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Research: Are We Valuing the Right Stuff?

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The incentives researchers face depend directly upon what we as a profession value. The impacts of research can be either disciplinary by adding to economic knowledge or real world by being useful to economic agents. Various measures of research impact such as publications, citations, and external funding are discussed, and the strengths and weaknesses of each measure are evaluated. Because of the difficulty of accurately measuring research impact, we must depend on internal incentives to motivate researchers to select topics with the most potential impact.

Key words: incentives, methodology, research impact

Introduction

Accountability is now a buzzword in education. Efforts toward accountability are beginning to creep into research. As economists, we should welcome accountability, even though as individual researchers we may resent the encroachment on our freedom. Another concern about accountability efforts is that they may be merely symbolic gestures, taking up a lot of time but offering minimal value.

As part of the accountability process, the following questions arise: (*a*) What is research impact? (*b*) How do we measure research impact? and (*c*) How do we obtain more research impact? This article addresses each of these three questions. In some disciplines, knowledge itself is valued. Most economists typically accept the premise that research has value when it increases social welfare. Yet, contribution to knowledge is often all we can reliably measure, and even that must be measured subjectively.

Research Impact

The impacts of research can be disciplinary or real world. As a profession, we need both. While most departments need both, individuals can specialize in one or the other. The association formerly known as the American Agricultural Economics Association (AAEA) has positioned its journals accordingly. The *American Journal of Agricultural Economics* is the outlet for creative work that adds to disciplinary knowledge. The *Review of Agricultural Economics* has been the forum for work that at least attempts to provide direct application to a policy or business problem. The *Journal of Agricultural and Resource Economics* (*JARE*) has traditionally encompassed both disciplinary and real-world research.

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Reviewers and editors cannot determine actual real-world impact. The product is only one of the four "P"s of marketing. Promotion is still the primary "P" that determines real-world research impact. Quality research is generally not enough to obtain real-world impact. To achieve real-world policy impact, researchers must work with state and federal agencies. To get real-world impact on business, some sort of an outreach program is needed. If we want to measure and reward real-world impact, it must be done primarily by departments. One exception is the AAEA outstanding policy contribution award, which is designed to specifically recognize real-world impact.

I have always been critical of policy or extension programs that do not have a strong base linked to work in peer-reviewed journals. Even if a policy is changed due to an agricultural economist's work, how do we know if the policy was changed in a social welfare-increasing direction? Peer review has the potential to at least reduce economic and statistical mistakes.

I have never been a fan of publications like *Choices*, *Western Economics Forum*, or my own department's now defunct *Oklahoma Current Farm Economics*. What is the goal of such publications? Is it certification or communication? As previously mentioned, certification of policy and extension work is certainly a worthy goal. In practice, such publications usually must commission papers with the implicit guarantee that the papers will be published. My department, for example, used to have a schedule of who was going to publish in each issue. While such papers typically undergo a form of peer review, the review is generally conducted with the understanding that the reviewer does not have the power to ask for much more than editorial changes. Thus, these publications do not go very far in providing certification, and the fact that they do not have the full status of a refereed journal article is understandable.

The goal of publications such as *Western Economics Forum* is most often given as communication to those outside the profession. The problem is that real-world audiences are too splintered. If you want to reach futures traders, publish in *Futures* magazine. If you want to reach feedlot managers, publish in *Feedlot* magazine. One possible exception is that a *Choices* magazine targeted toward Washington, DC, policy makers would have a possible audience. Still, if you want to reach Washington policy makers, contacting them directly seems to have shown the most success. Also, *Choices* articles are too long and too slow to appear in print for a Beltway audience. As argued with journal articles, publication is rarely sufficient to achieve real-world impact. Publications like *Western Economics Forum* are not without value and do promote some communication within the profession, but it is not clear they provide much research impact.

What is the role of our professional journals in promoting research impact? Journals provide communication among peers, but offer only meager discourse outside academics. Organizations such as the American Farm Bureau, the Commodity Futures Trading Commission, and the General Accounting Office have staff with the training to utilize journal articles directly, so journal articles have some direct real-world impact. Robison and Colyer (1994), however, argue that the main role of journals is certification. This certification is of originality, importance, and precision of the answer. The certification is not a certification of value to society.

Measuring Research Impact

Department heads bear the main responsibility for determining salary increases, and they presumably make decisions based on research impact. Department heads appear

to have delegated much of the responsibility for evaluating research impact to journal editors and reviewers. Broder and Ziemer (1982), as well as Hilmer and Hilmer (2005), report a clear relationship between journal article output and salary. Research, especially disciplinary research, is too specialized for department heads to accurately evaluate research quality.

The faculty salary studies show that the primary method used to measure research impact is the quantity of refereed journal articles and the prestige of the journal in which an article is published. One concern is whether editors and reviewers are valuing the right stuff. The criterion editors and reviewers are supposed to employ is contribution to knowledge—not contribution to social welfare.

Our journals suffer criticism from the more applied members of our profession. This conflict between the disciplinary researchers and the more applied researchers is certainly not new. A department that emphasizes only disciplinary research risks the loss of state support, and one that stresses only real-world research risks becoming stagnant. Supply and demand are such that disciplinary researchers are usually more highly paid, but there are exceptions. We could argue over whether our profession places too much value on disciplinary research as opposed to more applied research. Disciplinary research that truly advances our theory or methods is of long-term value and deserves its place on the pedestal. I argue here that our values placed on disciplinary versus applied research are at least not too far off.

Of greater concern is whether publication in a refereed journal is sufficient to indicate value. Even Albert Einstein argued that some of his own published papers were of no consequence (Isaacson, 2007). The recognition that not all journal articles are equal has led to the search for other measures of research impact. One such measure is awards and another is citations.

Table 1 provides a list of the *JARE* Published Research Award winners over the last 10 years. Of particular note in table 1 is the preference of award committees for papers that have policy implications. Three of the 10 papers listed made primarily disciplinary contributions of theory or technique. Only one award-winning paper was most relevant to an agricultural firm. Thus, awards are mostly given to a narrow component of the papers published in the *JARE*. An emphasis on awards would skew research toward policy issues and away from work relevant to agricultural businesses.

Table 2 gives a list of the most cited *JARE* articles in the *Web of Science* (which contains the *Social Science Citation Index*). As shown in the last column of table 2, the number of Google Scholar citations would lead to a different ranking, but a similar list. Only two of the *JARE* award-winning papers from table 1 appear on the list of top cited articles. The highly cited papers tend to be hot research areas. Thus, citations may be more a measure of how many other researchers are working in the same area than a measure of research impact.

The *Web of Science* cautions against comparing citations across disciplines. Agricultural economics is apparently too broad an arena to place substantial emphasis on citations without adjustments for the number of agricultural economists working in the area. The highly cited papers are likely above-average papers for the *JARE*. But, if we place too much emphasis on rewarding citations, we could end up with too much research focus on hot areas. Solving narrow specific problems is not likely to result in many citations. The second most cited paper in table 2 is a review article. Review articles are well known for receiving more citations (Monastersky, 2005) even though

Table 1. *JARE* Published Research Award Winners, 1991–2006

Article	Topic	*Web of Science*	Google Scholar
McNew and Fackler (1997)	Technique	25	47
Hurley, Babcock, and Hellmich (2001)	Policy	15	28
Chavas (1999)	Theory	5	21
Goodwin and Smith (2003)	Policy	4	16
Starbird (2000)	Policy	3	8
Richards and Patterson (1998)	Policy	2	8
Kan, Schwabe, and Knapp (2002)	Production/Technique	7	7
Blank, Erickson, and Moss (2005)	Policy	0	1
Norwood, Lusk, and Brorsen (2004)	Technique	0	1
Vedenov, Duffield, and Wetzstein (2006)	Policy	1	0

Table 2. Top 12 Cited *JARE* Articles in *Web of Science*, 1997–2006

Article	Topic	*Web of Science*	Google Scholar
Roosen et al. (1998)	Food Safety/WTP	32	53
Lusk et al. (2001)	Review/WTP	31	80
Baker and Burnham (2001)	GMO/WTP	29	81
McNew and Fackler (1997)[a]	Cointegration	25	47
Antle et al. (2001)	Carbon Sequestration	24	47
Coble, Heifner, and Zuniga (2000)	Crop Insurance	20	45
Dickinson and Bailey (2002)	Traceability/WTP	18	74
Keplinger et al. (1998)	Irrigation	17	21
Cooper (1997)	Water Quality/WTP	16	23
Huffman et al. (2003)	GMO/Food Labels/WTP	15	43
Babcock and Pautsch (1998)	Precision Agriculture	15	29
Hurley, Babcock, and Hellmich (2001)[a]	GMO	15	28

[a] *JARE* published research award-winning paper (see table 1).

review articles may have little direct impact. For agricultural economics, another weakness of the *Web of Science* is that many of our journals are not included. While citations can help in measuring quality in addition to quantity, they need to be used with caution.

Department heads are left with using journal articles largely as the measure of research output. The fear is that using journal articles as the only measure of research impact could lead to considerable research that is publishable, but adds little to the discipline and does not make a real-world contribution. Some would argue we are in such a situation now.

Grants are another possible measure of research output. Indeed, some other disciplines, such as engineering, use external research dollars as the major criterion. Hilmer and Hilmer (2005) report a substantial salary boost from successful grantsmanship, but the effect is at least partly due to the use of nine-month appointments. Beattie (1983) argues that grants are an input, and only the output from that input should be valued rather than the input itself. I would argue for two exceptions to Beattie's rule. One is that the amount the department can skim or can substitute for other funds generates a positive externality and should be valued. The other exception is when the granting agency plans to use the research directly. In this case, outside funding indicates the potential exists for the research to have a real-world impact.

As economists, we generally suggest cost-benefit analysis as an important tool. The problem with using cost-benefit analysis to evaluate research impact is that the necessary information is rarely available. A formal cost-benefit analysis of individual research projects ends up being a symbolic effort, often not worth the time it takes to conduct the analysis.

The one tool remaining to evaluate research is a qualitative case study. Frey (2006) contends the case study approach is the best way to measure research impact. Beattie (1983, p. 213) argues that the research "process must remain as decentralized, unregulated, unsupervised, and uncoordinated as possible." But this does not mean the results from that process should not be evaluated. My department asks for one-sentence bullets to describe research impacts as part of the annual appraisal process. This sort of evaluation can be performed at low cost and can help communicate the need to conduct research that matters.

Theory of Individual Research Productivity

Assume that a researcher's utility function can be represented as:

$$\max_{\boldsymbol{\lambda},\tau} U(\mathbf{B}, \mathbf{y})$$

$$\text{s.t.: } \mathbf{B} = f(\boldsymbol{\lambda}, \tau, \mathbf{x}),$$

$$\mathbf{y} = g(24 - \tau),$$

where $\mathbf{y}$ is a vector of non-research activities, $\mathbf{x}$ is the researcher's endowment of skills and knowledge, and $\mathbf{B}$ is a vector of benefits a researcher receives from publishing. These benefits include salary, but they also include prestige and praise plus the satisfaction of contributing to social welfare and the reward of solving a challenging intellectual problem. The researcher must choose the amount of time devoted to research (τ) as well as the topics ($\boldsymbol{\lambda}$). The issue is how to better align the benefits to the researcher ($\mathbf{B}$) with the benefits to society.

We decided long ago that putting few constraints on a researcher's production function is best. By definition, a constraint leads to lower output. Our main choices in motivating researchers to undertake more useful work are in defining the benefit function (f) or in increasing the ability of researchers to choose useful problems, which is one of the components of $\mathbf{x}$.

Selecting Research Topics

We do not want to concentrate too much on evaluating only immediate impact. Richard Feynman conducted some research on the wobble of a spinning plate because he thought it was a fun thing to do. It was an extension of this plate-spinning research that led to his winning the Nobel Prize (Feynman, 1985).

What makes a good research topic? In my early career I would have said any topic that is publishable in a refereed journal. I have become a little pickier over time, and now my written criteria are that I will work on a topic if (a) it has a good chance of being publishable in the *American Journal of Agricultural Economics* or a journal of similar prestige, (b) the research has a good chance of making an impact either within the profession or outside it, or (c) it is an externally funded project. Even I am not claiming that (a) necessarily implies (b). Also, I still occasionally break this rule, but it is mostly when helping someone else with their research idea.

Where do we find these research topics? We are increasingly hiring faculty who do not have a background in U.S. agriculture. In some departments, extension and research faculty rarely work together. Our national association has even changed its name to deemphasize agriculture. All of these factors raise legitimate concerns about maintaining the relevance of research within agricultural economics.

Where do good research ideas come from? Most of the creative people I know read a lot. They read journal articles as well as popular literature. Professional meetings are also a potential source of ideas. When I attend a meeting, I consider it a success if I get one new idea that I can use. I disagree with the push to emphasize short-term applied research at professional meetings, since I am more likely to be able to use ideas from disciplinary work. Research ideas can come from many other places. Colleagues in agricultural economics, and other departments, can be a good source of ideas. Even teaching has been a source of ideas when I realized there was no research to support what I wanted to tell my class. I wish I had more to offer about where to get ideas, but research ideas have never been my strength. I write all my research ideas on a pad of paper that I keep in the top drawer of my desk. The only reason I am now on my second pad of paper is that the first pad was already partially used when I started. One implication of this section is that our profession could benefit from allocating more time toward topic selection.

Overvalued Research

So far, I have argued strongly that we have two categories of research which clearly have value. One is disciplinary work that includes new theory and new methods plus empirical work that advances our understanding of economic behavior. The other category is applied work which is accompanied by a package of outreach or cooperation

whereby a real-world audience is reached. However, a large chunk of research is in between—research that adds little to the discipline, but is not ready or does not come with the necessary package to make an applied impact.

The value of this in-between research is more uncertain and more variable. It is applied work, placed in the public domain in the hope a later researcher will be able to build on it or a policy analyst will perhaps be able to use it. Research in this category also builds some human capital for graduate students and assistant professors.

Spending years as an editor and reviewer without developing some specific likes and dislikes is difficult. Below, I identify three categories of papers I think are overvalued.

Application of Methods

As an editor, I remember writing numerous times that we are not interested in papers that apply techniques to data sets; we are interested in papers that provide answers to economic problems. This reasoning is a mistake often made by young faculty. Learning new techniques and applying them can personally be very satisfying. A normal progression in agricultural economists' careers is to be excited about the newest and most complex techniques early in their careers, and to gravitate to more problem-focused research as they get older. Researchers typically become disenchanted with simply estimating models, and they want to do work that matters. I am not saying that offering something new in method is not positive. I refer to such things as technique gimmicks. Certainly the more precise the answer, the better it is. Technique gimmicks are good things if they add to the precision of the answer.

One approach to doing research is to scour the economics journals, find a new technique, and apply the technique to a readily accessible agricultural economics data set. It is possible to build a nice resumé with this approach, yet never achieve much research impact.

Semi-Attached Research

Another category of overrated research is what I call semi-attached research. One of my favorite books, *How to Lie with Statistics* (Huff, 1954), refers to the semi-attached figure. A semi-attached figure is used to address something it really does not. Similarly, semi-attached research identifies what Ethridge (2004) would call a very important problematic situation. However, the research itself never defines a researchable problem and ultimately provides no answers. For me, a large portion of the technology adoption and cross-sectional demand studies fall into this category. Yes, we can conduct a survey and determine that more educated people are either more or less likely to adopt—but what can we do with the information?

Far too much of our funded research falls into the semi-attached category. We have a tendency to throw money at a problem even when solutions are very unlikely. The finding of Huffman and Evenson (2006) that formula funds have greater impacts than competitive grant funds is consistent with my view.

Research Fads

The final category of overrated research is research fads. How many surveys do we need to tell us that consumers in Europe dislike food from genetically modified organisms

(GMOs)? We will soon have more research on ethanol, biodiesel, biofuel, and bioenergy than is needed. Economics research suffers more from fads than we do, but it is still a problem for us. Economics has endured fads about rational expectations, cointegration, and now "freakonomics."

I predict an overreaction to the current high oil prices, with nearly every experiment station director feeling the need to throw money at the latest issue. In the past, these issues have been international trade, value added, and GMOs; now it is biofuels. Although we do need people who chase the latest hot issue, we would be better off with a few less individuals doing so.

Revising Our Peer Review Process

Publications are the best measure of research impact that we have, if for no other reason than we have reviewers doing the evaluating who are qualified and take their job seriously. We should still consider the question of whether or not it is possible to improve the peer review process.

Our peer review system works amazingly well. It is still a noisy signal. We have a lot of variation in "contribution to social welfare" within a journal. I felt very comfortable making decisions as a *JARE* editor. In contrast, making decisions at the *AJAE* was more difficult. *AJAE* reviewers were much more negative, with very few papers getting all positive reviews. With the *JARE*, I could generally follow reviewers' recommendations, but with *AJAE* responses being so negative, too often it was left to me (or an associate editor) to decide which papers to publish in spite of reviewers' objections.

As a profession we share a common set of values regarding economics research. These values are mostly about the proper way to conduct research and are critical in helping us make a contribution to society. Publication is certification that the paper meets these common values. As an author, I am thankful we have the review process to impose these standards on me.

We do have differences in values, and these differences account for much of the randomness in the review process. My guess is that most of the decisions at the *JARE* would be the same regardless of the editor or reviewers. The *AJAE* is a little more random with about half of the papers almost certain to be rejected, a very small portion of the papers nearly certain to be accepted, and the rest having varying probabilities of being accepted. What explains this difference in values? First is simply a difference in the level of perfection expected by the reviewer. Reviewers who reject everything or accept everything are rare, but they can create a major problem. Reviewers should strive to recommend revision slightly more often than the journal's acceptance rate. The reasons for aiming slightly above the journal's acceptance rate are that (*a*) when reviews are split, the editor is more likely to side with the negative reviewer, and (*b*) some papers are not published because they are never revised or are revised unsuccessfully. The second disparity in values is due to different weighting given to the paper's attributes. These attributes include disciplinary contribution, potential real-world contribution, precision of the answer, and writing quality. Some reviewers have a nearly lexicographic preference and make decisions based on a single attribute. As an economist, I prefer allowing tradeoffs. Some reviewers place more weight on originality, while others place more weight on the importance of the topic.

If you skim through our journals, you see that most of the work included is well done. Editors must select from what people submit, and they must publish something. If an author is persistent, a competent piece of agricultural economics research will find a home somewhere, even if its contribution to social welfare is near zero. Shiveley (2007) argues that editors are like air traffic controllers, and they keep papers circling until they are polished well enough to land. I have eventually published about 90% of the papers I have written, but I have had papers rejected as many as eight times before they were accepted for publication. If you have something to say and are willing to persevere, you will be given a chance to say it.

Conclusions

The goal of increasing the social value of the research we do is a worthy objective. Some inconsistencies occur between the benefits a researcher receives from conducting research and the benefits to society. Society might benefit from a little more frontier disciplinary work, more rigorous research that has the package to achieve real-world influence, and a little less of the work in the middle.

We are still left with a system that uses peer review to provide a certification of sound economics. Departmental reviews can go part way in evaluating relevance, but going too far could discourage innovation. An asymmetric information problem occurs in that researchers have more information about the potential usefulness of their research than does anyone else. One goal of this article is to encourage researchers to try to undertake research that matters even though it may or may not make a difference in how much they are paid.

The certification provided by professional journals is one of the keys to scientific progress in agricultural economics. While we may lament our lack of influence on society, other academic disciplines envy the success of economics. As a researcher, would you really rather be in management or education where new ideas are often adopted without any empirical testing? Yes, we could do a little better. But I am still glad to be in a profession that values both disciplinary and real-world contributions, and that follows the scientific method so that we can make scientific progress.

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