquantitative	5	16
TOTAL	100	100

[a]Based on a count of 86 *RAE* articles and 492 *AJAE* articles.

problems specific to a particular area are not likely to pass the generalized interest test. Another factor influencing the choice of topics is the cost and time required to complete research.

Leontief (1993) has noted that fact finding is much more expensive than theorizing. Agricultural economists carefully attuned to the cost of publishing find it most efficient (cost of research required per page of publication) to publish applications of theories and the latest statistical or econometric methods using secondary data. Focusing on methods allows one with slight adjustments to retrofit past studies to other applications improving in the process their efficiency. Collecting primary data and carefully verifying secondary data is time-consuming and expensive. Moreover, it may not be easy to republish descriptions of empirical facts. We believe that pressures and efficiency considerations described above are what accounts for our profession's tendency to publish articles that analyze and test historical secondary data in a standardized format.

Another reason we find cost and time requirements so critical in the choice of research in agricultural economics is the emphasis on number of publications as opposed to quality of publications. The quality of articles published in our journals including *RAE* varies. And yet, we find few mechanisms in place that distinguish the quality of published articles beyond the fact they are published. Thus, without truly effective quality distinctions, the goal becomes to publish a lot. Consequently, unless more efforts are made to recognize and reward high quality in what we publish, we expect to continue to find an emphasis on numbers and articles that tend toward disciplinary methods demonstrated with secondary historical data.

This tendency toward theory and away from facts, Bonnen referred to as antiempirical. He noted:

> "Since World War II agricultural economics has been drifting toward an antiempirical and a disciplinary outlook, away from the great empirical tradition around which the profession was built and upon which its reputation still rests. Today we celebrate theory and statistical methods while ignoring the data collection and problem solving necessary to validate our theory and models. Any profession becomes what it celebrates and rewards" (p. 1078).

Bonnen also adds:

> "Disciplinary capability is vital, but we are not, as an institution, free to focus exclusively on disciplinary research: individuals, yes; departments and colleges, no. When entire departments devote themselves solely to pleasing disciplinary peers, they eventually lose much of their understanding of and relevance

> to the society and its problems...It leaves agricultural economics without a culture capable of sustaining extension or many types of applied research. One other consequence of the move toward cost efficiency is the simultaneous move away from empirical efforts. That we have abandoned all pretenses to be an empirically based science has been noted by several" (p. 1078).

Who Reads the Journals?

There may be one overriding reason why *RAE* readers subscribe; to see what is being published so they can join in the fun. The journal contains important clues about what is acceptable research. Moreover, if our science justifies itself through academic persuasion or rhetoric (McClosky), then one must be trendy in his or her choice of topics and methods. During our tenure, 73 percent of our subscribers were agricultural economists at universities; 7 percent of our subscribers were graduate students; 5 percent were employed by state or federal governments; and 15 percent were associated with private businesses or other nonacademic enterprises.

Granted that learning how others publish and therefore how one might direct one's own publishing effort is useful. But the fact remains that there are many other important reasons for reading *RAE* and other journals. And more importantly, there are other subscribers and potential subscribers who read with a different purpose. Barry lists some purposes of the journals such as to serve as gatekeepers of quality, to advance the interest of the authors, to enforce specific scientific norms, and to provide a scientific forum. Some other purposes for reading the journal might include:

- to gain insights into practical personalized problems;
- to learn improved teaching methods;
- to learn more about an interesting topic;
- to prepare for an outreach assignment;
- to upgrade one's research skills;
- to become more familiar with a respected author's work; and
- because you are assigned to a tenure and promotion committee and have to read a particular candidate's articles.

Despite the lofty potential good our journals could achieve, it is disappointing to find our profession finds *AJAE* (and we expect the same verdict for *RAE*) to be largely irrelevant. Again, Just and Rausser (1989) note:

> "...almost all major professional groups place high emphasis on output in the *AJAE* (which has high incentive given the reward structure facing most groups), but almost no group relies on it as an important input in their thinking (probably because of the high transaction cost of reading journal articles)" (p. 1190).

Perhaps we should take a lesson from a proposal made by the economics profession. In the December (1993) issue of *The American Economic Review*, 463 economists signed a petition calling on graduate departments of economics to do a better job of preparing economists to teach the real-world economics that undergraduate students need. We wonder how many of our journal subscribers would sign a similar petition asking for more real-world economics to be printed in agricultural economics journals? We expect it would be substantial.

The Review Process and Reviewers

Article reviewers have an important influence over what gets published. They are for the most part authors of refereed articles themselves. Thus, the standards applied in the review process reflect those of the suppliers and not necessarily the desires of those besides the authors who read the journals. An exception to the closed loop from one supplier to another supplier in *RAE*, at least, were the invited articles. These were reviewed but the acceptance decision had already been made.

VMR refer to the "luck of the draw" in getting articles accepted. They refer to Peters and Ceci's findings in which they cosmetically changed 30 manuscripts that had been published in the previous 18 to 33 months and submitted them for review to the same journals that had originally published them. Ten percent of the articles were recognized as having already been published and only 14 percent of the remaining submissions were reaccepted for publication.

Peters and Ceci's findings suggest that if luck of the draw is such an important factor in publishing, then repeating the draws or persistence may be the most important strategy to follow for authors who desire to publish. Supporting this view, VMR write:

> "A fairly clear and consistent outline for success in publishing appeared throughout the results. First, persistency was one of the most important characteristics in getting a manuscript published" (p. 252).

RAE reviewers often based their reasons for accepting or rejecting articles on the importance of the problem. It was not always clear, however, the criterion used to determine article importance. A reason reviewers frequently gave for accepting or rejecting articles was related to the methodology used. Reviewers believed they could determine if the methods used were appropriate and sufficiently innovative to warrant publication. Rejected articles were largely the same type of articles as those published except they were lower in quality.

Few extension and teaching articles were submitted for review in *RAE*. Those that were had higher acceptance rates than research-oriented articles. We, like most editors, depended on reviewer recommendation. However, these frequently provided conflicting signals (see table 4). When three reviewers reported, 21 percent of the time they all gave the same advice; 72 percent of the time two of the three reviewers agreed; and 7 percent of the time they all had different advice. When only two reviewers reported, they agreed 63 percent of the time and disagreed 37 percent of the time.

Based on a random draw, three reviewers should have agreed 30 percent of the time. Instead, they only agreed 21 percent of the time. Moreover, based on random draws, when they did agree one-third of the time, they should have recommended acceptance, rejection, or reconsideration after revision. Instead, when three reviewers did agree, 36 percent of the time it was to recommend rejection, 9 percent of the time it was to recommend acceptance, and 55 percent of the time it was to recommend reconsideration after revision.

Our response as editors to the lack of agreement was to err on the side of the authors by providing them the opportunity to respond to the reviewers before making a final publication decision. The result of this policy was to find on several occasions reviewers changing their recommendations based on additional information provided by the authors.

Still, the luck of the draw undoubtedly applies to *RAE* and some rejected articles were as good or better than some that were accepted. Much as in the case of democracy, the peer review process probably is the worst method for selecting materials to be published-with the exception of all other methods that might be used.

Refutable Hypotheses and Cumulative Results

It seems to us that most useful scientific advances have been a result of efforts to build and test refutable models with reliable data in the spirit of Popper's recommendations. To illustrate, consider a scientific model represented by the letter p that predicts an outcome represented by the letter q. The relationship between p and q is: "if p, then q." If it is true that "q" can also be predicted by models p1, p2,..., then observing q does little to confirm p. The scientific community agrees that observing q never proves the model p nor does observing not q disprove the model. Observing q or not q does, however, help the scientific community arrive at a consensus about the relationship between p and q.

Table 4. Agreement Among Reviewers

Panel A: When Three Reviewers Reported

	Number	Percent
Three Reviewers Agree	11	21
Two Reviewers Agree	38	72
All Reviewers Disagree	4	7
TOTAL	53	100

Panel B: When Two Reviewers Reported

	Number	Percent
Two Reviewers Agree	10	63
Reviewers Disagree	6	37
TOTAL	16	100

A reliable test of a model is possible when the prediction q is unlikely to be made by other competing models. For example, Mendel's inheritance model that predicted 1/16 long stem sweet peas from a particular breeding experiment and the prediction of the appearance of Halley's comet were unlikely to have been predicted with incorrect models. Thus, when the predicted q was observed, it provided important evidence used to build a consensus. Commenting on efforts to build consensus, Kuznets wrote:

> "To obtain convincing confrontations between theory and fact, large bodies of microdata are needed which refer directly to decision units whose economic behavior is the object of study. Such data are costly and we have much to learn about how to collect and analyze this information. But it may be the only way in which new and useful theory can be developed, and this, after all, is the primary purpose of quantitative research."

While it may be that the articles characterized as analysis of historical data in a standard methodology may be the profession's response to testing refutable hypothesis, it has not been very successful. There are two reasons for this lack of success. First, the reliability of the data is often suspect; and second, there are few efforts to accumulate or confirm earlier results.

Pope et al. attempted to replicate results of previously published land value studies. Pope and his co-authors found that, "when recent data were added to the sample, numerous changes in signs of coefficients occurred for all of the simultaneous equations models. Further, most of the estimated coefficients were not statistically significant from zero." They concluded from their study that the previously published models did not reflect accurately enough the relevant structural changes and other characteristics of the farmland market. Thus, we conclude that the earlier studies did not produce cumulative knowledge or learning.[6]

Tomek's efforts to confirm earlier studies matched Pope et al.'s experience. Tomek writes:

> "Published and anecdotal evidence on confirmation in economics suggests the disheartening conclusion that many published empirical studies

> contain errors and that some of these errors are serious in the sense that, if corrected, the stated conclusions of the study would change" (p. 13).

Wible explains why so little research is confirmed:

> "The highest rewards in science are reserved for innovations, and research time is scarce. Moreover, empirical research in economics is often complex, and hence confirmation is potentially time-consuming...replication failure is understandable in economic terms: a researcher's overriding objective is to maximize his or her own expected utility, and this utility is maximized by the prompt publication of (hopefully) 'innovative research.' This discourages confirmation research on the one hand, and on the other does not provide payoffs for maintaining records which can be used by those interested in confirmation."

Leontief explains further why the focus on econometric studies is related to the lack of confirmation studies:

> "Formal manipulation of an econometric model containing a small number of variables, and correspondingly few equations, is relatively simple. The structure of such a model and the conclusions derived from it can be easily explained. However, these advantages of aggregative modeling are secured at a very high, probably unacceptable, price: the uncertain, very tenuous relationship between the model and the real phenomena it is supposed to describe and explain."

As the link between the model and the real phenomena it is supposed to predict becomes less tenuous, so does the possibility of building refutable hypotheses. Thus, we are left without the ability to accumulate results. So, instead of building refutable models, we construct increasingly complex methodologies applied to fragile nonreplicable data sets which produce uninteresting empirical results.

We believe that because so few confirmation studies have been made, the marginal social benefit from one more confirmation study is higher than the marginal social benefit from another article attempting an innovative approach to a previously studied problem. The reason such an imbalance exists in the type of articles we publish has already been described: authors are responding to higher costs of doing confirmation studies and the pressure to publish often to achieve certification. Therefore, the private market signals to authors to do confirmation studies are inadequate to encourage the desired efforts.

Changes are needed in our journals to encourage confirmation-type studies. One change might be for journals to require authors who present new empirical results to demonstrate that the new results improve upon previously published results. The test would require both the old and new model be fitted to the original data file (used for the old model) and to new data now available. Tomek's criterion for publishing a new model: "New results should increase our understanding, not add to existing confusion" (p. 14) should be adopted.

Conclusions and Recommendations

We conclude that one of the main private benefits from *RAE* and other similar journals is to certify authors' professional credibility. Thus, it is more important for assistant professors and others in the early phase of their career to publish than for more senior professionals.

The cost of publishing, we believe, is a fundamental force in determining what gets researched and eventually published. An additional factor is that published articles must be general in orientation pushing us toward more disciplinary articles. We must reluctantly agree with Bonnen that our journal publications represent a drift toward antiempiricism. Equally discouraging for the advancement of our science is that we appear determined to do our research in isolation from other disciplines. Moreover, it appears that our research is increasingly isolated from extension applications. The result of this separation is increasingly irrelevant research appearing in even applied journals and problem-solving methods employed in extension that are inadequate or inappropriate.

Unfortunately, we find little effort being made to identify high quality among our published efforts. Perhaps it is because high quality work requires the test of time. Whatever the difficulty we have in identifying quality, we appear willing to substitute quantity so that agricultural economists appear under pressure to publish a lot.

It is clear that society's patience with academic enterprises is being exhausted.[7] If this lack of patience becomes translated into further decreases in funding, our research enterprise will be driven more toward contracts and grants, challenging our ability to perform what in the past has been the land grant mission. To maintain and hopefully regain our relevance and public confidence, it is essential that we make some changes in what and how we publish. Some recommended changes are:

1. Increase the incentives to publish articles with high social benefits so that innovative articles introducing new ideas with high private goods benefits are balanced with studies with high social benefits.

2. Increase efforts to document the social benefits that result from our research. In the past, the public has generally accepted that our research does indeed have a high social rate of return. They are less believing now. Thus, we must demonstrate how our research benefits the public. Research that never reaches the public directly or indirectly cannot be justified. And unless we can at some point demonstrate how the public benefits from our research (even if they do not do so directly by reading the journals), then public support for our publication activities may be hard to maintain.

3. Make applied journals like *RAE* more accessible to the public. We do need journals like *AJAE* that are written by and for narrow groups of researchers with similar skills and interests. The hope is that from such communications, there will eventually be a public good produced. However, we do not need all of the formerly regional journals to imitate *AJAE* in what they publish and how they communicate. The development of new methods and novel applications of existing tools is important. But not all of our journals need to be publishing mostly articles of the same type.

   Perhaps a publication like *Economic Perspectives* written primarily in English or the *Journal of Production Agriculture* that publishes English summaries of articles could serve as role models for new publication efforts designed to be more reader accessible. If important and novel applications of econometric and economic tools are used to obtain the findings, they can be described in a later part of the articles or in an appendix (as in the National Tax Journal) for those who will be required to investigate the efforts before publishing on the same subject or for those who merely want to check the work.

4. Refuse to accept from editors the excuse for not publishing teaching,

extension, and other types of integrative and review articles that would appeal to a more general readership. The excuse: "I can only publish what is submitted" is not valid. We must demand more of our editors. Our experience publishing invited papers leads us to conclude that we could have also commissioned teaching or extension articles. Editors can and should exert publication directions and solicit articles when private incentives do not lead to the proper balance in the types of articles published.

5. Correct empirical carelessness. We place this problem in the category of a crisis. We must return to our roots and redouble our efforts to discover and report facts which will likely point us to more cooperative efforts with our sister disciplines and extension counterparts in the real world. We are after all an applied field.

6. Redress the market failure in our profession in the publication of confirmation studies. We desperately need more of the articles like Pope et al.'s and Tomek's. We also need to encourage, reward and, if possible, require any new article on a subject where there is an existing published model supported by historical empirical tests to confront the existing model. For example, in the area of land values, many articles have been written with competing assumptions. Surely, before we add still more land value models, we owe it to those who pay for our research and those who read it, to place the new model in the context of what has already been done.

7. Make our journals more of a classroom where ideas can be more generally shared and explored. We believe the exchange of ideas should be an important purpose of the journal with the public being the ultimate beneficiaries. These goals for our journals are unlikely to be achieved if we continue to limit publication space to those who can speak the latest in econometric or mathematical models. In truth, we aren't nearly as precise as one might infer from our prose-so why not let more authors express themselves in English if they prefer? Perhaps if we accepted English as the common language, we would hear more from those with important ideas written from different views.

We believe that publishing in our professional journals is important. It is the profession's effort to renew itself. We also believe that the peer reviewed journals need to change. We believe research publication should be the information exchange for those engaged in outreach, research, and teaching. We must recognize that we are not meeting the needs of a general readership and the social benefit of our efforts needs to be better documented if we are to maintain our public support. Perhaps for *RAE* this means adopting the style of *The Journal of Economic Perspectives*. Or at the very least following the lead of the *Journal of Production Agriculture* and printing executive summaries at the beginning of the journal.

If a paper can be made more accessible to a broader audience without reducing the quality or value of the contribution, then why not do it? Enhanced accessibility, which does not detract from the value of the basic contribution, will increase social returns because it is likely to "permeate" more quickly and society has a discount rate.

The good news is that we have all of the tools necessary to become a truly relevant force for supplying useful knowledge in today's world which is much broader than firms engaged in production agriculture. Our ideas and our contributions are indeed important, but as a successful business manager once reported: "marketing is everything."

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Endnotes

1. See Ayer and Schuh for an example of a study of social rates of return related to research.

2. This estimate is based on a sample of issues of *AJAE* during the same 1991-93 time period.

3. These results are based on a 10 percent sample of the membership listed in the 1991 AAEA Directory. The 1991 Directory was used instead of the 1993 Directory because the latest Directory does not list rank. The membership percentages were: Assistant Professors and Research Assistants - 22 percent; Associate and Full Professors - 33 percent; and others were 45 percent.

4. It may be that the benefit or the usefulness of today's current research requires a longer run view. For example, the research results may not enter the mainstream of teaching, research, and outreach until the next generation of agricultural economists. That is, research results used in extension and teaching today may well have been in the journals of an earlier time that were ignored by teachers and outreach workers at the time they appeared.

5. Evidence of the public's increasing concern over how funds are spent is manifest in Congress's arbitrarily eliminating projects from NSF budget because they viewed the projects as trivial.

6. For an experience similar to Pope et al.'s, see Dewald, Thursby, and Anderson.

7. See, for example, articles by Limerick, Collander, and "Towers of Babble" appearing in *The Economist.*