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2016 WAEA Presidential Address: Comments on Agricultural Economics Research

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Agricultural economics research has changed over my career. Articles have gotten longer, with more equations, tables, and figures. We now use maximum likelihood almost as much as least squares. I use this talk as an opportunity to discuss the strengths of our research as well as areas where we could improve. I also make a few suggestions about directions of future applied econometric research.

Key words: econometrics, research methodology, research metrics

Introduction

"The problem with science is that so much of it simply isn't."
William A. Wilson
"Scientific Regress"
First Things, May 2016

Writers in the popular press are questioning the money spent on academic research (Vedder, 2016a). Paying back student loans is a problem for many young people today (Denhart, 2013), and there is a concern that tuition dollars are being used to subsidize research of questionable value (Bauerlein, 2009). For example, Gobry (2016a,b) has written a series of articles with titles like "Big Science Is Broken" and "Why So Many Scientists Are So Ignorant."

As economists we should call for reductions in our own budgets if the value of the research we do does not exceed its cost. Given the questions about the usefulness of research, it seems appropriate to look at our own research in agricultural economics. I am going to focus largely on the use of applied econometrics in agricultural economics since that is my interest. I will discuss both things that I think we are doing well and places where I think we can improve.

I begin with a comparison of journal articles in 1983, the year I got my PhD, and articles in 2015. I then use this as a springboard to say some things I want to say. Some of them fit together and some do not, but that is the beauty of giving a talk like this. I do not have to follow all the rules.

Historical Changes in Articles in Agricultural Economics Journals

Table 1 compares articles in the *Western Journal of Agricultural Economics* (*WJAE*) in 1983 and in the *Journal of Agricultural and Resource Economics* (*JARE*) in 2015. Table 1 shows that our papers have gotten longer. We now have more pages, more equations, more tables, and especially

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Table 1. Changes in WAEA Journal Articles over Time (Medians)

Category	1983	2015
Pages	12	19
Equations	6	9.5
References	20.5	35.5
Tables	4	5.5
Figures	0	4

Table 2. *AJAE* Word Search

Key Words	1976-1985	2006-2015
Items	3,110	1,410
Econometric*	535	703
Robust	71	416
Probit or logit	55	282
Least squares	296	279
Maximum likelihood	88	224
Simultaneous	215	193
Hesteros*edasticity	13	163
Monte Carlo	49	150
Autocorrelation	66	120
Cointegration	0	46
Mathematical programming	58	26
Spatial autocorrelation	0	25
Differences in differences	0	12

Notes: Based on Google Scholar.

more figures. I did a similar analysis for the *Journal of Agricultural and Applied Economics*, and it shows the same pattern, so this is not a result of our current editors. Ellison (2002) has found this phenomenon in economics journals.

What has caused the increased length? In 1983, many people were still using a typewriter. As word processing technology has improved, the costs of producing figures, equations, and tables has been reduced. As research productivity has increased, acceptance rates have decreased and reviewers may select the longer papers. Also, the culture has developed to where reviewers often demand changes that increase length. Other disciplines have strict length limits, but we do not. When I was *AJAE* editor, we had a statement in our submission guidelines that papers should not exceed thirty manuscript pages. The *Journal of Agricultural and Applied Economics* still has such a statement. In practice, I sent papers back to the authors when they exceeded fifty pages. Using a rule of thumb of 2.4 manuscript pages per journal page, the median *JARE* article is forty-four manuscript pages. The shortest article in the 2015 *JARE* (Fausti et al., 2015) was twelve pages. By my estimate, it was the only article that might have been less than thirty manuscript pages. Thirty pages has now become a minimum rather than a maximum.

Table 2 contains the frequency of articles containing various phrases in the *American Journal of Agricultural Economics* (*AJAE*). The number of items in the *AJAE* is now about half of what it used to be. Like other journals, the *AJAE* has changed to reduce the denominator used to calculate its impact factor. The *AJAE* has pushed information about meetings, editor's reports, Fellows, etc. to other places. The *AJAE* used to publish a notes section and invited papers from the annual meeting. The invited papers were subject to a seventeen-page manuscript limit and the notes were even shorter. Comments and replies used to be more frequent than they are now. Invited papers sort of still exist in the *AJAE*, but they have largely died in *JARE* and *JAAE*. If publication cannot be guaranteed, then people do not want to do them. The increase in manuscript length may have also

Table 3. Econometric Methods Used in WAEA Journals

WJAE 1983	*JARE* 2015
Descriptive statistics (2)	Quantile regression (2)
Ordinary least squares (2)	Zero-inflated Poisson
Nonlinear least squares	Nonlinear 3SLS
Probit	Ordered probit
Logit	Fractional logit
Box-Cox	MIMC logit
Iterative SUR	GARCH-M
EGLS	Asymmetric VAR

Table 4. Econometrics Articles in WAEA Journals

Category	1983	2015
Econometrics	50%	67%
Observations	48	1,814
Primary data	44%	44%
Misspecification tests	30%	25%
Robust	0	1
Bootstrap	0	3

reduced the number of papers published. So, apparently the marginal value from another page in an accepted article is higher than the marginal value from a page on a new topic. What happened to the law of diminishing marginal returns?

The use of econometrics terms has gone up (table 2). "Maximum likelihood" is now almost as common as "least squares." The use of "probit/logit," "robust," and "heteroskedasticity" have gone way up. Clearly econometrics is the primary tool of our research. Some tools—such as cointegration, spatial autocorrelation, and differences in differences—did not appear in the early period, but all of these have modest usage more recently.

Table 3 shows that the econometric tools we use have changed. When I graduated in 1983, I knew how to use all of the tools used in the 1983 *WJAE* articles. Students graduating with a PhD today are probably not going to know how to use all of the tools used in 2015 *JARE* articles. I know that I do not know all of them. The changes in tools could result from advances in knowledge, advances in econometrics software that makes the methods easy to use, or changes in the research that we do. Much of the research published in 1983 dealt with structural models and aggregate data, such as estimating supply and demand elasticities. The 2015 articles are harder to categorize. Certainly, the complexity of our econometric methods has gone up.

Table 4 has some additional statistics comparing *WJAE*/*JARE* articles from 1983 and 2015. The table shows that the portion of econometrics articles has gone up from half to about two-thirds. The most striking number in table 4 is the increase in number of observations. Since many of our methods are only asymptotic, the increase in number of observations is a good thing. Increased observations reduce the need for data mining (also called pretesting, forking paths, p-hacking, etc.) in the hunt for the magical p-value of less than 0.05. In 1983, Leamer (1983) published his classic article "Let's Take the Con Out of Econometrics," and so it appears that researchers can now select topics that do not require as much data mining. Other results in table 4 show little change in the use of misspecification tests or primary data.

Comments on Agricultural Economics Research

As we can see, there have been changes in the way we do research. What can we learn from this? I will now provide a few comments on our research.

We Are Strong in Theoretical Rigor and Econometric Precision

A strength of our research is its theoretical rigor and econometric precision. Being careful with statistics is an important step toward advancement of knowledge. Other disciplines without this rigor—like management and education—typically do not produce much advancement in knowledge. Part of the increased length is due to larger theory sections. As a general rule, econometrics goes better with theory, so it is hard to argue too much against this increased rigor. I find it difficult to assess which papers have led to advancement of knowledge, but at least we are giving our research a chance. While we might have gone a little overboard on rigor and precision, it is certainly not something that we want to lose.

Our Papers Are Too Long

One thing that stands out in table 1 is the increase in the length of manuscripts. The increased length is not all bad—since it can sometimes mean more robustness checks and more detail that will help a few readers—but the increased length can also be costly. Not necessarily to authors; as the saying attributed to Blaise Pascal goes: "I didn't have time to write a short letter so I wrote a long one instead." The cost is on the reader because it takes more time to read the paper. The cost can also be on the science since the paper may not be read if it is too long. I advocate twenty manuscript pages of text as a target. I select this length because it is roughly my own attention span. Anything much longer and I am not going to read it. In addition, we have become one dimensional. We need a place where ideas that do not need forty-four pages to describe can be published. There is an opportunity for a letters journal in agricultural economics. It is debatable whether or not the WAEA is the right association to start such a journal, but there would be a market for such a journal.

We Need an Alternative to Impact Factors

Some of the changes in our journals are caused by a desire to increase impact factors. We know from economic theory that incentives matter. If journals and authors are ranked based on impact factors, then they will do things to increase them. We also know that incentives can create unintended consequences. One effect of the emphasis on impact factors is the death of short papers, but it can also create incentives to change the research that we do and the research that is accepted in journals. Eleven of the top fifteen most cited papers in *JARE* are a variation of willingness-to-pay/consumer-preference studies. If we want to maximize impact factors, then maybe we should only publish willingness-to-pay studies.

The weaknesses of impact factors as a measure of research contribution are widely recognized (Seglen, 1997; Smeyers and Burbules, 2011), but they are still used. Most of the criticisms are about using impact factors to evaluate individuals, but the incentive problems still occur in using impact factors to rank journals.

Vedder (2016b) argues, "A core economic principle is the law of diminishing returnsâĂę that applies—with a vengeance—to academic research." Impact factors go against the law of diminishing returns. If you want to be cited a lot then you need to do research in the same area that a lot of other people are doing research. So impact factors can create the wrong incentives.

What would an alternative be? An H-index such as Google Scholar now provides does not penalize journals for publishing shorter articles— such as comments and replies—that get fewer cites. Citations per page would reduce the bias toward longer articles but would create an incentive for bigger pages. None of these are really satisfactory. Why do we have department heads determine our merit pay using subjective methods rather than a formula that everybody knows? Herrmann et al. (2011) argue for using a survey as an alternative to impact factors. Rigby, Burton, and Lusk (2015) survey agricultural economists and find little correlation between journal reputation and

impact factors. If we need an alternative to impact factors, then our professional associations are perhaps in the best position to produce one.

We Use Maximum Likelihood More Than Generalized Method of Moments

Generalized method of moments (GMM) estimators such as White's (1980) heteroskedasticity consistent covariance matrix, Newey and West (1986), and cluster robust standard errors are becoming the standard in economics and finance, but that is not yet true in agricultural economics. Let's keep things the way they are in agricultural economics.

GMM estimators have a place in our set of tools. GMM methods are well known to underestimate standard errors in small samples, but this can be overcome with bias corrections. As McCloskey and Ziliak (1996) argue, estimation of parameters—and not significance tests—is our objective. Nester (1996) argues that all null hypotheses are false anyway. Significance tests are merely a way of seeing whether we have enough data that we have the right sign on our parameter. Asterisk econometrics (writing this coefficient has an asterisk and this one does not) is common among my students, and McShane and Gal (2015) argue that it is common among researchers in general. GMM methods are only consistent. Correcting for heteroskedasticity using maximum likelihood can give parameter estimates that are asymptotically efficient. The gain in efficiency is worth the risk of any misspecification bias due to misspecifying the form of heteroskedasticity.

Freakonomics/Differences in Differences

Economics has gone through fads, such as the rational expectations fad and the cointegration fad. One current fad is to do research using large cross-sectional datasets and conduct studies like those described in the book *Freakonomics* (Levitt and Dubner, 2005). The estimation method is often differences in differences. Such work proposes a challenge to the rigor of our discipline.

We have had a few papers like this published in our discipline already (e.g., Sneeringer, 2009; Whitacre and Manlove, 2016; Qian et al., 2016), and even more are coming. They typically have vague theory and large sample sizes, address important problems, and reach strong causal conclusions. The problem is that too often they reach conclusions that are hard to believe. It is not so much that I really think the effect is zero, but that it is so small that it should be impossible to find in the noise. This type of work often has many forking paths, such as alternative ways to do matching. Just as in other areas of econometrics, there is an incentive to follow the path that gives the desired result. Another drawback of this work is that it can be difficult to handle endogeneity. Once you leave the world of supply and demand, instruments can be hard to find (Larzelere and Cox, 2013). We are going to accept some work like this because of the importance of the problems addressed, but we need to view such work with caution.

Note that there is a set of current research that attempts to provide identification strategies. Randomized controlled trials (RCT) are being increasingly used in international development. Opportunities to register pre-analysis plans are occasionally being used. Regression discontinuity designs can sometimes offer identification. There is an opportunity for us to continue to do rigorous research, even if we move toward *Freakonomics*-style topics.

Simplicity Is Undervalued

The papers published in 1983 are easier to understand than those published in 2015. I recently had a discussion with one of my students about the goal of writing. He said that the goal was to impress. In particular, to impress people with the technical difficulty and precision of the work. I argued that the goal was to be understood: you have done something useful and you want to communicate it to others. I may be idealistic, but I am not going to change.

There is hope for change. The plus side of the *Freakonomics* fad is that parts of economics are changing to a more storytelling style based on empirical work as opposed to a modeling framework. While I dislike impact factors, they may be partly responsible for a move to care a little more about the issue investigated than the precision of the answer. There are hints that we are already moving away from the precise modeling frameworks that have dominated past research.

Spatial Autocorrelation

As spatial data become easier to use, we can expect more people to use it. The econometric focus so far has dealt primarily with spatial autocorrelation. Following Anselin, Bongiovanni, and Lowenberg-DeBoer (2004), the focus has been on spatial lag and spatial error models. In my experience, spatial error is most common. Correcting for spatial autocorrelation may reduce t-values 5–10% and has little effect on the coefficients. So most of the time, spatial autocorrelation is not worth considering. There are some major exceptions, such as when the spatial lag model is appropriate. Another example is when the effects of interest are due to state policies; using data from Census districts may appear to add observations, but in this case spatial autocorrelation tests would show that—in effect—there are not as many observations as it appears. We do not yet always impose spatial autocorrelation corrections on each other and we should keep it that way. My hope is that the future of spatial analysis will focus more on the use of spatial data or on Bayesian Kriging (Park, Brorsen, and Harri, 2016) than on spatial autocorrelation.

Time Series Methods

We see an occasional paper published in agricultural economics using time series tools such as vector autoregressions and error correction models (e.g., Beckman and Riche, 2015). Time series methods currently play a minor role in agricultural economics, and that is probably how it should be. The possibility of spurious correlations limits what we can learn from time series models. Time series models are reduced-form models, which limits what we can learn from them. There is work going on with structural vector autoregression models (Pozo, Bejan, and Tejeda, 2016) and directed acyclic graphs (Shiva, Bessler, and McCarl, 2014). Currently such models are not widely accepted due to tenuous identification assumptions. We see an occasional paper that has a new time series method. In the 2015 *JARE*, Ahn and Lee (2015) use time series methods to study asymmetry of price transmission and Malone and Lusk (2016) use them as a component of their analysis. Generally, time series methods are used to study short-term movements of prices when the data necessary to estimate a structural model are not available. Time series methods have proven useful in forecasting, but our profession seems more interested in explaining than in forecasting. While the goals of science are often said to be to both explain and predict, Shmueli (2010, p. 289) argues that "in many disciplines there is near-exclusive use of statistical modeling for causal explanation," which offers some support for my opinion that agricultural economists also prefer explanation over prediction.

Data Science

One thing that has not shown up much in our journals is data science/machine learning. Such methods include neural networks, random forests, boosting, Boltzmann machines, etc. These methods skip the theoretical modeling that has been a staple of our research. The objective of machine learning is to predict. Science has traditionally put more emphasis on explanation than prediction (Shmueli, 2010). I know that my own papers on prediction tend to generate little enthusiasm from agricultural economics reviewers. It remains to be seen whether agricultural economics journals will accept much machine learning. Machine learning, however, does provide excellent opportunities for our students who want to work in industry. Varian (2014, p. 3) says "my standard advice to graduate students these days is go to the computer science department and take a

class in machine learning." The machine learning approach is fine when an infinite amount of data is available, but when the data are finite there are gains to be made from pruning the set of inputs and transforming the data. There are also issues about what to predict. The mistakes of asking the wrong question or choosing the wrong dependent variable can still be made in machine learning. Agricultural economists have training that makes them potentially valuable as part of a machine learning team. For example, our PhD students who have gone into industry say that someone trained in marketing will seek to maximize sales and an engineer will seek to maximize output subject to a set of constraints, but an economist well seek to maximize profit.

Conclusion

Most of our work is rigorous and has the potential to help advance knowledge. There are budget pressures that could result in research funding cuts if our research does not deliver on increasing social welfare. As research becomes more empirical, agricultural economists are in an excellent position to deliver research with real world impact. Careful econometrics work is a key part of achieving the goal of delivering useful research.

A major change seen in our research is an increase in the number of observations. This has reduced the need to use data mining to achieve the magical 0.05 p-value. Our papers have gotten longer and the methods have gotten more complex. We have seen many new econometric techniques adopted.

I have suggested several changes that our profession needs to make, such as reducing the length of manuscripts and reducing complexity. I am not the first to make such suggestions. The reviewers and editors are us. If we want to change what we value, we can.

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Academic journal article Journal of Agricultural and Resource Economics. 2016 WAEA Presidential Address: Comments on Agricultural Economics Research. By Brorsen, B. Wade. Read preview. Academic journal article Journal of Agricultural and Resource Economics.Â As economists we should call for reductions in our own budgets if the value of the research we do does not exceed its cost. Given the questions about the usefulness of research, it seems appropriate to look at our own research in agricultural economics. I am going to focus largely on the use of applied econometrics in agricultural economics since that is my interest. I will discuss both things that I think we are doing well and places where I think we can improve. Innovation, Agricultural Productivity and Sustainability in the United States. TAD/CA/APM/WP(2016)15/REV1, Organisation for Economic Cooperation and Development (OECD), Paris, France, 2016. â€œThe Evolving Institutional Structure of Public and Private Agricultural Research.â€ Keith Fuglie and Andrew Toole, American Journal of Agricultural Economics 96(3): 862-883, 2014. â€œThe Growing Role of the Private Sector in Agricultural Research and Development World-wide.â€ Keith Fuglie, Global Food Security 10: 29-38, 2016. â€œLong-Run and Global R&D Funding Trajectories: The U.S. Farm Bill in a Changing WAEA presidential address deciding where to publish: Some observations on journal impact factor and article influence score, Journal of Agricultural and Resource Economics 37, 335-348. Priem, J. (2013). Scholarship: Beyond the paper, Nature 495, 437-440. Purcell, W.D. and Hudson, W.T. (2003). Risk sharing and compensation guides for managers and members of vertical beef alliances, Review of Agricultural Economics 25, 44-65. Serra, T., Zilberman, D. and Gil, J.M. (2008). Differential uncertainties and risk attitudes between conventional and organic producers: the case of Spanish arable crop far