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Research and Publishing: Relevance and Irreverence

Gary W. Brester

The value, relevance, and efficacy of conducting and publishing research has been widely debated throughout the agricultural economics profession. On the one hand, some argue that the research process creates little value and directly competes with teaching/outreach output. On the other hand, others argue that research provides answers to important questions, improves human capital, and complements teaching/outreach activities. I argue that the research and publishing process develops human capital, improves the quality of teaching/outreach, reduces bias, generates new ideas, improves societal welfare, creates innovation, and is essential for public policy debate.

Key words: publishing, research

Introduction

> *"It is worth noting that, in 1891, Professor Alfred Marshall had no doubts about the need for the Royal Economic Society to establish a journal (the Economic Journal) but was less convinced about the need for professional associations to convene meetings, observing that 'For such discussions, unless conducted by a very strong association, might do harm: they might be attended chiefly by people whose time was not very valuable.'"*
>
> — Francis Y. Edgeworth (1891, p. 8)

> *"Research is to teaching as sin is to the confessional: if you haven't done the first, you've no business doing the second."*
>
> — David S. Jordan, President, Stanford University, 1891–1913

As I developed this address, the title seemed to evolve with each draft. When I happened upon the current title, it seemed appealing even though I was somewhat uncertain of the exact definition of the word "irreverence." According to *Webster's II* (1988), the first definition of irreverence is "lacking of reverence." This led to the discovery of two definitions for "reverence." The first is "respect, awe" and the second is "lightly or humorously sardonic." Furthermore, "sardonic" is defined as "bitter and scornfully derisive, sarcastic." Once I saw a reference to sarcasm, I just had to keep the title.

Dana Hoag's 2005 WAEA Presidential Address in San Francisco set the stage for this paper. He argued that Extension had historical claims to two comparative advantages: (*a*) information delivery, and (*b*) analytical, unbiased research efforts. He further noted

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I appreciate the comments and suggestions provided by Joseph Atwood, Bruce Beattie, James Brown, David Lambert, Dino Falaschetti, Richard Perrin, Vincent Smith, Myles Watts, and an anonymous reviewer. Given the high quality of and my admiration for each of these colleagues and the review process, those named should accept some responsibility for any remaining errors in this paper.

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that Extension no longer has a comparative advantage in information delivery. Consequently, Dr. Hoag concluded that the ability to conduct "analytical, unbiased research efforts" is Extension's only competitive advantage. I want to expand on this argument because if it is true for Extension, it is also likely true for resident instruction.

My primary thesis is that research distinguishes agricultural economists from "talking heads." It is our pursuit of research that makes us better teachers. The process of conducting research makes us better thinkers. Research builds human capital in ourselves and others. Our research leads others to seek out our advice and opinions. It is research that creates new ideas, creates value, and produces our competitive advantages. Finally, efforts to publish research have two additional benefits: (*a*) they improve our ability to explain complicated economic phenomena, and (*b*) they encourage the production of integrity.

I recognize that the publishing process is not perfect. Type I errors occur whereby some "good papers" don't get published, and Type II errors occur whereby some "bad papers" do. However, these imperfections do not provide a sufficient excuse for not participating. Most professionals (e.g., accountants, medical doctors, elementary and secondary teachers) are required to improve their human capital by attending seminars and/or formally enrolling in courses. These continuing education efforts improve professional skills and are often required for continued certification. The publishing process serves a similar role for university professors.

Perhaps more importantly, the publishing process provides a mechanism for us to exercise the necessary activity of irreverence. The generation of new ideas requires hard work, critical thought, debate, and skepticism. Economic phenomena are too complex to be left to "conventional wisdom." However, one of the side effects of analytical thought processes and disciplinary rigor is the impression that economists are "uncooperative" or "difficult to get along with." Such claims are often made by administrators and noneconomist colleagues. Nonetheless, when our research results or professional irreverence runs counter to conventional wisdom or invalidates claims of special interest groups, our uncooperativeness indicates a job well done.

Whether one has a 100% extension appointment, a 100% teaching appointment, a research appointment combined with either, or an industry/government economics position, research, peer review, and professional publishing are our comparative advantages. Therefore, and this is my message, we all need to embrace research and publishing because it is the engine of idea generation. It separates us from self-appointed experts. I couch my remaining remarks in three areas: (*a*) the value of research, (*b*) the value of publishing, and (*c*) the value of the review process.

The Value of Research

The primary value of research is that it contributes to social welfare through knowledge generation. More importantly, the process of conducting research with an eye on publishing is, in my mind, reason enough to do research. As Steven Landsburg (1997) notes in his book *Fair Play*:

> The engine of prosperity is technological progress—not just feats of engineering, but also the design of new insurance contracts, better legal systems, and improved patterns of crop rotations. And the engine of technological progress is people. Ideas come from people. The more ideas, the more we prosper (p. 144).

Beattie and Watts (1987) further argue that university professors should strive to be on the frontier of some element of their discipline, and that research keeps us engaged in our profession and intellectually active. Similarly, Boyer (1990) notes scholars must: (*a*) establish credentials as researchers, (*b*) maintain currency in their field of expertise, (*c*) retain high standards of integrity, and (*d*) submit work for peer review. Therefore, I want to discuss the value of research in terms of disciplinary teaching, job security, integrity, and thinking.

Teaching

I find it interesting and absurd that some university professors claim they do not need to continue to build human capital. I have witnessed those who claim their Ph.D. degree has bestowed on them enough knowledge to "teach undergraduates." And, some university professors and many administrators argue that research efforts are detrimental to teaching effectiveness (Beattie, 2006). However, that argument critically hinges upon one's definition of "effectiveness." Certainly, research efforts are competitive with teaching efforts in terms of time allocation. Increases in the time spent conducting research necessarily reduces the time one can commit to teaching. At the limit, if one spends 100% of his or her time on research efforts, then teaching output is zero. But, research and teaching are complementary in terms of "quality" teaching. Steven Landsburg (1997) supports this contention when writing the following advice to his daughter:

> When choosing a college, try this thought experiment: Imagine that you've walked into a living room where a small circle of people is talking animatedly and excitedly while several others sit quietly on the sidelines. If you wanted to know what the conversation was about, who would you prefer to ask? If you think the participants would give you a more accurate and enticing answer than the observers, then you should go to a university where you're going to be taught by active researchers (p. 216).

In support of Landsburg's opinion, I seldom find those who believe their only role is to teach either resident or non-resident students discussing economics in general, or the economics of food, agriculture, or natural resources in particular. However, a common theme emanating from those who have a disdain for research and publishing seems to be that researchers are necessarily poor teachers. Hamermesh (2006) addresses this concern in *Economics Is Everywhere*:

> Many students believe that professors are either good researchers or good teachers, but not both. This belief implies that there is a negative relationship between research and teaching comparing across different professors. I don't believe this is true at all: The better researchers are also the better teachers. This doesn't mean that professors have no trade-offs in their activities. Instead, those who are good at one thing are good at the other, and those who are mediocre at one are typically mediocre at both. There is a trade-off for each individual, but the overall level of ability differs among professors so that some professors can perform better in both areas (pp. 6–7).

Although I agree with Hamermesh, we should not condone the arrogance of those who believe they are too valuable as researchers to put forth the necessary effort required to do a good job of teaching. The fundamental purpose of universities is to teach and conduct research.

As Thomas Friedman (2005) states in his book *The World Is Flat*, "If we are going to produce those who can think, we need good thinkers to do it." Friedman quotes Shirley Anne Jackson, a former President of the American Association for the Advancement of Science and the current President of Rensselaer Poytechnic Institute, as saying:

> The U.S. today is in a truly global environment, and those competitor countries are not only wide awake, they are running a marathon while we are running wind sprints. If left unchecked, this could challenge our preeminence and capacity to innovate (p. 253).

Friedman continues:

> ... it is our ability to constantly innovate new products, services, and companies that have been the source of America's horn of plenty and steadily widening middle class for the last two centuries (p. 253).

Universities are a primary source of innovation. Economic thought processes are critically valuable to innovation and innovators across all fields. Economics provides a logical framework for decision making. Economics explains the mechanisms for allocating scarce goods and resources (and why such mechanisms are necessary). Economics provides the basis for understanding the economic growth of firms, industries, and nations. Economics explains the creation and sustainability of value. Economics describes the nature of risk and uncertainty.

We certainly have some teaching left to do. In a commentary in the *Bozeman* [Montana] *Daily Chronicle*, a writer commenting on the need for higher minimum wages notes:

> First, let's dispel the grand myth of economic theory, beginning with the fact that almost no one understands it, or cares to, and even those who do pretend to some knowledge are wary of its pseudo-scientific shortcomings. As John Kenneth Galbraith, a renowned and unusually introspective economist noted: '[Economics] is a subject profoundly conducive to cliché, resonant with boredom. On few topics is an American audience so practiced in turning off its ears and minds. And ... none can say that the response is ill-advised.'
>
> So we will not engage in a pedantic review of 'insider-outsider wage determination,' 'supply-side economics,' or the effects of falls in income along the Laffer curve. With this promise, I invite you to continue reading (Muhlenfeld, 2005, p. A4).

I declined the invitation.

Beattie (2006) asserts we have three audiences for our teaching: resident students, non-resident students, and peers—i.e., we often forget that advances in social welfare and knowledge require advances in our own human capital. We improve our own human capital through active participation in research. Research produces and improves wisdom and cleverness. I argue that the most important characteristic for being a good teacher is *caring* about teaching. The second most important characteristic is wisdom, and the third is cleverness.

Job Security

Certainly, economists are just as worried about their own job security as are others. However, the perversion of the concept of "tenure" from that of "academic freedom" to

"job security for life" has reduced this concern for some. Nonetheless, my university continues to increase the number of non-tenure-track adjuncts relative to the number of tenure-track faculty. This reduces teaching quality because most adjuncts do not have an active research program. John C. Branner, President of Stanford University from 1913–1915, was also concerned about this process. Soon after becoming President, Stanford University's Trustees asked Dr. Branner to cut expenses. Branner affirmed Stanford's desire for teaching excellence by responding, "If the scholars are to be chased away or replaced by cheap instructors, I don't want anything to do with the outfit."

On a broader scale, this is certainly an important issue. Friedman (2005) refers to people whose jobs are secure in a globalized world as "untouchables." He groups such jobs into four categories: (*a*) special, (*b*) specialized, (*c*) anchored, and (*d*) adaptable. "Special" people are those who have a global market for their unique services such as athletes and entertainers. "Specialized" people are those who have skills that cannot be digitized and outsourced to lower-wage locations. These include knowledge workers and advanced technology engineers. "Anchored" jobs are those which must be performed in a certain location and require personal contact. Most occupations are of this nature including physicians, electricians, waitresses, and mechanics. Of course, having an "anchored" job does not mean it will be a high-paying job. Many anchored jobs do not require unique skills. Therefore, wages are often low because of a lack of entry barriers. Finally, "adaptable" people are those who are constantly developing new skills and learning new techniques.

As we think about our profession, only a few can argue that they are in the "special" category. While our jobs are generally thought of as being "anchored," distance education may change this perspective. One might argue we are "specialized" in that our jobs cannot be outsourced to lower-wage locations. However, Yale University has essentially outsourced some research functions to its sister university (Fudan University) in Shanghai.

As the world becomes more globalized and trade expands, worldwide wealth will increase. The citizens of any region can only share in this growth if they are either specialized or constantly adapting to change. Are we as economists in either of those two categories? Unless we are continually improving our human capital through the discovery of new knowledge, we are not. By the way, who among us has not preached (lectured) on this very subject to our students? We continually explain the importance of adaptability, cleverness, "learning how to learn," wisdom, knowledge, analytics, and critical thinking. Certainly, the same issues are equally applicable to us.

Integrity

Watts (1989) contends it is not the purpose of the agricultural components within a university to serve as advocates for agriculture (or any other constituency). The appropriate role is to generate knowledge and educate agricultural producers, agribusiness managers, and policy makers so that agriculture can make a larger contribution to societal welfare. In this context, a researcher who is willing to place his or her work in the cauldron of the idea market, knowing it will be irreverently scrutinized by others, must necessarily be conducting research with at least some integrity and credibility. The willingness to be evaluated by peers provides our comparative advantage.

Let me provide a personal example from the beef industry. I am sure those of you working in natural resources, agricultural policy, or other commodity areas can certainly think of similar situations. During the latter part of the 1990s, many agricultural economists who study the beef industry became embroiled in a battle over the issue of U.S. imports of Canadian fed cattle. The Canadian fed cattle industry had expanded for a number of reasons, and the Canadian/U.S. Free Trade Agreement had lowered barriers to trade. These barriers were not lowered symmetrically, however, and this became a cause for concern.

A group of U.S. cattle producers decided that low U.S. cattle prices in 1996 and 1997 were caused by the dumping of cattle by Canadian producers onto the U.S. market (Brester, Marsh, and Smith, 2002). However, almost every agricultural economist who conducted research in this area agreed that the impact of trade with Canada only slightly contributed to lower U.S. cattle prices. Other market fundamentals were much more to blame. Nonetheless, a group of cattle producers pursued legal actions against Canada and generated a make-work project for lawyers and economists. The group was able to hire "noted international trade experts" to back up their claims. Interestingly, these experts were unwilling to share their "research" results with others, claiming confidentiality issues. Certainly, this is a legitimate concern. However, even after the courts settled the issue, these same economists were not willing to share their research. In addition, to the best of my knowledge, the research was never submitted for peer review.

Throughout this process, I was chastised publicly by many cattle producers for not "caring about agriculture" and not "helping the Montana cattle industry." I gave many presentations of research results developed by myself and other colleagues on this issue. Nonetheless, a number of unflattering things were said about me. At one point, one of the principals involved in the dispute held up a paper copy of Powerpoint© slides from one of my presentations during a National Public Radio interview (a colleague was present and witnessed the event), and claimed, "This work would receive an 'F' in any economics course!" Perhaps. I was somewhat surprised that, armed with the knowledge imparted on him by paid "experts," this person was seemingly able to grade my work. However, the important point is I presented my work for a "grade" by: (*a*) submitting my research for peer review, (*b*) widely distributing my results through a website, and (*c*) providing all of my research results to the very experts hired by the cattle producer group. Had the peer reviewers of my research decided the work deserved an 'F,' then I would have tried to improve my approach.

In summary, the lack of integrity by some of the "experts" in our profession was appalling. Levitt and Dubner (2005) explain the actions of so-called experts:

> ... an expert doesn't so much argue the various sides of an issue as plant his flag firmly on one side. That's because an expert whose argument reeks of restraint or nuance often doesn't get much attention. An expert must be bold if he hopes to alchemize his homespun theory into conventional wisdom. His best chance of doing so is to engage the public's emotions, for emotion is the enemy of rational argument (p. 148).

The leaders of this group of cattle producers eventually resorted to *ad hominem* chastising of my research. The important lesson, however, is that once our research was published in multiple professional outlets, *ad hominems* became the only avenue for rebuttal (Brester, Marsh, and Smith, 2002; Brester, Smith, and Marsh, 2003).

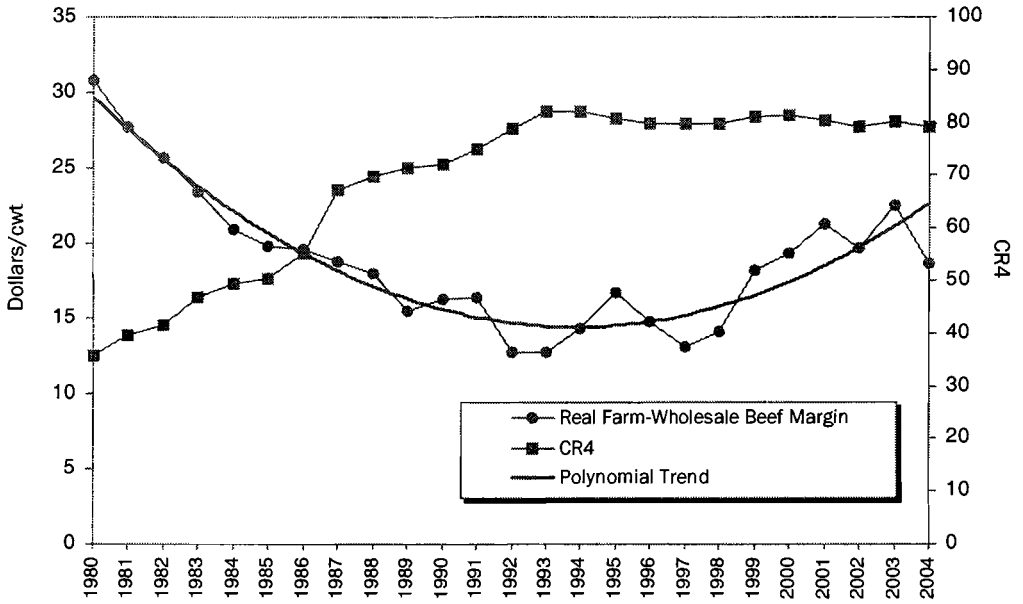


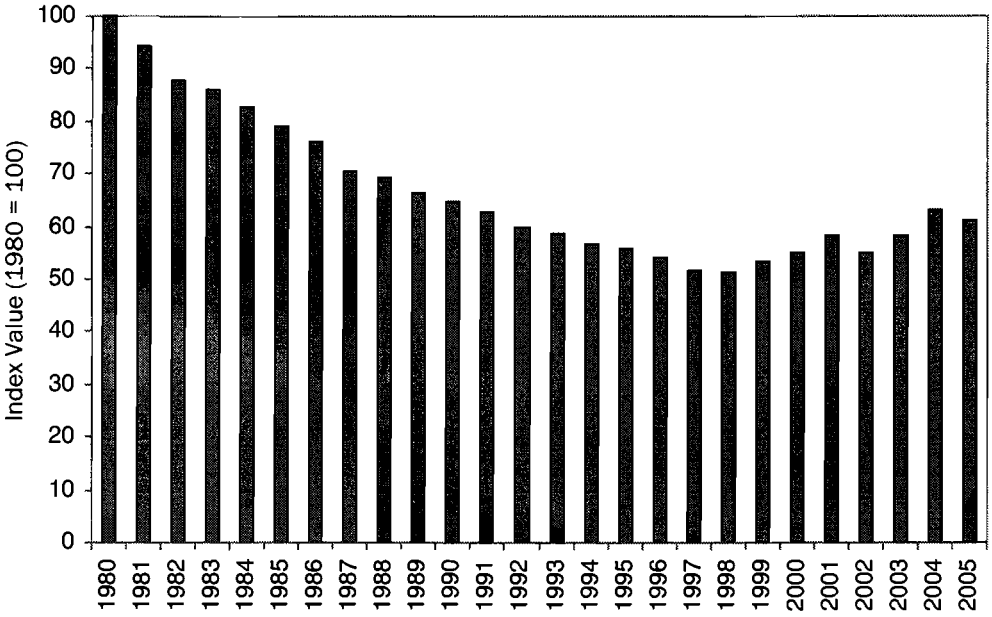
Figure 1. Real farm-wholesale beef marketing margin and four-firm packer concentration ratio, 1980–2004

Thinking versus Conventional Wisdom

A few agricultural economists have argued that recent increases in farm-to-wholesale marketing margins have clearly been caused by anti-competitive behavior in the food processing industry (Taylor, 2002). Figure 1 presents the farm-wholesale margin for the beef sector. Some suggest the farm-to-wholesale beef price spread declined from 1980 to the mid-1990s because of increased processing efficiencies. However, the same economists argue that since the mid-1990s, anti-competitive behavior has been responsible for increases in the margin. I will give my colleagues credit for at least publicly posting this idea. But, I have yet to see this research meet peer-review criteria.

I find it surprising that well-trained economists would take such a bivariate view of the world. While their conclusions could be correct, the lack of analytical rigor in the analyses is apparent. Nonetheless, the story is consistent with "conventional wisdom." John Kenneth Galbraith coined the phrase "conventional wisdom" and noted, "we associate truth with convenience" (Levitt and Dubner, 2005). Galbraith was not using the phrase as a term of endearment. Levitt and Dubner write that conventional wisdom "... tends to be simple, convenient, comfortable and comforting—though not necessarily true." However, conventional wisdom, though possibly false, often provides "... a nice place to start asking questions" (p. 89).

The conventional wisdom approach to the marketing margin issue lacks analytical rigor and represents a scientific failure. Marsh and Brester (2004) have shown that impacts of changes in marketing margins on farm-level prices critically depend upon the source of those changes. Specifically, marketing margins are an accounting residual of differences in prices between marketing levels. Those price differences exist (and change) because of the aggregate behavior of firms who provide a wide variety of marketing



Source: James Mintert, online at http://www.agmanager.info/livestock/marketing/.

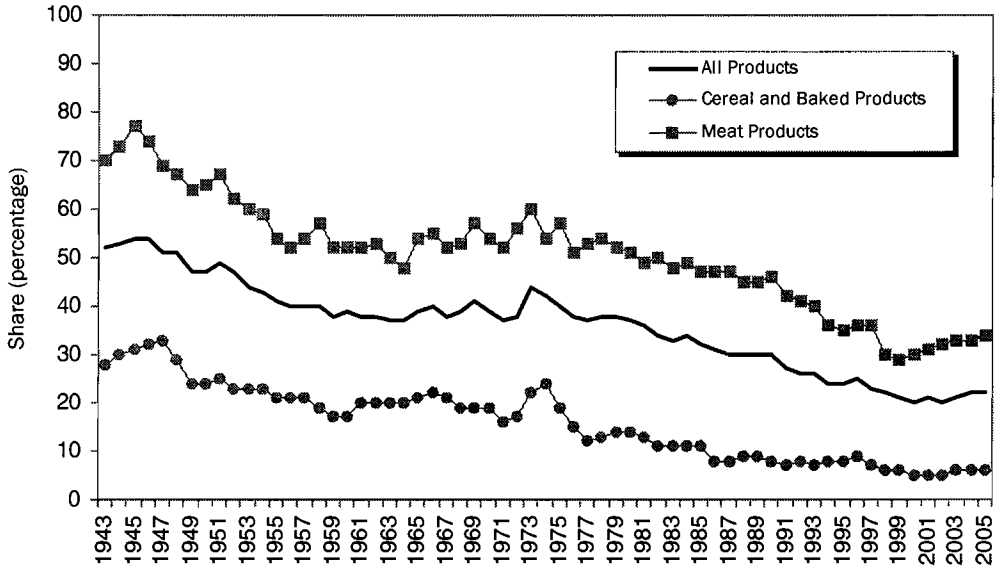
Figure 2. U.S. beef demand index, 1980–2005 (1980 = 100)

services. Hence, marketing margins are not a "thing," but rather a compilation of a broad range of economic activity. For example, figure 2 illustrates a measure of beef demand between 1980 and 2005. Note that demand increases began in the late 1990s. One could surmise an increase in retail beef demand would likely increase the demand for marketing services and lead to an increase in the price of providing those services. Just like the anti-competitive story, this hypothesis is consistent with the data reported in figure 1. However, I am curious as to why it took 15 years (from 1980 to 1995) for the packing industry to learn how to behave anti-competitively, given the four-firm concentration ratio was virtually unchanged since 1990 (figure 1). My point is that while it is easy to look at bivariate relationships, it is almost always a suboptimal approach to economic analyses.

More recently, the issue of declining "farmer's share of the retail dollar" has been cited as evidence of the existence of anti-competitive behavior virtually everywhere in the food processing industry except at the farm level. The premise that the farmer's share of the retail dollar is somehow a measure of producer welfare is, at best, weak. However, the statistic is often cited as an indicator of low returns to agricultural production.

For example, Senator Tom Harkin's opening remarks to the Hearing of the Senate Agricultural Committee on June 10, 1998, included the statement:

> Now, I am not calling for government action on this particular matter, but I just want to point out that the farmer's share of the retail dollar paid for red meat has fallen substantially. Since 1970 the farmer's share of the retail price of beef has fallen from 64 percent to 49 percent, while for pork the farmer's share has fallen from 50.5 percent to 34.8 percent.



Source: U.S. Department of Agriculture (1949, 2006).

Figure 3. Farmer's share of the retail dollar, 1943–2005

A cursory search of the internet provides hundreds of examples of the misuse of the farmer's share of the retail dollar statistic. The farmer's share of the retail dollar is, again, not a thing that can be manipulated, but an arithmetic construct that is ultimately a function of many factors. The farmer's share of the retail dollar for a commodity is calculated as:

$$FS_i = \frac{P_{fi}Q_{fi}}{P_{ri}Q_{ri}}, \tag{1}$$

where P_{fi} is the farm-level price of i, Q_{fi} is the farm-level retail-weight equivalent quantity of i (adjusted for by-product values), P_{ri} is the retail-level price of commodity i, and Q_{ri} is the retail quantity of commodity i.

Figure 3 presents the farmer's share of the retail dollar for all U.S. agricultural commodities, livestock products, and bakery and cereal products between 1943 and 2005. In each case, the farmer's share of the retail dollar has trended downward. The farmer's share of the retail dollar for bakery and other cereal products averaged 30% in the 1940s (and, using a longer time series, 54% in the 1920s) and only 6% in 2005. Yet, I defy anyone to find a person who lived on a wheat farm in the 1940s and lives on a wheat farm in 2005 who would argue that life was better in the "old days." My mother grew up on a farm in the 1940s, married my father who was a farmer, and then built a successful farming operation with him from scratch. They have only recently retired. My mother has often told me that the "good old days" on the farm might have been "old," but they were far from "good."

John Marsh, Joseph Atwood, and I are currently working on a paper that explains the data presented in figure 3. We are constructing a structural supply/demand model of the beef and pork industries. The comparative statics show that the effects of changes in the farmer's share of the retail dollar on farm-level producer surplus are ambiguous. In

addition, the impacts depend upon relative supply and demand elasticities, as well as the specific factors leading to changes in the farmer's share of the retail dollar (Gardner, 1975; Wohlgenant, 1989). In the case of beef, the farmer's share of the retail dollar has consistently declined (figure 3). As noted earlier, the increase in beef demand over the past eight years may have increased the farm-wholesale marketing margin, and it may also be responsible for reductions in the farmer's share of the retail dollar. Nonetheless, it is entirely possible that increases in beef demand have increased farm-level producer surplus.

In summary, I refer once again to Landsburg (1997), who comments on the lack of analytical thought processes:

> Intellectual complacency is the disease. Platitudes are the vectors. The habit of *irreverence* is the vaccine [emphasis added]. I hope a child ... will find it natural to laugh at intellectuals, politicians, teachers, judges, journalists, and even economists whose cultural icons have displaced their ability to wonder. Irreverence, of course, is not enough. To see through a false intellectual construct is only the first step toward replacing it with a new one.... expanding the limits of human knowledge is an honorable endeavor. And the first step is to free one's mind of the biases that stand in the way of inquiry (pp. 74–75).

The Value of Publishing

Beattie and Watts (1987) note that research results which are not committed to paper, or conveyed to resident or non-resident students, is research undone and fails to meet societal goals. In addition, McCloskey (2000) asserts:

> Economically speaking, the production function for thinking cannot be written as the sum of two sub-functions, one producing 'results' and the other 'writing them up.' The function is not separable. You do not learn the details of an argument until writing it in detail, and in writing the details you uncover flaws in the fundamentals (pp. 6–7).

But, as economists, it would almost be sinful not to ask the question, "What is the value of publishing to an individual?" I work at a public institution where salary increases have averaged about 2% annually during the past six years. Nonetheless, what is the marginal value to me, a tenured full professor, of an additional journal article? According to Golden et al. (2006), an additional *American Journal of Agricultural Economics* article is worth about a $200 increase in annual salary. In the same study, being successful at not dying during a year is worth an increase in annual salary of approximately $1,000.

Hilmer and Hilmer (2005) report that the marginal increase in salary for an additional publication in a top agricultural economics journal is 0.5%, which is about one-half the size of their estimated marginal effect of not dying during a year. Thilmany (2000) also notes that annual salaries are more highly correlated with experience than with publications. Of course, there may be some survivor bias in the results. Most of those with more than seven years of experience are still in the sample while others who did not receive tenure are not around to be counted. At the risk of displaying irreverence and uncooperativeness, I argue that these studies may also result in estimates which are biased downward because they seem to ignore potential collinearity and nonlinearities among regressors.

As economists, we understand that factors other than money are components of utility functions. Although everyone weights them differently, most economists' utility functions contain the usual seven deadly sins—prestige, ego, bragging rights, self-satisfaction, self-importance, humiliation, and shame. In addition to potential monetary awards, we publish to obtain (or avoid) some level of each.

One might also argue that personal wealth is created beyond salary considerations because of increased consulting potential. Certainly, a strong publishing record is noticed by those seeking economic expertise. Given that many universities appear unwilling to compensate faculty at levels commensurate with their value in the absence of job changes, consulting opportunities are an entrepreneurial approach to solving this problem. However, my experience is that most consulting activities reduce research output. In addition, publishing improves the likelihood of competing job offers—although most of the research noted above accounts for job changes.

Watts (1989) notes that publishing research serves three non-pecuniary purposes. First, it provides a method of communicating results to other professionals. Second, it provides a critical evaluation of competency, unbiasedness, and logical thought processes. To argue that such an evaluation is not necessary is arrogant, and just as insulting as to argue that one does not need to work hard at doing a good job of teaching because it interrupts other research/grant activities. Third, active researchers have larger incentives to read the extant literature. Failure to do so limits the scope of teaching and idea generation.

The Value of the Review Process

The purpose of communicating scientific results to other scientists is to provide an organized method of facilitating the exchange and development of creative thinking (Chubin and Hackett, 1990). Publishing provides an accessible method for others to understand the current state of a profession. The peer-review process contributes to these objectives by providing a check on procedural correctness and logical consistency, reducing search costs, and allocating scarce resources (journal pages). If it was not for the latter, then all articles could be published. It has been suggested to me that publishing all submissions would allow readers to sort the "wheat from the chaff." However, this is essentially the approach taken by opinion pages in many newspapers. What lasting relevance results from such a process?

I am somewhat comforted in my own publishing trials and tribulations by the knowledge that others have also struggled. For example, in the book *Rejected: Leading Economists Ponder the Publication Process* (Shepard, 1994), Robert Lucas reports that his 1972 paper "Expectations and the Neutrality of Money," which introduced rational expectations into discussions of economic behavior, was rejected by the *American Economic Review* in 1970. Eventually, it was published in the *Journal of Economic Theory*. George Akerlof's paper, "The Market for 'Lemons': Quality, Uncertainty, and the Market Mechanism," was rejected by three journals before being published in the *Quarterly Journal of Economics*. It is reassuring to note, at least in the context of rejection, I have something in common with these outstanding economists. However, I would be remiss if I did not confess that my initially rejected papers have been published in more modest outlets than those of Lucas and Akerloff.

In the same book, James March states:

> I recall on one occasion a referee filing a two paragraph commentary on a paper I co-authored suggesting (in the first paragraph) that the key theorem involved was trivially obvious and (in the second) that it was wrong. I thought on the whole that he ought to choose (in Shepard, 1994, p. 94).

Shepard also contends:

> The publication process is important. At its best, the process identifies the best ideas, improves them, and spreads them. At its worst, it suppresses creative new thought, and maintains erring orthodoxy. For better or worse, the process is the marketplace of ideas. With or without market failure, it determines the path of economic thought—and controls economists' careers (p. 124).

Problems with the Process

We must recognize that the peer review process is not perfect. Samuelson argues that because publishing controls economists' careers, the process does not create the best science. He notes:

> People say 'quality not quantity counts.' Alas, not so. I have been surprised to observe how much three mediocre publications can do for a budding career (in Shepard, 1994, p. 146).

Samuelson even questions the use of the publishing process to evaluate faculty because "deans cannot read anyway and neither can their appointment committees" (in Shepard, p. 136).

According to Chubin and Hackett (1990), a survey of members of the Scientific Research Society indicated "only 8% agreed that peer review works well..." (p. 122). Furthermore:

> Today peer review is besieged on both practical and symbolic grounds. In their complaints, critics point to the operating characteristics of peer review: low level of consensus among reviewers, inconsistencies of judgment, errors of omission (when a flawed or fraudulent manuscript slips through) and commission (when a competitor's manuscript is blocked or delayed, or its results or arguments are stolen), the partisan flavor of reviewer comments (which seemingly violates principles of impartiality), and the unsettling influence of authors' characteristics on the fate of their manuscripts. These are neither a blueprint for selecting the best science nor an enactment of the values we hope science will honor (p. 122).

David Kaplan (1995) states, "Despite its importance as the ultimate gatekeeper of scientific publication and funding, peer review is known to engender bias, incompetence, excessive expense, ineffectiveness, and corruption" (p. 10). John Zinman (1982), former editor of *Science Progress*, asserts: "The peer-review process seems not merely imperfect: it is entirely useless, if not positively harmful activity, based upon quite erroneous assumptions" (pp. 245–246).

Ann Weller (2001) examined more than 200 studies that had undergone a peer-review process. She concluded:

> Peer review's outstanding weaknesses is that errors of judgment, either unintentional or intentional, are sometimes made. Asking someone to volunteer personal time evaluating the work of another, possibly a competitor, by its very nature invites a host of potential problems, anywhere from holding a manuscript and not reviewing it to a careless review to fraudulent behavior (p. 308).

However, Weller continues, "like Democracy, editorial peer review is messy and does not always work as it should, but it is essential to the integrity of scientific and scholarly communications."

The subject seems to be especially sensitive and relevant to the medical field. The *Journal of the American Medical Association* has frequent contributions on the subject. The process of obtaining additional grant funding based upon research results certainly raises questions. The issue has become so controversial, that an entire conference (The First International Symposium on Knowledge, Communication, and Peer Reviewing) devoted to peer-review issues was held in July 2006.

Although the process is far from perfect, perhaps part of the problem is the assumption that the peer review process somehow results in the discovery of truth. We should admonish ourselves for such pretentiousness. The process is designed to meet the much more modest goals of advancing knowledge, evaluating the plausibility of ideas, documenting scientific activity, assigning property rights to ideas, and evaluating competency. Ginsparg (2003) expands on this:

> Outsiders to the system are sometimes surprised to learn that peer-reviewed journals do not certify correctness of research results. Their somewhat weaker evaluation is that an article is (a) not obviously wrong or incomplete, and (b) is potentially of interest to readers in the field. The peer review process is also not designed to detect fraud, or plagiarism, nor a number of associated problems—those are all left to posterity to correct. In many fields, journal publication dates are also used to stake intellectual property rights (indeed their original defining function) (p. 2).

A Market-Based Alternative?

My colleague Joseph Atwood alerted me to changes occurring in the physical sciences regarding review processes. The changes are designed to reduce problems associated with traditional peer reviews. He has termed it a "market-based" review process. It essentially follows Thomas Friedman's (2005) discussion of the development of "freeware" or "open-source" software. Some elements of the LINUX operation system have been developed in this way. The statistical package "R" and the online encyclopedia "Wikipedia" (which is accessed more frequently than the online version of the *Encyclopedia Brittanica*) have also been developed by an open-source network. The open-source process involves placing software (or encyclopedia entries) on websites, and allowing others to modify the code (or entry). Some monitoring is required, but most is done by diffused volunteers and users rather than any single entity.

The benefits of such a system are manifest in the speed and collaboration it fosters. Improvements are offered and bugs are quickly fixed. However, one has to wonder if such systems are sustainable and if innovation can be created without associated assignments of property rights. Nonetheless, the development of the internet provides an amazing example of innovation in the absence of property rights and profit motives (Warsh, 2006).

Peer-review processes could be conducted in a similar fashion. For example, some proceedings publications already use this process as a means for improving the speed of innovation. The process involves placing manuscripts on a website, and inviting comments. Good ideas are not stolen, rather they can be referenced immediately. Comments on the manuscripts are posted. The Cornell University Library hosts the arXiv.org website. The site was started in 1991, and is an electronic archive for research papers in physics, mathematics, computer science, and quantitative biology. The site has a current archive of 372,755 e-prints, and approximately 4,000 submissions are received each month. In a randomly selected 12-hour span on June 21, 2006, a total of 237,538 connections were reported. The site is used to provide a pre-review dissemination of ideas. The American Mathematical Society publishes *Mathematical Reviews*. Over 100,000 articles are posted per year, and approximately one-half are eventually reviewed. Ginsparg (2003) argues that this reduces the costs of formal reviews. However, this ignores the opportunity costs of users searching through this broad set of papers.

In many academic professions including our own, working papers have a long history of serving as a conduit for comments and ideas prior to journal submission. More recently, many authors post working papers with accompanying copyright dates. This encourages discussion and assigns property rights to ideas.

As mathematical and statistical complexities increase, editors report that it becomes increasingly difficult to find qualified reviewers within our discipline. A market-based review process would allow access to those who have a genuine interest in the paper, expand the reviewer base within and outside the discipline, and reduce some of the problems associated with blind peer-review processes. Advancements in information technology make such a review process feasible. Perhaps a future *Journal of Agricultural and Resource Economics* editorial team may propose such a review process for a specific group of submitted papers? Papers deemed as primarily being quantitative in nature seem to be reasonable candidates for such an experiment.

Summary

When I began preparing this address, I thought I might have stumbled onto an issue that was "new." After reviewing the literature, I found that the issue of research and publishing has been widely discussed. In fact, I am not even the only President of an agricultural economics association to address the topic this year! In her Southern Agricultural Economics Association (SAEA) Presidential Address, Damona Doye (2006) discusses the importance of scholarship to agricultural economics extension and the role of peer-reviewed publications. And, in receiving a 2006 Lifetime Achievement Award from the SAEA, Gerald Doeksen (2006) offers eight suggestions for rural development Extension faculty to remain effective. Two of those are "concentrate on doing what economists do" and "publish, publish, publish" (pp. 238–239).

Nonetheless, after visiting with many of the authors of earlier papers on the subject, I was told that it is valuable to remind our profession of research obligations and benefits from time to time. Given the speed of changes in information technology, perhaps this is a perfect time for us to reflect on our professional activities and responsibilities. In addition, it is incumbent on us to broadcast the reasons for conducting research to a wider audience including administrators, politicians, and taxpayers.

For some of my peers, my suggestion for renewed research emphasis represents somewhat of a change. For them, I point out that General Eric Shinseki, Chief of Staff for the U.S. Army (1999–2003) said, "If you don't like change, you are going to like irrelevance even less" (Fast Company, 2004).

In summary, regardless of our appointments or position, we need to continue to conduct and publish research because such efforts:

- build human capital,
- make us more valuable professionally and personally,
- improve our teaching,
- contribute to societal improvements, and
- provide a limiting factor in the production and proliferation of nonsense.

We should, of course, try to obtain an optimal amount of Type I and Type II errors in our publishing processes. Note that I argue for an optimal amount of these errors because the complete elimination of both is not possible. If we decide to publish all papers, then Type II errors occur. If we decide to publish no papers, then Type I errors occur. Finally, the research and publishing process, while not always resulting in increased personal wealth, certainly enriches resident students, non-resident students, society, and in one way or another, ourselves.

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