Agricultural Policy Analysis: The Good, The Bad, and The Ugly*

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Legislators will never be economists, and they will always work on economic theory of one kind or another. They will quote and apply such principles as seem to serve their turn …. Let us suppose there were a recognised body of economic doctrine the truth and relevancy of which perpetually revealed itself to all. Economics might even then be no more than a feeble barrier against passion, and might afford but a feeble light to guide honest enthusiasm … and the roughly understood dicta bandied about in the name of Political Economy would at any rate stand in some relation to truth and to experience, … instead of being … a mere armoury of consecrated paradoxes that cannot be understood because they are not true. Excerpt from P.H. Wicksteed, Presidential Address to Section F of the Royal Economic Society, Birmingham *Economic Journal* (1914).

Agriculture is one of the most protected and regulated industries in the modern global economy. In most cases, at least in developed economies, protection largely serves to transfer economic welfare from taxpayers to producers, and to a certain extent, to consumers. The agricultural economics profession has a very long history of undertaking empirical analysis of these policies. Such analysis, on rare occasions, may even guide or modify the form, direction, and scope of the policy. It would be naive to suggest that policymakers are uninformed about the impacts and beneficiaries of agricultural policies in the absence of such analysis. Likewise, casual observation certainly suggests that the leverage exerted by empirical research in the policy formation process is minimal in most cases. However, this is not always true and applied economists have an ethical obligation to provide our own insights about the effects of policies, even if we ourselves are the primary consumers of such research.

In this address, I want to focus on certain aspects of policy analysis that I believe merit contemplation by applied economists working within the empirical realm. My coverage of topics is neither comprehensive nor representative of the most important policy issues, since importance is determined to a large degree by opinion. The opinions presented here are my own and have been garnered over several years of consuming the outstanding work of my professional colleagues, teachers, collaborators, and students. They also reflect my own particular interests. I have chosen to characterize this body of work in terms of "The good, the bad, and the ugly." The title of that iconic movie (one of the best ever made in my opinion) has become a modern colloquialism defined by Wikepedia as "An idiomatic expression … used when describing upsides, downsides and the parts that could, or should have been done better, but were not." Though perhaps overused, the expression seems particularly well-suited for describing the current state of empirical policy analysis. A wealth of such analysis exists and more is produced every day. However, among the good is also bad and, in some cases, an ugly side to our research often lurks beneath the surface.

There are three fundamental points that I hope to address in this brief essay. First, an essential role exists for applied policy analysis

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I have benefitted from extensive discussions with numerous friends, colleagues, teachers, and students. I am particularly indebted to Andy Barkley, Ron Gallant, Matt Holt, Nick Piggott, Vince Smith, Ian Sheldon, Dan Sumner, Jim Vercammen, and seminar participants at the Ohio State University, at the 2014 International Agricultural Risk, Finance, and Insurance Conference, and at the 2014 SCC-76 Conference on Risk. Any remaining errors are of course my responsibility.

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in agricultural economics. The earliest empirical analyses largely addressed agricultural economics topics. Our research is often contradictory and is always wrong in some sense. That is, our empirical depictions of the natural world are always abstract representations of reality that are based upon a number of stated or unstated assumptions and maintained hypotheses. Our research may have little traction with most policymakers, though as I point out, there are certainly important examples where empirical research plays a very important role in shaping and directing policy (and, in the process, in the allocation of billions of dollars in taxpayer outlays). Second, I believe that we often stop short of providing important empirical estimates that play a critical role in the interpretation of our research. Policy positions are often expressed with the utmost degree of confidence, even when such positions are based upon empirical estimates that may be predicated upon faulty or incomplete inference or, in some cases, on no inference at all.

Finally, I have become increasingly concerned with the roles that dogma, narrow ideology, and an arrogance toward opposing views have assumed in our discipline. The famous statistician George E.P. Box is often quoted for having stated that “All models are wrong, but some are useful.” He also stated that: “Since all models are wrong the scientist cannot obtain a ‘correct’ one by excessive elaboration. On the contrary following William of Occam he should seek an economical description of natural phenomena. Just as the ability to devise simple but evocative models is the signature of the great scientist so overelaboration and overparameterization is often the mark of mediocrity,” (Box 1976).

I hope to make this address forward-looking rather than retrospective in nature. To this end, I combine topics that many may see as only loosely connected. This may indeed reflect my own apophenic tendencies, though the connections are apparent to me and indicate great potential for policy analysis and improvement through the application of newly-developed econometric techniques.

I attempt to illustrate these points with examples from contemporary agricultural policy analysis. In particular, I draw upon the federal crop insurance program, which has become the primary mechanism of support to agricultural producers in the United States and is increasing in prominence as a policy instrument throughout the world. The federal crop insurance program is an example of a situation where data and proper empirical analysis can actually be used to shape and direct policy. This is largely due to a progressively-minded agency (the Risk Management Agency) and a reliance on empirical analysis (warts and all) in setting policy parameters. I also hope to make the point that we often stop short of recognizing the fragility of our empirical estimates and in communicating their potential shortcomings. This includes reporting and discussing such basic things as confidence intervals, pre-test estimation, and a clear accounting of implicit assumptions.

Finally, I have chosen the ongoing debate over identification and “quasi-natural experiments” as a case where narrow thinking and a dismissive attitude toward alternative approaches has, in my opinion, damaged the progress of research. The points often made in this debate are fundamentally sound and largely unassailable. However, the absolute confidence with which they are sometimes expressed is not. My basic conclusion is that there is ample room for divergent approaches toward empirical research. No single method is absolutely preferred to any other since all have strengths and shortcomings. Progress in our profession is largely based upon criticism from our peers and colleagues. However, the absolute opinions that sometimes underlie such criticisms can impede the progress of knowledge by dismissing large bodies of research based upon alternative approaches.

The Good

Like so many other things, the simple fact is that our policy analysis toolkit is growing at an exponential rate. So much of this is driven by the astounding increases in computational power, techniques, and data availability and storage. “Big data” continues to grow in prominence and the sources and use of huge data sets will undoubtedly have important applications to policy analysis. Rapid technological developments in remote sensing, precision agriculture, and passive data collection methods are occurring at a rapid pace and we are on the cusp of having a whole new realm of information for evaluating policy issues. Policymakers are also certainly doing their part in providing a never-ending and constantly changing set of policy questions that merit analysis. Leontief (1971) noted that "The spectacular advances in computer technology increased the economists' potential ability to make effective analytical use of large sets of detailed data." One can only wonder what he would think of today's computational resources, which are many orders of magnitude greater than what was available in 1971.

George Stigler (1976) wrote that "Economists exert a minor and scarcely detectable influence on the societies in which they live." Blinder (2006) noted that "The market for economic advice is one in which supply most emphatically does not create its own demand," and that "Politics regularly makes a hash out of economic policy." Thus, one must often conclude that empirical policy analysis largely serves our own, narrow interests within the profession. We produce for each other and our discoveries rarely result in actual changes to policy. I would argue, however, that this is not always true in agricultural policy analysis. I have always been puzzled by the fact that, as government subsidy programs go, the farm bill represents one of the biggest and most interesting public policies and yet seems of limited interest outside of our own narrow professional sphere of influence. A glance at any citation index shows many empirical papers evaluating programs that are minor when compared to the trillion dollar omnibus farm legislation.

Agricultural policy developments are occurring in areas that are particularly amenable to and dependent on empirical analysis. Crop insurance, which depends upon empirical analysis for determining such important policy parameters as premium rates, expected yields, levels of coverage, and so forth, was further expanded by the 2014 Farm Bill and now is the primary farm safety net and subsidy program. This included the authorization of "shallow loss" programs that further reduce insurance deductibles by raising coverage levels. The leadership of the Risk Management Agency that administers this program has actively reached out to empirical analysts in academia, industry, and in other agencies to assist in developing new techniques that can improve the accuracy of insurance programs while reducing taxpayer costs. As I discuss in greater detail below, the extent to which most agricultural policy is actually guided or dependent upon the output of empirical research is an open question. However, there is little doubt that the federal crop insurance program has certainly been shaped by our profession.

Between 2009 and 2013, federal costs associated with the program averaged \$8.4 billion each year, and the Congressional Budget Office estimates the ten year cost of crop insurance in the 2014 Farm Bill to exceed \$84 billion. With such a significant allocation of taxpayer resources, I am reminded of Ronald Coase's observation that "… if an economist can delay by a week the adoption of a policy that will decrease national income by a present value of \$100 million—and that is such a small policy!—he will have saved society twice his lifetime salary, and his teaching services will have been thrown in for free," Stigler (1976). This is a self-serving view, but such huge budgetary allocations on programs that demand empirical analysis certainly provides ample scope and opportunity for our own empirical analysis to generate significant societal welfare gains.

This does, however, raise a related point of controversy and dilemma. Most agricultural economists are quick to acknowledge the inefficiencies, distortions, and welfare losses associated with farm programs. Stigler (1976) noted, with a trace of sarcasm, that "Evidence of professional integrity of the economist is the fact that it is *not* possible to enlist good economists to defend protectionist programs." The same can be said of "good agricultural economists" and farm policy. Yet, many of us (this author included) undertake empirical policy analysis that serves to improve, extend, or perpetuate certain aspects of farm programs. This is sometimes done for purely venal purposes and in other times it may be a response to the particular politics of a given situation. I would argue that this does not necessarily represent a betrayal of our core beliefs, but rather reflects realistic self-interest, as well as the fact that there is ample room for welfare-improving analysis that accepts the fact that these programs exist and likely will continue to exist in spite of any strident protests to the contrary that may emanate from the academic community. I believe that farm subsidies usually generate deadweight losses. At the same time, I am fully aware that this belief is largely irrelevant to the future course of policy, and that there remains an important role for analysis to improve and inform policy.

Policymakers do not need the empirical insights of agricultural economists to be made aware of who gains and loses as a result of specific agricultural programs. On the contrary, legislators are keenly aware of the costs and benefits of their own policy actions and, congressional rhetoric notwithstanding, most certainly undertake those policy actions that best serve their own political self-interests. Recognition of these facts does not diminish the importance of our empirical policy analyses and our obligation to evaluate, critique, and potentially modify the direction of farm policy remains essential.

The History of Agricultural Policy Analysis

In his 1971 Presidential Address to the American Economic Association, Wassily Leontief (1971) described agricultural economics in glowing terms: "An exceptional example of a healthy balance between theoretical and empirical analysis and of the readiness of professional economists to cooperate with experts in the neighboring disciplines is offered by Agricultural Economics as it developed in this country over the last fifty years … agricultural economists demonstrated the effectiveness of a systematic combination of theoretical approach with detailed factual analysis. They also were the first among economists to make use of the advanced methods of mathematical statistics. However, in their hands, statistical inference became a complement to, not a substitute for, empirical research."

From the very beginning, statistical and econometric analysis was directed toward solving agricultural policy problems.[1] Most observers trace the roots of econometric policy analysis to the political arithmeticians. William Petty (1691) empirically studied a range of macroeconomic issues, including measurement of income, population, labor, and capital. Graunt (1662) worked with Petty to establish the foundations of demographics. Stigler (1954) attributes one of the first statistical evaluations of a demand schedule to the work of Davenant, who in 1698 published corn prices and corresponding consumption quantities. Geweke, Horowitz, and Pesaran (2006) note that this early work strived to develop a unification of theory and empirical measurement much in the spirit of Newton's laws of physics.

Agriculture and policy together have played a central role in the development of empirical economics. Stigler (1954) notes that much of the early empirical work involved household budget analysis. Engel (1857) used his analysis of household budgets to make a social policy recommendation—"The optimum social structure requires that the distribution of laborers among industries be proportional to the distribution of expenditures," (Stigler 1954). The empirical science was further advanced by the development of modern statistics. In the late 19th century, Galton, Edgeworth, Pearson, Jevons, and Yule developed methods for correlation analysis, curve-fitting, and statistical inference.

Pesaran (1990) notes that Henry Moore (1914, 1917) was a pioneer in the establishment of statistical estimation of economic relationships. Moore's followers included many of the founders of modern econometrics—Paul Douglas, Henry Schultz, Holbrook Working, Mordecai Ezekiel, and Fred Waugh—all names familiar to those working in applied agricultural economics. Louçã (2007) notes that the U.S. Bureau of Agricultural Economics—the precursor to today's Economic Research Service—was *the reference institution* for statistical research. The 1915 founding of the National Agricultural Economics Association and subsequent 1919 founding of the American Farm Economics Association, with Henry C. Taylor as its first president, also enhanced the development of agricultural policy analysis.

The modern era of econometric policy analysis has its foundations in the pre- and post-war activities of the Cowles Commission. Pesaran (1990) also notes that the founding of the Econometric Society and the Department of Applied Economics at Cambridge also played an important role in establishing the combination of economic theory, data, statistics, and computing techniques to form the discipline of econometrics. As I discuss in much greater detail below, work by Haavelmo (1944) and Koopmans, Rubin, and Leipnik (1950) undertaken with the Cowles Commission addressed many aspects of the problem of identification that has served as a pivot point for contemporary criticisms of structural models. Current criticisms offer few original insights beyond the foundational work undertaken by the Cowles Commission.

[1] My brief summary of empirical policy analysis depends heavily on the review papers of Pesaran (1990), Geweke, Horowitz, and Pesaran (2006), Maddison (2007), Stigler (2002), and Louçã (2007).

Dependence Modeling and the Federal Crop Insurance Program

As I have noted above, policymakers are certainly doing their part to keep government and academic researchers engaged in the empirical evaluation of agricultural policies. The most obvious example is perhaps the federal crop insurance program. U.S. policymakers have committed over $84 billion to this program over the next ten years. The program was first established in 1938 as a yield protection form of subsidized insurance, and it played a minor policy role until passage of the Federal Crop Insurance Act of 1980 (PL 96-365), which greatly expanded the scope and spread of crop insurance coverage. The program has been modified and expanded several times since the 1980 Act, and now provides yield and revenue risk coverage that exceeds $120 billion in most years. The program continues to expand and to take on new forms of coverage. These changes have raised a number of new empirical challenges to policy modeling.

Beginning in 1996, a new form of crop insurance that provided coverage against multiple, dependent sources of risk was introduced. This insurance initially took the form of "Crop Revenue Coverage" (CRC), which was followed by "Revenue Assurance" (RA). It is important to point out that the innovators behind these developments were agricultural economists—primarily Art Barnaby and Bruce Babcock. These economists' empirical policy efforts laid the foundations for what has become the primary form of crop insurance coverage in the United States—revenue insurance. I believe the introduction and subsequent expansion of crop revenue coverage is an important milestone in the development of U.S. agricultural policy. The 2014 Farm Bill is primarily constructed around a suite of revenue insurance types of programs. Figure 1 illustrates the prominence of revenue insurance in terms of its liability share of the total insurance portfolio. Revenue coverage quickly accounted for the largest share of participation in the federal program and now represents over 90% of the total liability in the program. Figure 1 also shows how the 2014 book of insurance, which accounted for $109 billion in total liability, was allocated among the different plans. The important fact is that over 88% of the total value insured was under a single program—"Revenue Protection" (RP). Revenue protection is a variant of revenue insurance where a revenue guarantee (established by a proportion of the product of expected yields and expected prices) is insured, and decreases in yields and/or prices can trigger an indemnity. Any lost yields are indemnified at the higher of the expected price or the actual price at harvest.

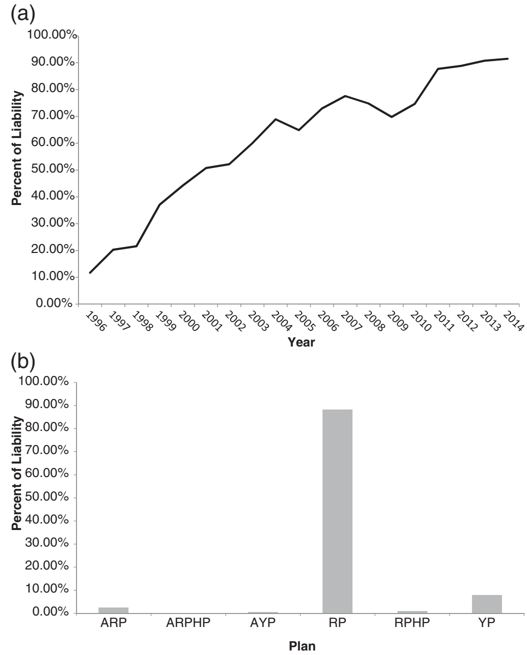

Figure 1. Increasing prominence of revenue coverage

(a) Proportion of Total Liability as Revenue Coverage
(b) 2013 Liability by Crop Insurance Plan (YP = Yield Protection, RP = Revenue Protection, RPHPE = Revenue Protection with Harvest Price Exclusion, AYP = Area Yield Protection, ARPHP = Area Revenue Protection with Harvest Price Exclusion, ARP = Area Revenue Protection).

In its role as the most prominent of U.S. agricultural policies, revenue insurance places some rather unique demands on the empirical analysis needed to derive important program parameters. In particular, revenue is determined by the product of two dependent random variables—yield and price. As empirical scientists, we have come to define this dependence largely in linear terms, typically represented by correlation or linear regression parameters. Galton introduced the notion of linear correlation in 1885, and this narrow idea of dependence has dominated our thinking ever since. Dependence is a much broader concept than this, however, and it is key to understanding multivariate ordering and modeling. I want to focus on empirical models of dependence as an example of a situation where modern empirical tools can be applied in innovative ways when modeling agricultural policy issues. The same discussion could focus on modern time-series techniques, structural and reduced form modeling, and many other aspects of the leading edge in quantitative techniques. I have chosen dependence modeling in light of its central role in empirical models of our primary agricultural policy instrument—crop revenue insurance. This is an active area of methodological research that is enjoying rapid innovations and it also offers important methodological extensions to a wide range of other empirical issues of importance to practicing agricultural economists. A wide range of empirical issues arise in crop insurance, including the pricing of coverage, portfolio design, and reinsurance mechanisms. I chose this particular emphasis with the full realization that the connections to the "good, bad, and ugly" of policy analysis may only be obvious to me. However, I hope to convince you that dependence modeling exemplifies the rapidly developing opportunities for applying state-of-the-art analytics to real-world policy issues of importance to contemporary agricultural economics.

Mari and Kotz (2001) note that "Dependence permeates our Earth and its inhabitants in a most profound manner … examples of interdependent meteorological phenomena in nature, interdependence in medical, social and political aspects of our existence, not to mention economic structures, are too numerous to be cited individually." It remains common to hear dependence discussed solely within the context of correlation, even though we have long realized that the concepts are distinct and confusion of the issues can lead to huge implications for economic relationships in the real world.[2]

Mari and Kotz (2001) also note that "Casual readers … are often under the impression that to establish practical independence (or, more specifically, an absence of meaningful relationships among the variables), it suffices to verify that the correlation coefficients are, effectively zero … there has been a considerable degree of harm caused by this attitude." Bayes (1763) noted that "Events are independent when the happening of any one of them does not [n]either increase [n]or abate the probability of the rest." This definition of independence is one that remains appropriate today. However, empirical approaches toward modeling dependence have developed significantly over the last several decades. The conceptual ideas are not necessarily new, with many of the more salient points being traced to Sklar's (1959) theorem. However, much as our regression toolkit expanded to recognize linear models as a special case of nonlinear relationships in the 1970s and 1980s, so has our recognition of linear correlation as a special case of dependence. Figure 2 illustrates a variety of joint distributions, including several that have zero correlation but strong nonlinear dependence.

For multivariate normally-distributed variables, Pearson's linear correlation view of dependence works well. However, many important aspects of dependence are not fully captured in our conventional thinking. Recall that, if the covariance of two nondegenerate random variables X and Y ($cov(X,Y) = E(XY) - E(X)E(Y)$) is zero, we know that the variables are uncorrelated and that $E(XY) = E(X)E(Y)$. This concept is often communicated through the use of the linear, Pearson correlation coefficient $\rho = cov(X,Y)/\sqrt{var(X)var(Y)}$, or equivalently through an ordinary least squares estimate of β in $Y = \alpha + \beta X$, which is given by $\hat{\beta} = \hat{\rho}\hat{\sigma}_Y/\hat{\sigma}_X$. However, independence implies that $F_X(X)F_Y(Y) = F_{X,Y}(X,Y)$, where $F(\cdot)$ represents the cumulative distribution function. It is obvious that independence implies a lack of correlation but that the converse is not true since correlation only considers linear relationships.[3] In terms of joint distribution functions, departures from multivariate normality may lead to a divergence in the concepts.

When one recognizes this wider view of dependence, the conventional use of a Pearson correlation coefficient as a measure of dependence is no longer sufficient.

[2] As students in any introductory statistics class are taught, a lack of dependence implies a lack of correlation, but the opposite is certainly not always true.

[3] Recognition of this fundamental point is not new. In his 1918 text, *Introduction to Mathematical Statistics*, West (pages 84–85) noted that "if the regression curve is of a certain shape, the value of ρ would be very small even though practically perfect correlation [dependence] exists."

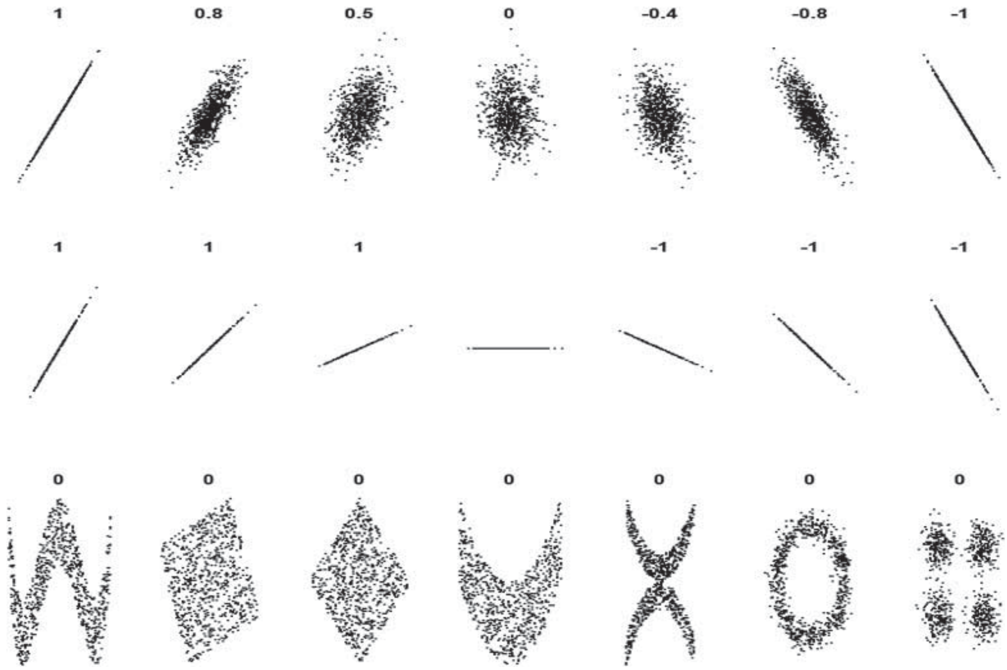

Figure 2. Correlation vs. dependence

Source: Generated using R Code authored by Denis Boigelot, available at: http://commons.wikimedia.org/wiki/File:Correlation_examples2.svg.

Joe (1997) discusses a number of alternative conceptual approaches to representing dependence. A non-exhaustive list includes quadrant/orthant dependence, association, increasing or decreasing dependence, and monotone dependence. Positive quadrant dependence exists if $Pr(X_1 > a, X_2 > b) \geq Pr(X_1 > a)Pr(X_2 > b)$. Stochastic increasing positive dependence, which implies that Y is likely to take on larger values as X increases, occurs when $Pr(X > x|Y = y) \uparrow y \ \forall x$.

At this point, one must wonder how we went from a discussion of agricultural policy and crop insurance to a digression on concepts of correlation and dependence. I hope to convince you that this very distinction between correlation and dependence has a critically important role in the current agricultural policy situation. Assumptions made about the nature of dependencies among multiple sources of risk, such as yields and prices, in the empirical modeling of policy parameters have significant implications for the resulting values of the parameters and operation of the program. I believe this issue merits the attention that I am devoting to it here because of the increasingly prominent role that subsidized crop insurance plays in U.S. agricultural policy, as well as in the policy actions of legislators around the world.

Though the distinctions between correlation and dependence have long been recognized, empirical methods that address the differences are a relatively modern development. Sklar (1959) introduced the notion of copula functions, which join together one-dimensional distribution functions to form multivariate distribution functions. Copulas represent an integral tool for modeling dependence relationships that allow us to distinguish between dependence relationships and models of univariate marginal distributions. I want to highlight copulas as one of the many "good" aspects of contemporary empirical policy modeling. Much of the work on copulas has been motivated by their applicability to issues in risk management, insurance, and financial economics (see, among others, Rodriguez 2003; Cherubini, Luciano, and Vecchiato 2004; Hu 2006; Patton 2006; and Jondeau and Rockinger 2006). In the empirical literature, copula models have been used extensively in the design and rating of crop revenue insurance contracts, where the inverse correlation of prices and yields plays an important role in pricing revenue risk.

A p-dimensional copula, $C(u_1, u_2, \ldots, u_p)$, is a multivariate distribution function in the unit hypercube $[0,1]^p$ with uniform $U(0,1)$ marginal distributions. As long as the marginal distributions are continuous, a unique copula is associated with the joint distribution, F, that can be obtained as:

$$(1) \qquad C(u_1, u_2, \ldots u_p) = F(F_1^{-1}(u_1), \ldots, F_p^{-1}(u_p)).$$

In a similar fashion, given a p-dimensional copula, $C(u_1, \ldots u_p)$, and p univariate distributions, $F_1(x_1), \ldots, F_p(x_p)$, equation (1) is a p-variate distribution function with marginals $F_1, \ldots F_p$ whose corresponding density function can be written as

$$(2) \qquad f(x_1, x_2, \ldots x_p) = c(F_1(x_1), \ldots, F_p(x_p)) \times \prod_{i=1}^{p} f_i(x_i).$$

Provided that it exists, the density function of the copula, c, can be derived using equation (1) and marginal density functions, f_i, as follows:

$$(3) \quad c(u_1, u_2, \ldots u_p) = \frac{f(F_1^{-1}(u_1), \ldots, F_p^{-1}(u_p))}{\prod_{i=1}^{p} f_i(F_i^{-1}(u_i))}.$$

There are several parametric families of copulas applied in the literature. Two of the most commonly used copula families are elliptical copulas and Archimedean copulas. Gaussian and t-copulas are examples of elliptical copulas, while the Clayton and Gumbel are among Archimedean copulas.

Copula models are especially well-suited to considering tail behavior in that they allow for more flexible characterizations of tail dependence. Tail dependence pertains to the dependency relationships among variables taking extreme values. The coefficients of upper tail dependence, λ_U, and lower tail dependence, λ_L, are defined as follows:

$$(4) \qquad \lambda_U = \lim_{u \to 1^-} P(X_2 > F_{X_2}^{-1}(u) | X_1 > F_{X_1}^{-1}(u))$$

$$(5) \qquad \lambda_L = \lim_{u \to 0^+} P(X_2 \leq F_{X_2}^{-1}(u) | X_1 \leq F_{X_1}^{-1}(u)).$$

These coefficients of tail dependence, λ_U and λ_L, can be expressed as a function of a copula as:

$$(6) \qquad \lambda_U = \lim_{u \to 1^-} \frac{1 - 2u + C(u,u)}{1-u}$$

and

$$(7) \qquad \lambda_L = \lim_{u \to 0^+} \frac{C(u,u)}{u}.$$

Different copulas allow for differing degrees of tail dependence.

My own thinking on dependence and eventual introduction to copula models was stimulated by a simple observation that was apparent to everyone working in crop insurance. Conventional wisdom recognized that the spatial correlation of yields tended to differ in years of extreme weather stress. Common sense and anecdotal observation revealed that extreme weather events such as drought tended to impact wide geographic areas. This observation was often communicated in terms of the "systemic" nature of agricultural risks. The geographic correlation of yields represents the same basic relationship as the correlation of yields and prices. Prices are determined in an aggregate, integrated market. The greater is the degree of spatial correlation among yields at a disaggregate level (e.g., the county), the greater will be the correlation of yields and prices. In 2001, I examined this phenomenon by considering the relationship between geographic distance (defined as the great-circle distance between county centroids) and the linear, Pearson correlation coefficient (Goodwin 2001). I want to emphasize that this observation was by no means original to me, but rather was something that all economists working in crop insurance were familiar with, even if many of us did not know the dependence concepts that underlie the observations. I have updated the calculations I made several years ago.

Figure 3 presents linear, Pearson correlation coefficients among detrended, county-level corn yields taken from Illinois, Iowa, and Indiana over the 1960–2013 period and the distances (in miles) between the centroids of each county.[4] The diagrams include a quadratic regression of correlation

[4] Yields were detrended using a nonparametric, generalized additive trend model. Deviations from trend were recentered to 2013–equivalent values by adding the deviations to the 2013 predicted yields for each county.

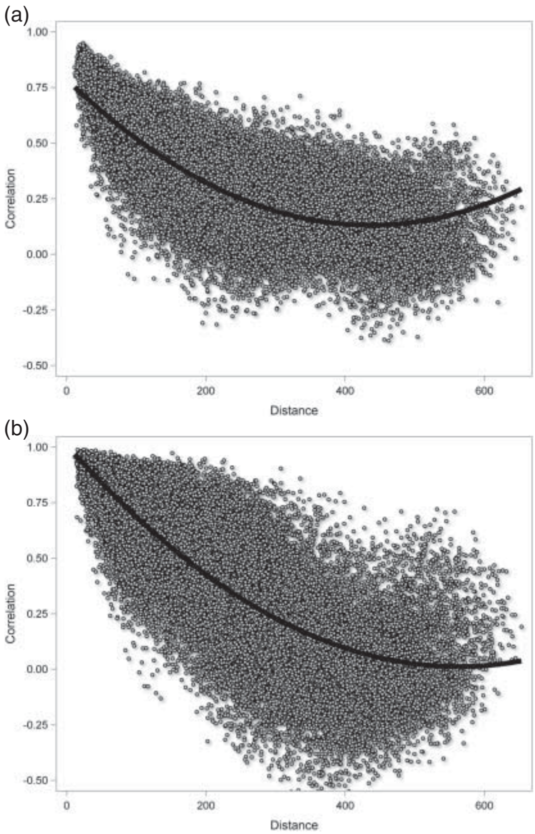

Figure 3. Corn yield spatial correlation and distance: IL, IN, and IA detrended yields, 1960–2013

(a) Linear Correlation and Distance: "Normal" Years
(b) Linear Correlation and Distance: "Drought" Years.

on distance. I defined "extreme" stress years as any year in which the average negative deviation from trend among all counties exceeded 5 bushels per acre. The analysis illustrates the important distinction between correlation and dependence. In years that experienced significant growing stress, correlation tended to die out much slower over distance than was the case during "normal" (non-stress) years. In normal years, spatial correlation approached zero when the distance between counties was about 200 miles. However, in extreme stress years, the decay of correlation across space was much slower, requiring about 400 miles to approach zero.

The suggestion arising from this simple observation is that correlation tends to be "state-dependent," or that dependence is non-constant across the marginals. These relationships came to be formalized in terms of copulas and models of dependence. The research was, interestingly, given a great boost as a result of the financial crisis of the latter part of the 2000s and the concomitant realization that assumptions of constant, linear correlation that were made in pricing derivative assets such as mortgage default guarantee swaps tended to significantly understate the "tail-risk" or probabilities of catastrophic losses. The same implications apply to crop revenue insurance contracts.

The finding that yields tend to be more strongly dependent (i.e., more positively correlated) during periods of yield shortfalls suggests that a copula function that captures this negative tail dependence may be suitable. To this end, I chose two representative counties (McClean County, Illinois, and Kossuth County, Iowa) and fit a Clayton copula to the detrended corn yield data using standard maximum likelihood estimation techniques. Nonparametric marginals were used, thereby sidestepping issues related to fitting marginal parameters.[5] The resulting copula parameter estimate was $\theta = 0.8032$ with an associated standard error of 0.2583. The copula is illustrated for simulated standard normal marginals in figure 4. The contour plot represents the joint distribution of yields and illustrates the lower tail-dependence predicted by the preceding evaluation of correlation and distance.

Figure 5 illustrates the different dependency structures that are implied by alternative copula specifications. As is true of any other econometric specification, there is no limit to the types of copula functions that can be specified, as long as the requirements necessary to represent a multivariate probability distribution function are satisfied. There are, of course, a limited number of copula families that have been identified in the applied literature.[6]

The distinction between linear Pearson correlation and dependence has an analogous interpretation in considerations of linear and nonlinear regression models. As I

[5] A nonparametric marginal cumulative distribution function (CDF) is representing using the empirical quantile (rank) CDF. Using the nonparametric, empirical marginals is preferred in that the asymptotic distributions of the copula estimates are not affected by the first-stage estimation of the marginals, as has been shown by Chen and Fan (1996). Further, Charpentier, Fermanian, and Scaillet (2007) have noted that copula estimates based upon the empirical CDFs may be preferred because this approach can lead to smaller estimation variations compared to those based on the true marginals, even if known.

[6] See Nelsen (2006) or Joe (1997) for detailed discussions of alternative copula specifications.

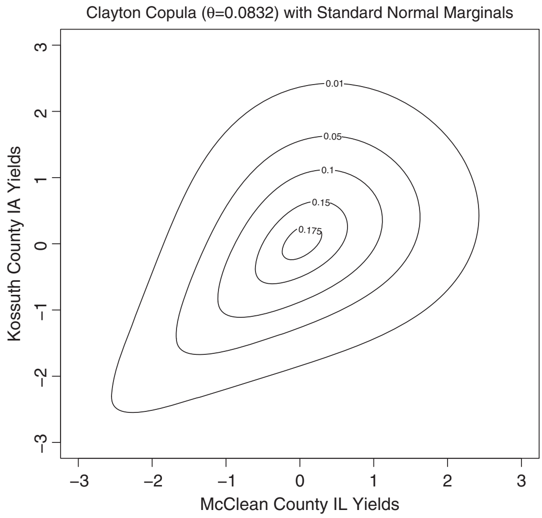

Figure 4. Clayton copula fit via maximum likelihood estimation to corn yields for McClean County, IL and Kossuth County, IA

have noted above, the ordinary least squares estimate of the slope parameter in a simple regression model is equivalent to a scaled version of the Pearson correlation coefficient ($\hat{\beta} = \hat{\rho}\hat{\sigma}_Y/\hat{\sigma}_X$). Thus, copula models of dependence that allow for departures from symmetric distributions with constant linear correlation and zero tail dependence may also be used to model nonlinear relationships among variables. Of course, such a specification is entirely equivalent to a parametric specification that incorporates a specific nonlinear (or linear) relationship among random variables. That is, the choice of a specific copula model is just like any other specification problem in that the particular model chosen necessarily defines the nature of the relationship among variables, at least within the parameter space permitted for each copula model. For example, the Clayton copula model illustrated in figure 4 is capable of representing varying degrees of lower tail dependence (depending on the parameter estimate) but necessarily imposes zero upper tail dependence. Recalling Dick King's 1979 AAEA Presidential Address (King 1979) on "Choices and Consequences," every specification choice necessarily imposes restrictions on the economic relationship being modeled, and one must always be aware of the consequences of such restrictions.

Such modeling of dependence represents only one avenue by which newly-developed methods drawn from econometrics and statistics can be brought to bear on important policy issues. Though my opinion is certainly shaped by my own interests, I believe empirical work that addresses specific policy problems in the design and operation of crop insurance contracts is one of the most important areas at present where applied and agricultural economists can contribute to real-world policy issues. This research has the potential to impact billions of dollars in U.S. Treasury expenditures. This importance was reinforced by changes in the 2014 Farm Bill that further heightened the importance of subsidized insurance programs. The RMA has commissioned numerous studies that address key policy issues in the program, and these studies have resulted in important changes in the operation of the program that reflect the empirical modeling efforts of the agricultural economics profession.[7]

A whole range of relevant policy questions demanding empirical analysis remains. For example, we suspect that the provision of subsidized risk management instruments affects the actions of growers receiving such support and in turn is likely to provoke distortions in what is produced and how it is produced. Yet despite an accumulation of empirical research results addressing these issues, little consensus exists about the nature and magnitude of such distortions. A fundamental paradox of subsidized insurance exists in the fact that participation in such programs always requires substantial subsidies, in spite of the fact that our conventional theory suggests that, under symmetric information, risk-averse agents will fully insure at actuarially-fair premium rates. As Hazell, Pomareda, and Valdès (1986) note: "… the fact is that, with few exceptions, farmers in both developed and developing countries have been unwilling to pay the full cost of all-risk crop insurance … most all-risk programs remain public sector schemes … their management is often subject to political pressure regarding premiums and coverage and the programs are often used as a mechanism to transfer income to farmers."

The potential for private insurance programs to successfully exist alongside such heavily-subsidized public programs remains

[7] See, for example, the comprehensive actuarial review of Coble et al. (2010) and the overview by LaFrance, Pope, and Tack (2012) of work undertaken by agricultural economists in response to requests for proposals by the RMA.

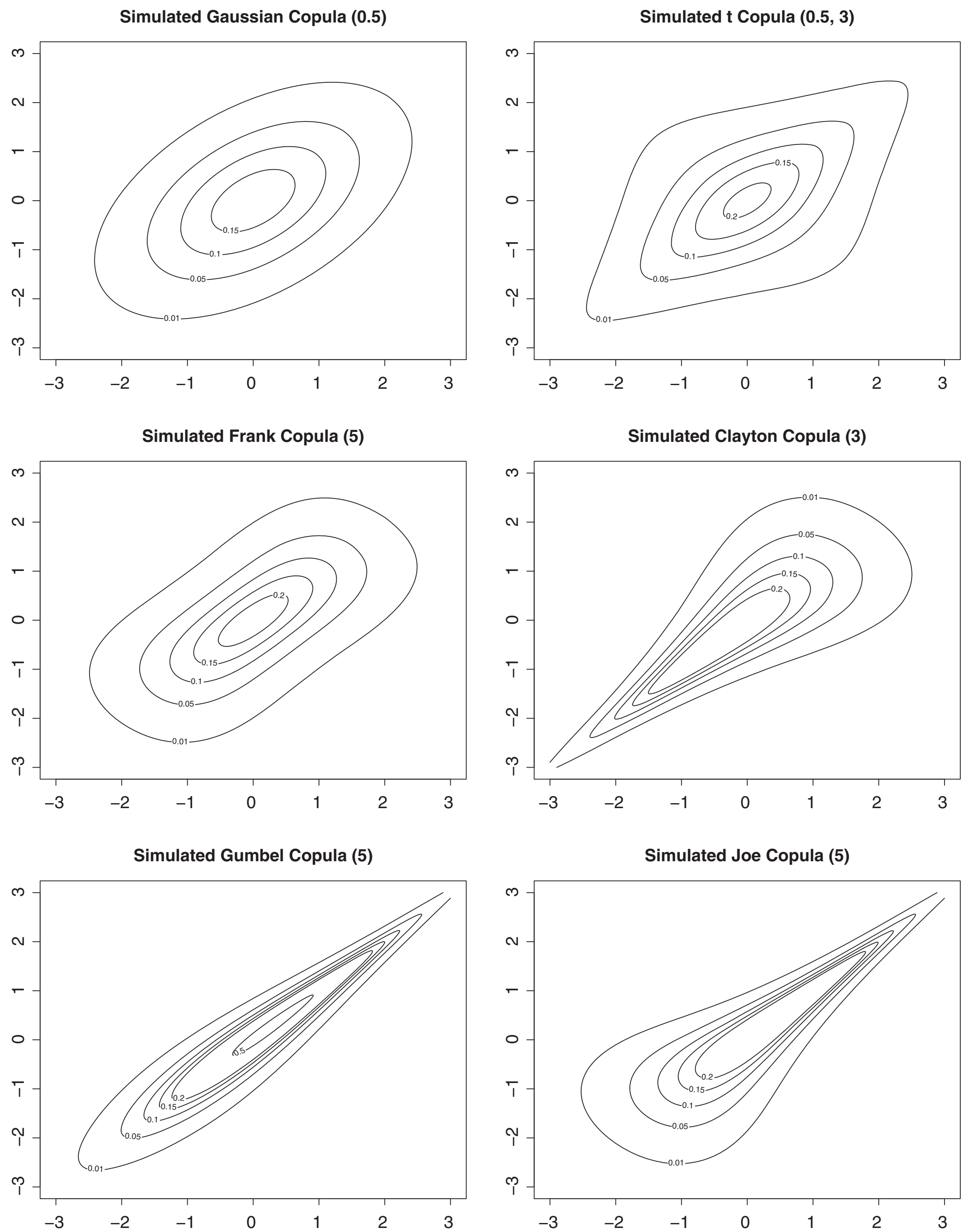

Figure 5. Dependence Structures Implied by Different Copulas

a very important issue. Is the lack of private insurance contracts a reflection of crowding out by government subsidies or does it represent some perceived market failure?

These questions lead to other empirical puzzles about how subsidies affect behavior. Do decoupled transfers affect production? Much has been written on the topic but the results remain conflicting. How important are risk preferences, wealth effects, and capital market imperfections in shaping producers' response to risk and subsidies? The questions are indeed endless and the policies are themselves of a nonstationary nature. Policy changes quickly reduce the relevance of existing empirical research and this is perhaps nowhere more apparent than in the federal crop insurance program, which has undergone enormous changes over a short period of time.

The Bad

Along with the enormous potential for empirical analysis to shape policy, there are several aspects of our work that often fall short of providing policymakers with all of the relevant information that may be needed to formulate effective policy. One simple fact that my own policy work has taught me is that policy problems often demand answers, even when the information necessary to adequately inform the problems is lacking. Applied policy work is one example of research that lacks the luxury of being able to back away from messy problems where data are lacking or the conditions desirable for appropriate science may be compromised. There are many research questions that we simply do not have adequate information or empirical data to pursue. We sidestep such problems when the desired end result is a publication intended to be communicated to our peers. In contrast, policymakers do not have this luxury and are often forced to make compromises in order to obtain results and implement policy. Such issues present severe challenges to the empirical purist. However, there remains much that can be done to improve our own empirical analysis in ways that better inform policy. I want to briefly identify where some of our empirical shortcomings exist and how we might go about reforming our science to address them.

Much empirical policy work concludes with a summary that posits “My results differ from the existing empirical literature and therefore my new method or estimator is superior.” This is common, for example, in models of yield and price distributions, where different density estimators are often used to derive different premium rates. I am guilty of this in my own work. I believe we often fall short in terms of communicating all of the weaknesses and maintained assumptions that may underlie the analysis. For example, it is common to provide estimates of premium rates but much less common to present confidence bands or standard error estimates associated with such estimates. Further, many aspects of the empirical exercise are often ignored when making inferences. It is very common to detrend yield data collected over time and then to treat the detrended yields as though they were observed without error. It is common to pool data items that clearly are not independent of one another. Pre-test estimation almost always underlies reported results and yet the distortions that result from such practices are rarely considered. This is perhaps one dark side to the very positive developments in our computational resources—cheap computing power has made it easy to undertake extensive pre-test estimation that is almost never acknowledged.

The N, P, and i.i.d. Problems

As I have noted, empirical policy analysis often demands answers even when the data are severely lacking. In the case of crop insurance, the sample sizes are almost always small and the data suffer from many shortcomings, including unobserved dependencies and heterogeneity. One is almost always forced to strike a compromise between pooling data collected over time, cross-sectional units, or space in order to obtain a sufficiently large sample against the fact that such pooled data almost always suffer from deficiencies that complicate inference. For example, consider the very prominent case of estimating a county-level density for corn yields in order to derive premium rates for index insurance plans. A very substantial body of academic research has addressed this issue. A shortcoming of yield data is that we only observe a single observation annually for each cross-sectional unit. We are forced to either pool data across counties or to use data collected over time. In the former case, we know that strong spatial dependencies exist among yields observed in the same year, meaning that N observations provide much less information than what would be implied by N independent data points. In the latter case, we are very much aware that the underlying technology and structure, as well as the nature of the policies being modeled, has undergone significant change, even across a relatively short history.

The technology underlying corn yields observed in 2013 is very different from that

Table 1. Alternative Copula Estimates for McClean County, IL Corn

Copula	θ_1	SE	θ_2	SE
Gaussian	−0.5291	0.0926	–	–
t	−0.5257	0.1065	7.7692	15.0467
Frank	−3.4432	0.9315	–	–
R Clayton 90°	−0.9507	0.2829	–	–
R Gumbel 90°	−1.4754	0.1651	–	–
R Joe 90°	−1.8375	0.2649	–	–
R Clayton-Gumbel 90°	−0.0010	0.0007	−1.5642	0.1762
R Joe-Gumbel 90°	−1.1775	0.7156	−1.4075	0.5896
R Joe-Clayton 90°	−1.7432	0.2874	−0.2896	0.3061
R Joe-Frank 90°	−2.1234	0.6345	−0.9639	0.0872
R Clayton 270°	−0.9507	0.2644	–	–
R Gumbel 270°	−1.4754	0.1635	–	–
R Joe 270°	−1.8474	0.2675	–	–
R Clayton-Gumbel 270°	−0.7909	0.4524	−1.1063	0.2159
R Joe-Gumbel 270°	−1.0010	1.9656	−1.4545	1.8660
R Joe-Clayton 270°	−1.1292	0.3434	−0.9292	0.3360
R Joe-Frank 270°	−6.0000	5.8025	−0.4499	0.3471

which shaped yields in 2003. Likewise, significant changes in the federal crop insurance program have occurred over the last several years. These changes include the shift toward revenue coverage illustrated in figure 1, the 1994 Crop Insurance Reform Act, the 2000 Agricultural Risk Protection Act (ARPA), and the substantial increases in subsidies that resulted in participation increasing from 10% to nearly 90% of insurable acreage. Private insurers deriving premium rates for standard commercial property and casualty lines such as automobile insurance balance these factors in their rating. Loss events among individual insureds are generally independent while the safety features on automobiles have changed dramatically over time. Thus, automobile policies are rated using pooled data taken from the most recent experience.

I have referred to these circumstances as the "*N*, *P*, and *i.i.d.*" problems. We are always balancing non-independence against structural changes. We are almost always working with small *N* sample sizes, which significantly constrains available degrees of freedom and thereby restricts the structure of our models (limiting us to a small *P* number of parameters to characterize the problem). Requirements for "independent and identically distributed" (*i.i.d.*) samples are almost always violated. Addressing these shortcomings requires that additional information be brought to the analysis. This may take the form of parametric restrictions (reducing *P*), the gathering of more data (increasing *N*), the addition of other information, such as credibility weighting factors or institutional knowledge, or other modifications to the empirical model such as detrending.

A further complication pertains to the fact that the probabilities and premium rates of interest in crop insurance often apply to certain rare events, which by their very definition are likely to suffer from thin data problems. We may want to identify the probability of a 1-in-100-year loss event. However, given the sample sizes typically available, we may never observe the event in our empirical sample. Alternatively, a short sample may mean that such an event is given too much weight in an empirical model. Returning to the discussion regarding tail dependence and copulas, it is apparent that such models are attempting to discern differences that may only apply to tail behavior, which by definition applies to rare events.

Consider again the problem of using a copula model to derive premium rate and loss-probability estimates for a typical area-wide revenue contract. Table 1 presents maximum likelihood estimates for a variety of copula models that are capable of capturing the inverse dependency between prices and yields. This particular example uses detrended county-average yield data for McClean County, Illinois, for the period spanning 1960–2013. Prices are assumed to be log-normally distributed and the dependency relationship between yields and prices is estimated using logarithmic returns between the February and November quotes for a

Table 2. Alternative Copula Estimates for McClean County, IL Corn

Copula	GoF P-Value	GoF Stat	LLF	AIC	BIC
Gaussian	0.71	0.14	7.26	−12.53	−10.56
t	1.00	0.03	7.40	−10.80	−6.86
Frank	0.57	0.33	6.85	−11.70	−9.73
R Clayton 90°	0.48	0.50	3.85	−5.70	−3.73
R Gumbel 90°	0.82	0.05	8.27	−14.55	−12.57
R Joe 90°	0.45	0.57	8.10	−14.20	−12.23
R Clayton-Gumbel 90°	–	–	8.41	−12.81	−8.87
R Joe-Gumbel 90°	–	–	8.44	−12.88	−8.94
R Joe-Clayton 90°	–	–	8.53	−13.05	−9.11
R Joe-Frank 90°	–	–	8.27	−12.54	−8.60
R Clayton 270°	0.60	0.28	7.88	−13.77	−11.80
R Gumbel 270°	0.37	0.79	5.69	−9.37	−7.40
R Joe 270°	0.13	2.32	2.83	−3.66	−1.69
R Clayton-Gumbel 270°	–	–	8.01	−12.03	−8.09
R Joe-Gumbel 270°	–	–	5.69	−7.38	−3.44
R Joe-Clayton 270°	–	–	7.96	−11.91	−7.97
R Joe-Frank 270°	–	–	6.40	−8.81	−4.87

Note: GoF = Goodness of fit, LLF = Log-likelihood function.

December corn futures contract. The estimated parameters are highly statistically significant and reflect the inverse relationship between prices and yields.

However, as is true in any empirical specification, one must have some method of distinguishing between the alternative estimates in order to determine the optimal model. Common approaches include comparing heuristic model goodness-of-fit criteria such as the Akaike Information Criterion (AIC) or Bayesian Information Criterion (BIC), log-likelihood function values, and goodness-of-fit specification tests. Table 2 presents a range of common criteria used to select from among alternative copula specifications. This includes test statistics and associated p-values for a variant of the Cramér-von Mises specification test.[8] A problem commonly encountered in applied work is immediately obvious—different criteria suggest different specifications and the goodness-of-fit tests support each and every specification. If all specifications are supported and various criteria suggest different optimal specifications, one must consider whether the alternatives result in differences in the estimated parameters of interest—loss probabilities and premium rates. Table 3 presents loss probabilities and associated premium rates for revenue coverage from each of the alternative copula specifications. At high coverage levels, the probabilities and rates are very similar. However, when one moves into deeper losses in the tails of the revenue distribution, significant differences arise. At a 75% guarantee, the Gaussian rate (which is actually used in rating revenue coverage in the U.S. program) is almost one-half of the largest calculated rate. The specification preferred by the AIC and BIC criteria yields premium rates that are almost 40% higher than that of the Gaussian copula.

In short, new empirical methods for modeling dependence show great promise in empirical policy analysis. Alternative estimates may have significant implications for the performance of insurance programs, especially for deep losses in the tails where differences in the multivariate distributions are most acute. However, information about tail behavior in the samples we commonly work with may be very limited, and distinguishing the "best" model from infinitely-many alternatives may be very difficult.

The Ugly

One could point to any number of shortcomings in our empirical practice as embodying an "ugly" side of policy analysis. I want to

[8] The goodness-of-fit test of Huang and Prokhorov (2014) was applied. The test statistic requires thrice-continuously differentiable functions, and thus cannot be defined for the mixture copula models.

Table 3. Example: Revenue Insurance for McLean County, IL Corn

Copula	95% P(Loss)	95% Rate	85% P(Loss)	85% Rate	75% P(Loss)	75% Rate
Gaussian	42.76	4.46	18.14	1.47	4.68	0.29
t	40.81	4.21	17.05	1.50	4.49	0.35
Frank	41.84	4.58	17.85	1.65	5.18	0.42
R Clayton 90°	43.34	4.67	20.04	1.57	4.47	0.37
R Gumbel 90°	40.87	4.45	17.38	1.58	5.18	0.40
R Joe 90°	38.55	4.39	17.45	1.65	5.77	0.43
R Clayton-Gumbel 90°	40.32	4.22	17.26	1.48	4.37	0.35
R Joe-Gumbel 90°	39.78	4.24	17.46	1.48	4.50	0.38
R Joe-Clayton 90°	39.43	4.10	16.69	1.51	4.47	0.31
R Joe-Frank 90°	39.39	4.32	17.08	1.64	5.14	0.41
R Clayton 270°	39.83	4.34	17.54	1.64	5.06	0.40
R Gumbel 270°	43.41	4.63	18.38	1.67	4.68	0.38
R Joe 270°	44.01	4.70	18.90	1.54	4.81	0.41
R Clayton-Gumbel 270°	41.24	4.28	17.27	1.41	4.76	0.35
R Joe-Gumbel 270°	43.77	4.77	19.10	1.70	5.28	0.37
R Joe-Clayton 270°	40.25	4.21	16.56	1.51	4.82	0.31
R Joe-Frank 270°	41.68	4.58	17.31	1.92	5.75	0.52

focus on two specific issues that are only loosely related and reflect my own set of concerns regarding the current practice and future direction of empirical policy analysis.

Sampling Variability and the Precision of Estimated Policy Parameters

Models of dependence have become important in agricultural policy analysis because of the significant expansion of subsidized risk management policy instruments that address multiple, dependent sources of risk. Crop revenue insurance must consider the relationship between price and yield and the fact that each year of experience generates only a single observation necessarily means that actuarial models are often forced to work with very short samples. Further, risk management policies are being expanded to encompass far greater numbers of dependent sources of risk. For example, the dairy livestock gross margin (LGM) insurance plan is based upon the combination of 24 futures contracts—12 for milk, 5 for corn, and 7 for soybean meal. The 2014 Farm Bill mandated development of new revenue minus cost margin plans that will address multiple sources of risk arising from input and output prices and production. It is not uncommon to see complex, multivariate insurance instruments developed using as few as 10 observations. What is less common, however, is to see some consideration of how the sampling variability associated with estimates based on such small samples may affect the precision of important policy parameters.

To illustrate these issues, I considered a synthetic and hypothetical insurance instrument comprised of county-level revenues and the sum of revenues for four major Illinois counties.[9] I considered rating models based on a t-copula that was estimated for sample sizes of 20 and 50 observations. I adopted the standard approach of estimating a nonparametric trend equation

$$(8) \qquad y_t = g(t) + \epsilon_t$$

and generated a sample of detrended yields as

$$(9) \qquad \hat{y}_t = \hat{y}_{2013} + \hat{\epsilon}_t.$$

Figure 6 illustrates the potential impacts of small sample sizes on the resulting trend model estimates. Panel (a) illustrates the yield trend based on 20 randomly sampled years, while panel (b) repeats the analysis for the full sample of 54 observations. Note the sensitivity of the trend estimates to sample size, and in particular to the drought experienced in 2012.

[9] These four counties (McClean, Logan, Macon, and Tazewell) are in a common crop reporting district and thus are in close proximity to one another. Yield and price data cover the 54-year period spanning 1960–2013.

I randomly sampled with replacements from the available data, and for each replicated sample detrended the yields and estimated a *t*-copula. Table 4 contains a summary of the sampling variability for the estimates based on sample sizes of 20 and 50 observations. I include 90% coverage intervals about the mean estimates of the correlation matrix and degrees of freedom parameter. The most striking result pertains to the very wide confidence bands. For example, the 90% coverage intervals for the correlation parameters for prices and yields—a key parameter in rating revenue coverage—range from about -0.15 to −0.75. Revenue coverage rates are very sensitive to the values of these parameters. Table 5 presents coverage intervals and mean values for revenue rates and loss probabilities for each county and for the sum of revenues across all four counties. In the case of estimates based on a sample size of 20, the uncertainty associated with the true values of the parameters is very significant, with key policy parameters differing by a factor of three or more. The sampling variability of rates and loss probabilities remains significant, even for a sample size of 50, which far exceeds what is commonly used in practice.

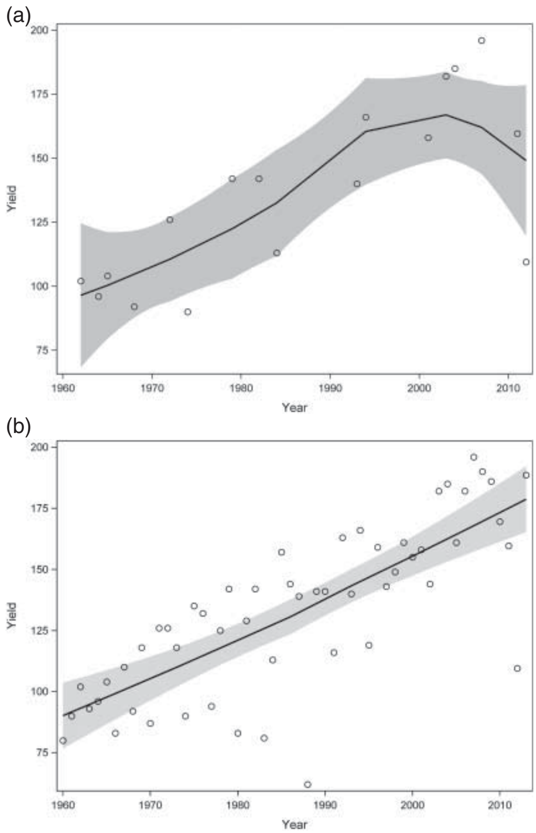

Figure 6. Detrending? 20 vs. 54 Observations

(a) 20 Observations
(b) 54 Observations.

So, what is one to do with knowledge of such sampling variation in critical policy instruments? There may be little that can actually be done in a programmatic sense. Estimates of these parameters must be derived. However, I would argue that the policy analyst and researcher has an obligation and responsibility to clearly communicate not only estimates of the necessary parameters but also the precision of such estimates. Policymakers may adjust policies in response to such information and a wide range of industry participants should find an understanding of the precision of estimates to be important. In the case of crop insurance, such

Table 4. Sample Size Impacts on Variability of Correlation Estimates

	$n = 20$			$n = 50$		
Parameter	5%-ile	Mean	95%-ile	5%-ile	Mean	95%-ile
ρ_{12}	0.64	0.86	0.97	0.77	0.88	0.94
ρ_{13}	0.69	0.89	0.99	0.81	0.90	0.96
ρ_{14}	0.72	0.91	0.98	0.87	0.93	0.96
ρ_{1P}	−0.72	−0.44	−0.03	−0.70	−0.48	−0.26
ρ_{23}	0.73	0.88	0.98	0.81	0.89	0.95
ρ_{24}	0.79	0.91	0.98	0.87	0.93	0.97
ρ_{2P}	−0.77	−0.47	−0.15	−0.71	−0.51	−0.29
ρ_{34}	0.73	0.88	0.98	0.81	0.90	0.95
ρ_{3P}	−0.74	−0.46	−0.14	−0.69	−0.51	−0.28
ρ_{4P}	−0.74	−0.46	−0.15	−0.71	−0.51	−0.27
df	1.57	15.35	100.00	3.54	17.59	100.00

Table 5. Sample Size Impacts on Rate and Probability Estimates

	$n=20$			$n=50$		
Parameter	5%-ile	Mean	95%-ile	5%-ile	Mean	95%-ile
Pr($loss_1$)	3.00%	6.28%	10.06%	5.42%	7.28%	9.43%
Pr($loss_2$)	3.09%	5.96%	9.23%	5.18%	7.08%	9.17%
Pr($loss_3$)	3.03%	5.92%	8.92%	5.13%	6.79%	8.35%
Pr($loss_4$)	2.69%	5.64%	8.60%	4.93%	6.65%	8.56%
Pr($loss_{sum}$)	2.88%	5.55%	8.27%	4.86%	6.50%	8.27%
$Rate_1$	0.18%	0.51%	1.03%	0.37%	0.63%	0.95%
$Rate_2$	0.18%	0.47%	0.92%	0.36%	0.60%	0.89%
$Rate_3$	0.18%	0.47%	0.86%	0.35%	0.56%	0.78%
$Rate_4$	0.15%	0.42%	0.78%	0.33%	0.53%	0.78%
$Rate_{sum}$	0.16%	0.41%	0.76%	0.33%	0.52%	0.75%

information may be used to better inform credibility weighting or reinsurance decisions.

The Identification Conundrum

I want to conclude my discussion of some of the uglier aspects of empirical policy analysis by briefly noting the ongoing debate over structural versus non-structural models and the role of so-called "quasi-natural" experiments in the identification of policy impacts. There is probably no more contentious issue among those working in empirical policy analysis than the issues surrounding the proper identification of policy effects. In his online blog, Frank Diebold (2013) observed that "The structure police, especially new recruits, are often fanatical." Esther Duflo (2004) argued that "Creating a culture in which rigorous randomized evaluations are promoted, encouraged, and financed has the potential to revolutionize social policy during the 21st century, just as randomized trials revolutionized medicine during the 20th." The debate, though uncharacteristically nasty, is very similar to many other methodological arguments that will be familiar to anyone who has worked in empirical policy analysis in recent years. Similar dogmatic approaches and sweeping dismissive reactions to entire methodologies have arisen over the years around such issues as nonstructural time-series modeling, behavioral and experimental economics, and models of imperfect competition. In nearly all such debates, I would argue that both sides of the argument are correct and, to the extent that their views are absolute, both are incorrect. The intellectual gatekeeper, who is empowered to dismiss entire approaches to scientific inquiry or empirical analysis (often in the referee role) may, in fact, inhibit the progress of science and policy research in the mistaken belief that there is only one way (their way) to empirically evaluate a policy problem. As Box noted in the aforementioned quote, "All models are wrong." Leamer (1983) noted that there seems to be a "… sharp distinction between economics where randomized experiments are rare and 'science' where experiments are routinely done. But the fact of the matter is that no one has ever designed an experiment that is free of bias and no one can … economists have inherited from the physical sciences the myth that scientific inference is objective and free of personal prejudice. This is utter nonsense. All knowledge is human belief; more accurately, human opinion."

I want to briefly highlight some of the issues.[10] My fundamental belief is that any research approach that is absolute, narrow, and dismissive of alternative views is dangerous, arrogant, and generally inconsistent with scientific progress. On a fundamental level, the issues involve definitions of causality, correlation, structure, unobservables, counterfactuals, and conditioning factors. In policy evaluations, a fundamental problem is that individuals cannot both participate in the policy and not participate (be both treated and untreated). If individuals differ in ways that cannot be observed (unobserved heterogeneity), inferential problems may arise. Despite arguments to the contrary, recognition of these issues (identification, randomization, policy evaluation, etc.) is far from new or original. In fact, these very issues drove much

[10] Much of what I have to say is drawn from the work of Heckman and Pinto (2012) and Deaton (2009).

of the early work on identification and evaluation of policy effects undertaken by the Cowles Commission and such pioneers as Frisch (1933), Haavelmo (1944), Klein (1946), and Koopmans (1947).

I first want to review a few fundamental econometric points. Consider a standard structural model:

$$(10) \quad \begin{aligned} Y_1 &= \alpha_1 + \gamma_{12} Y_2 + \beta_{11} X_1 + \beta_{12} X_2 + U_1 \\ Y_2 &= \alpha_2 + \gamma_{21} Y_1 + \beta_{21} X_1 + \beta_{22} X_2 + U_2 \end{aligned}$$

where $E(U_i|X_i) = 0$. This can be solved for a reduced form of:

$$(11) \quad \begin{aligned} Y_1 &= \pi_1 + \pi_{11} X_1 + \pi_{12} X_2 + \epsilon_1 \\ Y_2 &= \pi_2 + \pi_{21} X_1 + \pi_{22} X_2 + \epsilon_2 \end{aligned}$$

where $\pi_{ij} = \frac{\beta_{ij}+\gamma_{ij}\beta_{ji}}{1-\gamma_{ij}\gamma_{ji}}$, and $\epsilon_i = \frac{U_i+\gamma_{ji}U_i}{1-\gamma_{ij}\gamma_{ji}}$, and so forth. Obviously, without more information we cannot identify causal effects $(\gamma_{ij}, \gamma_{ji})$ from the reduced form. If we can assume exclusion restrictions of the form of $\beta_{ij} = 0$ and/or $\beta_{ji} = 0$, we can identify *ceteris paribus* causal effects of Y_i on Y_j. Other restrictions on parameters or distributions of error terms can also achieve identification. Diebold (2013) notes that the structural model is equivalent to the reduced form with parametric restrictions imposed and that it is "… a delicate and situation-specific matter as to whether imposing structural restrictions on reduced forms is necessary or desirable." Deaton (2009) noted that we have assumed that the parameters (and distributions of ϵ_i/U_i) are necessarily invariant to changes in X and modifications of the distribution of U, or equivalently, that the effects of assigning a treatment to one individual are unaffected by treatment assignments to others. Frisch (1933) called this "autonomy" and the work of the Cowles Commission came to call this the "stable unit treatment value assumption (SUTVA)." One possible violation arises in the familiar "Lucas Critique," where parameters may change with the introduction of a program or policy, thereby violating the SUTVA assumption.

If we are interested in only a portion of the structural model, we may make use of the reduced form to obtain "instruments." In such a case, we are necessarily ignoring part of the larger structural model. The instrumental variable must satisfy two important conditions: $E(U_i, X_i) = 0$ and $E(Y_j, X_i) \neq 0$ (external/exogeneous and relevance). Many argue that exogeneity is usually only possible in cases of a controlled, randomized experiment and/or a "natural" or "quasi-natural" experiment that involves random/exogenous assignments to treatments, allowing identification of what Angrist and Imbens (1994) call the "Local Average Treatment Effect" (LATE). Examples include a controlled experiment where the researcher assigns treatments, purely external variation (e.g., weather, quarter of birth, etc.), or an exogenous policy change.[11] A number of other paths to identification are also possible, including matching estimators (difference in difference and propensity score estimators), restrictions on distributions of errors and/or treatments (covariance restrictions, quantile restrictions, intervals, mixing distributions), stratification (instruments give restrictions on strata identification), other restrictions on structure (monotonicity, thresholds, etc.), and recursive structures.[12]

These arguments, as stated, are factually correct and unassailable. In practice, however, the facts may not conform to the arguments, especially in our sometimes contrived efforts to make them fit. A few examples bear mention. First, an obvious point is that what appears to be a randomized program assignment may not actually be random due to unobserved heterogeneity. Because we only glimpse a small part of a system, it is often impossible to be confident about what we do not know. A famous example of purely exogenous variation was provided by Angrist (1990), who argued that the draft lottery number of Vietnam veterans is a perfect exogenous instrument for identifying the returns to schooling. However, as Wooldridge (2002) noted, the instrument may not be exogenous if how veterans responded to the draft depended on their returns to schooling. In terms of agricultural policy analysis, some have argued that the 1996 Farm Bill represented an external quasi-natural experiment. However, it is also likely that favorable market conditions and the political balance in the U.S. Congress motivated the 1996 Act, thus raising important questions about the exogeneity of the provisions of the Act. The relevance of instruments

[11] Does such an exogenous policy change ever really exist?

[12] See Heckman and Pinto (2012) for an extensive discussion of alternative approaches to structural identification of policy effects.

may also be critically important. Bound, Jaeger, and Baker (1995) found that modest departures from exogeneity may increase bias enormously when the instruments are weak. Purely exogenous weather variation is often noted as providing exogenous instruments. However, if producers' reactions to weather shocks depend on market conditions, the optimality of the instrument may be questionable.

My basic point is that no methodology is perfect and a tendency toward dismissing any approach that does not conform to one's own view of perfection results in flawed research. I would conclude this digression on identification with two appropriate quotes. Deaton (2009) noted that: "… randomized controlled trials cannot automatically trump other evidence, they do not occupy any special place in some hierarchy of evidence, nor does it make sense to refer to them as 'hard' while other methods are 'soft'. These rhetorical devices are just that; a metaphor is not an argument."

Finally, I asked one of my own econometrics professors, whose opinion I hold in the highest regard, for his views on the debate. Ron Gallant responded thusly: "What bothers me the most about the natural experiment obsession is the corollary that structural models are of no value. For myself, I do not see how a science can advance without serious use and enhancement of structural models. True, they will have to get more complex over time to match observational data, as in physics and climate science, but we have the computational equipment and algorithms to deal with serious models."[13]

Concluding Comments

I have attempted to outline a few selected points of importance to the ongoing quest to understand the effects and implications of agricultural policies by way of applying empirical techniques. My discussion focuses largely on examples drawn from the federal crop insurance program. Although this undoubtedly reflects my own biased interests, this program continues to grow in prominence and offers some rather unique opportunities for empirical work that actually does have real and tangible impacts on policy. I have made a particular case for a wider consideration of dependence modeling and have provided a few cautionary recommendations that we be careful to communicate our maintained hypotheses, pre-test analysis, and the precision of our empirical estimates. Finally, I have pointed to the ongoing debate over identification and structure as an example where I believe absolute opinions may result in a wholesale rejection of alternative approaches to analysis. There is ample room for alternate views and approaches, and absolute opinions, even when they are based upon unassailable facts, have the potential to stifle the progress of knowledge. At the minimum, such dogmatic views likely diminish the value of our research output for policymakers and most certainly make our professional lives and interactions as applied economists less interesting.

[13] Personal communication, September 11, 2012.

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