What Practicing Agricultural Economists Really Need to Know About Methodology

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There has always been considerable methodological diversity among agricultural economists. Nevertheless, about a quarter century ago, a broad methodological consensus seemed to be emerging, especially among the younger members of our discipline. We sought to do positive economics, to apply the scientific method in pursuit of objective knowledge about the empirical phenomena in our domain. We believed that our discipline was on the right track when seeking to pose a refutable hypothesis and test it with empirical evidence, and at its very best when the hypothesis had been deduced from a well-articulated economic theory, so that the outcome of the empirical test could potentially challenge the theory itself.

Most agricultural economists made no claim to have read deeply in the philosophy of science or even in the specialized literature on economic methodology. Our methodological views had been gleaned mostly from the introductory chapters of economics textbooks and the off-hand comments of our teachers and mentors. That, of course, is not a criticism of our profession: things would be fine, so long as the methodological orthodoxy that had trickled down to the practitioners reflected a well-articulated consensus among the specialists in philosophy of science and economic methodology.

The practitioners occasionally picked-up discordant signals. First, there were internal inconsistencies in the positivist-empiricist consensus. It became clear that the gurus of positive economics, Robbins and Friedman, had in mind methodologies very different from one another, and that neither methodology was consistent with logical positivism. Also, while many of us assumed Popper’s falsificationism was a cornerstone of positivist methodology, Popper himself (1959) saw it as a devastating attack on logical positivism. Second, agricultural economists were sometimes disturbed by the inconsistencies between the orthodox methodology of economics that enshrined scientific objectivity and the pragmatic ideology of the land-grant college that emphasized science in the service of humankind.

Beginning in the 1960s, the orthodox methodology came under sustained attack from philosophers and historians of science. Kuhn challenged Popper’s heroic model of science, with its bold conjectures and unflinching refutations. Kuhn’s world of normal science is neither bold nor heroic, and the occasional scientific revolutions are not exactly rational processes. Lakatos leaves us facing only the prospect of long wars of attrition among rival but noncomparable research programs, each of which may pass through periods of amendment, advance, and degeneration. Feyerabend (1975) argued exuberantly that the acquisition and validation of knowledge is not a strictly rational process: therefore, it is a foolish project to develop a prescriptive methodology.

By this time, Popper himself was moving away from the falsificationist school that had formed around his earlier work. At the end of his long and productive career, Popper was willing to defend only critical rationalism, clearly a much less ambitious methodology than the strict falsificationism of his youth, or his mid-career scientific objectivity.

Recent writing on the methodology of economics reflects these trends in the broader philosophy of science literature. Caldwell (1982) considered the positivist-empiricist consensus and the various critiques thereof and concluded with a plea for methodological pluralism. No particular methodology has unchallengeable claim on our allegiance, and we would do well to remain

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1 Among agricultural economists there have been and currently are honorable exceptions who read, write, and teach seriously about methodology.
open to a diversity of methodological approaches. McCloskey’s (1983) attack on the orthodox methodology was fundamental and sweeping. There is no scientific method, no prescription for acquiring warrantable knowledge. All we have is rhetoric, the art of argument. And there is no absolute standard for economic truth; the best economic ideas are simply those that are the most convincing.

McCloskey had expressed the hope that his concept of rhetoric would liberate economists from the artificial restraints of the orthodox methodology. Yet many practicing economists reacted with skepticism. Some worried that McCloskey had legitimized the kind of advocacy and propaganda from which scholarly economics had struggled so long and hard to separate itself. Even those who had some sympathy for McCloskey’s project, e.g., Solow and Hoover (Choi), were obviously disappointed that McCloskey had not offered more specific instructions to guide the search for economic knowledge.

Some economic methodologists were similarly disturbed by the decline of prescriptive methodology. Redman dismisses Kuhn and Lakatos as offering little more than “science as consensus.” Blaug reluctantly concedes that a strict positive-normative distinction is untenable. Nevertheless, he argues, it is best that economists behave as though they believe in such a distinction; such behavior would lead to the doing of better economics. Hausman and McPherson, in a brief article directed to agricultural economists, assert that there is (has to be) more to methodology than McCloskey’s rhetoric.

Agricultural economists whose interests lie in more practical matters have been unable to escape the fallout of these developments. Where only two decades ago there seemed to be a growing majority backing some sort of positivist prescriptive methodology, that consensus is now in disarray. More threatening to some is that a strong contender in the current methodological disputes goes by the name “rhetoric,” ridicules the very idea of prescriptive methodology, and (in the eyes of some critics) is relativist to the core. A common reaction among the practitioners is that it is one thing to recognize the problems with the prescriptive standards that emerge from positivism and scientific objectivity, but quite another to embrace a methodology that seems to impose no standards at all.

My objective is to examine the state of prescriptive methodology in a post-Popper, post-McCloskey world, focusing always on what practicing agricultural economists really need to know in order to go about their work. However, before getting to the heart of the argument, there are some preliminaries to take care of.

First, what do practicing agricultural economists hope to get from methodology? Hausman suggests that what economists want from methodology is theory appraisal: how can we tell whether a particular economic idea is good science? I think we want also appraisal of method and procedure: how can we tell whether a particular procedure will lead us to a clearer recognition and a better explanation of economic phenomena, i.e., is good method? In addition we want prescriptions, recipes for success, rules that will guide us in acquiring warrantable knowledge. Essentially, then, those practitioners who are neither serious scholars in methodology nor hobbyists therein make relatively narrow demands of methodology. They want appraisal and prescription.3

Second, what was the methodological consensus? McCloskey (1983) defines the methodological consensus as “modernism.” He defines modernism in terms of eleven propositions (some of which contradict others in important ways), such that most modernists believe in most of these propositions.4 Reductionism, empiricism, positivism, falsificationism, scientific objectivity, the belief that science is a human undertaking fundamentally different from all others, and the belief in a specifyable scientific method, are all elements of modernism.

The essence of the consensus methodology is its obsession with demarcation and prescription. Demarcation is the notion that there ought to be very clear and firm boundaries between science and other things that human beings do, and that it is the task of methodologists to define the things that make science different. This leads easily into prescription because if one can define what makes

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1 The word “rhetoric,” while it has an honorable history, has accumulated some nasty baggage along the way. The dictionary on my desk gives equal billing to “the art and practice of argumentation,” which is close to what McCloskey has in mind, and “inflected and grandiose oratory.”

2 While practitioners’ demands may be narrow, the methodologist would need to conduct a broader enquiry to respond to those demands. Epistemology (what are facts, truth?) is necessary for methodology (how does one have the facts, the truth?). But it is not sufficient: claiming to know something about the nature of knowledge is not the same as knowing how to create new knowledge.

3 As Backhouse points out, there are other definitions of modernism, clearer than McCloskey’s, in the literature.
science different, then those things that make science different can be reformulated as imperatives: a set of rules by which new science ought to be done. Rationalism, empiricism, and the twentieth-century attempts to synthesize them (such as logical positivism, falsificationism, and logical empiricism) share the common objectives of demarcation and prescription. So I shall characterize the consensus methodology as demarcationist prescriptive methodology (DPM).

Third, what could a DPM do for practitioners? Assume that economists are confronted with arguments that take many forms—logic, mathematical models, empirical estimates of many kinds, calculations of efficiency and distributional outcomes, normative claims—and come in a wide variety of qualities. Assume, also, that we have the scholarly tools of rhetoric and criticism, as well as the specialized tools of economics and related disciplines, available to help us confront bad arguments and construct better ones. Then, what could a DPM do for us?

The useful service that a DPM could perform is to provide reliable shortcuts. After all, if every argument has to be taken seriously, at least at the outset—as rhetoricians seem to suggest—the rhetorical approach entails a lot of hard work. If it can be established that some things are settled in principle, then we can reliably resolve particulars by reference to these established principles. Incorrect or irrelevant arguments, failed theories, unfruitful methods, and unreliable results could be eliminated a priori. Historically, the search for a DPM has been a search for exclusionary principles: rules that eliminate certain kinds of arguments on principle. Without simple and robust exclusionary rules, it is much harder to identify "unscientific" practices or to claim that science is distinctly different from other forms of scholarship.

Can a Satisfactory DPM Be Developed?

Instead of asking what might be lost by abandoning DPM and relying on rhetoric or critical rationalism, consider the reverse question: starting with the standard intellectual tools of broad-based scholarship, would we want to add some kind of DPM to guide the doing of science? Let us consider the major DPMs as candidates for adoption.

Logical positivism

The most ambitious DPM developed thus far is logical positivism. The earlier rationalist and empiricist methodologies had sought to liberate the people from superstition and dogma. Rationalists would accomplish this by establishing the power of the mind to arrive at the truth through correct reasoning. Empiricists argue that warrantable knowledge about the material world is accessible to the human senses; thus, a scientific method based on systematic observation would lead people to the truth.

The logical positivists combined rationalism and empiricism to produce a methodology that, they hoped, would unify science and rid it of all metaphysical elements. Their program was based on four tenets: (i) all complex propositions can be derived from elemental propositions; (ii) for every elemental proposition, there corresponds a sense-observable elemental fact; (iii) a statement is meaningful if a method of verifying it can be described; and (iv) the difference between science and nonscience is identical to the distinction between meaningfulness and meaninglessness or between sense and nonsense. Tenets (i) introduces the rationalism and (ii) the empiricism. While (iii) and (iv) provide the demarcation between science and nonscientific thought, and place science on a pedestal. Tenets (i)–(iii) provide an unambiguous set of instructions for doing science.

While logical positivism promises a powerful DPM, it has three major problems. The first is the insistence that theories be constructed of observable elements. The body of science, at the time of the logical positivists, included the atomic theory of chemistry and the gene theory of inheritance. Both disciplines were making good progress with these theories, it seemed, although no one had yet seen an atom or a gene. The second problem is the insistence on empirical verification despite Hume's earlier critique of induction. The third problem is the insistence on a one-to-one correspondence between the elements of theory and observable facts. The language of theory includes its elements and axioms and the rules of logic by which they are manipulated. It is not logically essential that a theory be about the real world. But, if that is the objective, the elements of the theory need to be linked to real-world objects via an observation language, i.e., a dictionary (Campbell, Harre) or a set of correspondence rules (Brown). However, theories are necessarily simplified abstractions, while phenomena are complex and multifaceted. It follows that no unique observation language exists (Brown, pp. 46–48). It is characteristic of different theories—and different paradigms (Kuhn) and scientific research programs (Lakatos)—that they establish for their
own good reasons different observation languages. Thus, the climactic test in which one theory is vindicated and its competitors are defeated is unavailable: if the competing theories are meaningfully different, their different observation languages will render them noncomparable (Feyerabend 1962).

**Falsificationism and Scientific Objectivity**

Popper solved the first and second problems of logical positivism. To resolve the first problem, Popper argued that it is not essential that theories be constructed of observable elements. It is enough that theories have observable consequences. This preserves the link between theory and observable evidence, and expands the set of theories to which it applies. This Popperian amendment extends, rather than shrinks, the scope of positivism.

Popper, most famously, resolved the problem of verification by replacing it with falsification. He showed rigorously that no finite series of confirming observations would verify a general statement. However, there is an asymmetry between verification and falsification: a single contrary observation is sufficient to falsify a general statement. Popper claimed that this totally destroyed logical positivism; and he thereafter refused to use the word “positivism” in any characterization of his own methodology.⁵

By mid-career, Popper (1957) had developed his concept of scientific objectivity, which he defined as a process involving (i) posing refutable hypotheses, (ii) testing them with relevant evidence, and (iii) reporting the hypotheses, the tests, and their results in a manner accessible to any interested person (Castle). This, too, is a strong DPM, offering a strict demarcation between science and nonscience⁶ and a clear prescription for doing science. However, it too had major problems.

First, the Duhem-Quine thesis (which Popper recognized) denies the possibility of refuting a hypothesis unambiguously. Because an empirical test of a theoretical proposition requires auxiliary assumptions, it is always possible to preserve the theory by attributing an empirical anomaly to the failure of an auxiliary assumption.

Second, while strict falsificationism may serve to weed out false conjectures, it does not provide for the accumulation of warrantable knowledge. This problem worried Popper deeply. He worked for many years to establish the concept of verisimilitude—conjectures that had survived many attempts at falsification could be said, for that reason, to possess the property of verisimilitude, or truthlikeness—before eventually abandoning the project (Caldwell 1991). Falsificationism tells us what not to believe, but it does not tell us what to believe, even tentatively.

Third, falsificationism does not solve the problem of noncomparability. Among the important auxiliary assumptions for hypothesis testing are those that specify what counts as evidence (Gleymour). Competing theories, with their different dictionaries or observation languages, will have different evidentiary requirements and different interpretations of whatever evidence is brought to bear. The direct confrontation of competing theories, with the evidence as final arbiter, is denied.

**The Methodology of Scientific Research Programs**

Lakatos, a former student of Popper, attempted to salvage something of Popper’s scientific objectivity—the concept of science as a rational process and the centrality of testing in that process—while recognizing the noncomparability problem and accommodating the growth of knowledge. He offered important amendments to the Popperian scheme: research programs consisting of a “hard core” of propositions that are untested (by convention) and perhaps inerently untestable, and a “protective belt” of derived propositions that are tested; criteria for amending refuted propositions (Popper had ruled against any and all amendments as “immunizing stratagems”); criteria for judging whether a research program is advancing or degenerating; and contests, between rival but noncomparable research programs, viewed not as climactic battles but as wars of attrition. As with Kuhn before him, many practitioners found Lakatos quite plausible and congenial, and his methodology enjoyed a decade (beginning about 1976) of popularity among economists (Redman).

However, the bottom line is that Lakatos’ attempt to find the middle ground failed to satisfy either the rationalists (Redman) or those who were most strongly opposed to the DPM project per se (Feyerabend 1975, McCloskey 1983).

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⁵ Others (Adorno et. al.) regard falsificationism as cleaning up a rather modest detail in a positivist agenda that Popper continued to pursue.

⁶ Popper (1957) uses his demarcation criterion to question the scientific status of Adler’s psychiatry and Darwin’s theory of evolution.
We are left with the failure of the aggressive DPMs—logical positivism and strict falsificationism—and the unsatisfactory outcome of Lakatos’ attempt to develop a DPM responsive to the criticisms of the aggressive DPMs. This, of course, is not sufficient to demonstrate the impossibility of a convincing DPM. Nevertheless, I am convinced that the noncomparability thesis is valid, and suspect strongly that it provides insurmountable obstacles for a DPM (Randall 1985).

Post-DPM Methodology

Given that we have no satisfactory candidate to adopt as a DPM, it makes sense to take another look at McCloskey’s rhetoric and Popper’s late-career critical rationalism. These methodologies are non-demarcationist—they appeal to intellectual and scholarly processes that apply not just to science but across the board—and both have been criticized for providing relatively little in the way of prescription. But, perhaps there is more to rhetoric and critical rationalism than has been recognized by the critics.

Rhetoric

In a nutshell, the rap on rhetoric is that, in the end, it offers little more than “good economic argumentation is whatever in fact persuades economists” (Hausman, p. 123). That is, persuasion is the test, and economists are those who have to be persuaded. By emphasizing that economists should be open to a wide variety of kinds of argument, McCloskey has attracted charges of relativism, i.e., the claim that any argument is as good as any other. By appealing to Rorty, McCloskey seemed to endorse the view that knowledge is socially constructed. By proposing literary criticism as a model for scientific discourse, McCloskey seemed to endorse the contention that meaning is provided neither by the text nor the reader but the interpretive community (Fish). If (i) knowledge is socially constructed, (ii) there are no general standards for argument except whatever persuades, and (iii) it is the interpretive community (or discourse community) that must be persuaded, what is to protect us from a proliferation of discourse communities, each proclaiming its version of knowledge secure from criticism from the outside, and demanding to be accepted by outsiders as a legitimate school of thought (Backhouse)?

McCloskey brought some of this upon himself by his refusal throughout the 1980s to say very much about what constitutes good argumentation. In 1990, he proposed what he called the rhetorical tetrad: fact, logic, metaphor, and story. He made much of the role of conflict: one learns something by opposing conflicting metaphors and stories. While few would deny this, it remains rather a broad-brush treatment that places McCloskey in an intellectual tent large enough to include Hegel and the late-career Popper.

Let me suggest a way to defend rhetoric from the charge of relativism. To make this case, it is necessary to deal with the role of appraisal in rhetoric, and the problems of persuasion and defining the discourse community.

To define rhetoric, McCloskey (1983) relied on Booth, who offered the art of discovering good reasons, of improving beliefs through shared discourse, and of finding what really warrants assent because reasonable people ought to be persuaded. There is ample room here for discovering and applying principles of appraising arguments.

The role of persuasion in rhetoric is worrisome to those who fear it might open the door to advocacy and propaganda. However, the problem of persuasion pervades all serious discussion of ideas in a liberal society. Assume the human mind demands a reasonable degree of autonomy; that is, assume that, in the end, people make up their own minds.7 Then any argument, in order to be successful, must be persuasive. This applies even to methodological arguments; persuasion is not a special problem for rhetoric. In addition to the autonomy assumption, assume people have a tendency to listen to reason, sort through the arguments, and arrive at the right conclusion. Without this assumption, rhetoric would be a disaster; and without it, people would be likely to adopt false and unhelpful prescriptive methodologies. In a liberal society, both rhetoric and prescriptive methodology rely on the assumption of a human tendency to be persuaded by the better argument.8

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7 If this is problematic, consider assuming the contrary.
8 But, it must be conceded, they rely on this tendency in different ways. It is true that in order for the DPM project to succeed, people must be persuaded, correctly, by a valid DPM. However, the purpose of DPM, once adopted, is to provide a small and coherent set of general principles by which to resolve particular questions of method, evidence, and inference. With a valid DPM in place, its prohibitions and exclusionary rules would serve a valuable legislative and policing function, to minimize error and deviance and to expose them when they occur.
The concern that narrow, self-selected discourse communities could establish themselves as arbiters of knowledge without scrutiny from the outside is overblown. We certainly observe small groups of scholars attempting to establish new paradigms. Recent examples include ecological economics and human values in agriculture. Nevertheless, these paradigms and discourse communities are permeable to stimuli from the outside (just as, to use another example, game theory has penetrated philosophy and most of the social sciences). To lapse into Lakatosian language, among these emerging research programs, those that are most successful at withstanding criticism and incorporating useful ideas from the outside will progress while the others degenerate. The ultimate discourse community is humankind.

Regardless of whether McCloskey has himself done it, one can develop a strong case that rhetoric is not inherently relativist. That is, one can adopt a stance of a priori openness to many different kinds of argument and evidence, without committing oneself to the belief that any argument is as good as another.

**Critical Rationalism**

While Popper's falsificationism is ultimately an untenable methodology, Popper has developed and promoted many useful ideas. Two, in particular, stand out. The mere search for confirmations of one's prior beliefs is a timid and unambitious kind of science; the unexpected observation and the unlikely explanation are the most interesting. Science is a social process, so that objectivity derives not so much from the commitment of individual scientists to be at all times objective and impartial as from the openness of the process to criticism from any interested person.

Popper's (1974, 1983) late-career critical rationalism backed away from strict falsificationism, while retaining a central place for bold conjectures, unflinching criticism, and science as a social process. However, critical rationalism prompted from many methodologists and practitioners much the same reaction as McCloskey's rhetoric: it doesn't say enough; surely there has to be more. Specifically, for those who had been nurtured on some version of DPM, it doesn't prohibit enough. As Caldwell (1991) observes, Popper would discard conjectures that cannot be criticized, but said relatively little about the critical process itself. His former student, Bartley, attempted to address this issue. He concluded that a theory is held rationally if (i) it contains no built-in devices for avoiding or deflecting critical arguments or contrary evidence, and its holder (ii) makes every attempt to expose the theory to criticism and (iii) does not maintain it in the face of cogent criticism. But this, still, provides little in the way of demarcation between science and non-science (Redman). While critical rationalism remains disappointing to those seeking a DPM, it nevertheless provides a framework useful in appraising arguments.

**Reasoned Discourse**

From here on, I shall be less concerned with what McCloskey and Popper actually wrote, and more concerned with what one can make of their ideas. The best elements of rhetoric and critical rationalism can be combined in an approach that I propose to call reasoned discourse: reasoned, to emphasize the rational, critical, and appraisive aspects; and discourse, a term that is consistent with McCloskey's concept of rhetoric and Popper's notion that the search for knowledge is a fundamentally social process.

**Meta-Methodology and the Tools of Reasoned Discourse**

Reasoned discourse commits its adherents to evaluate a broad range of arguments and the evidence on their merits. However, not all arguments and not all evidence deserve to be taken equally seriously. To submit all statements to the same scrutiny would be unwarranted, and too costly. Not every exercise in appraisal needs to start from scratch. The failed search for a DPM has nevertheless generated some useful precepts to guide the task of appraisal. I propose to call this body of precepts "meta-methodology." Meta-methodology differs from DPM in that it forgoes the claim to have discovered universal rules.
of demarcation. It does claim, however, that there is a fairly well-developed scientific method that provides some instructions helpful in appraising scientific conjectures. There is no claim that each of these instructions is universally serviceable or that those who would be considered scientists are obliged to follow each. Furthermore, some inconsistencies among the components of meta-methodology are tolerated because meta-methodology is viewed as an inventory of potentially useful suggestions for researchers, not a single, coherent methodological system.

From the rationalists, we have learned that logical coherence is a highly desirable property of an argument, and we have some well-established principles of logic to guide us. From the empiricists we have learned to respect the evidence, and to develop procedures (both experimental and econometric) to impose *ceteris paribus* and to come to terms with stochastic phenomena. The logical positivists taught us to respect the distinction between empirical and metaphysical propositions, just as the disputes that led to the dismantling of their school taught us that this distinction is not quite as simple as it might seem. From falsificationism, we learned to cherish the opportunities to conduct a definitive test of an interesting, refutable hypothesis, on the rather rare occasions such opportunities are presented to us. These are among the ideas central to meta-methodology.\(^{10}\)

In reasoned discourse, the tools of meta-methodology remain available and will be used on their merits. But reasoned discourse is not confined to meta-methodology; it also draws upon the whole body of precepts and methods of scholarship and rhetoric. The net effect of replacing DPM with reasoned discourse, it seems to me, is to unify scholarship by drawing science (however reluctantly) back into the fold.

**The Prospect: Local Provisional Methodologies**

There remains, nevertheless, merit in the idea that a prescriptive methodology might provide shortcuts for sorting out the evidence and the arguments. Rather than a universal scientific method or a DPM, it may be possible to develop local prescriptive methodologies, methodologies for solving the peculiar problems of particular fields. Much of this methodological work would need to be done by insiders, who are familiar with the details. But, in keeping with the idea that discourse communities are permeable and the ultimate discourse community is humankind, it would not be just an inside job. Local methodologies would be disciplined by the critical processes and reasoned discourse of society at large. Meta-methodology is both broader and narrower than any local methodology: broader because a local methodology will adopt some but not all of the precepts of meta-methodology, and narrower because local methodology will reach beyond meta-methodology to include the broader scholarly devices of reasoned discourse. Finally, at this level some things can be treated as settled for now, but one suspects that little is settled forever. So, instead of a universal scientific method, the realistic prospect is for local provisional methodologies (LPMs).

**LPM: Illustration 1, Econometrics**

The development of local provisional methodologies may be illustrated by the example of econometrics, a subject of concern to practitioners in all fields of agricultural economics. Haavelmo’s synthesis of economic theory, probability theory, and classical hypothesis testing was adopted by the Cowles Commission. The Haavelmo program consisted of (i) specifying an economic model, (ii) specifying a probability model to capture the indeterminism of the data, (iii) using Neyman-Pearson testing theory to test pre-specified hypotheses, and (iv) applying probability theory to generate parameter estimates with good properties and forecasts with interpretable stochastic variability. This program quickly became the methodological standard for economics (Heckman) and agricultural economics (Judge).

This econometric methodology has several interesting characteristics. It focuses on model testing to the exclusion of model selection, and on theory testing rather than theory development. Theory and model come prior to data. Learning from data is denied; new data may be used only to test old models. The list of explanatory variables must be unique, complete, small in number, and observable. The theory and model to be tested must be completely specified and parsimonious: “quantity demanded is determined by prices, income, and who knows exactly what else; let’s take a look” just will not

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\(^{10}\) A more complete account of the contents of meta-methodology could be compiled by reviewing several reputable textbooks on scientific methods.
do. Thus, the Cowles methodology makes existing demands on economic theory. It has no patience with the approach of starting with a relatively permissive theory and amending it according to what is learned from the data. All of this is consistent with the strict falsificationism of the young Popper. It is also consistent with the idea of DPM, in that it makes an unambiguous claim to be the one right way to do (its particular) science. It is a local methodology for econometrics, but it displays the influence of (what were at the time) recent developments in probability theory and the philosophy of science.

In the intervening half-century, econometricians have come to accept that economic theory does not strictly forbid very much and it therefore makes little sense to prespecify a unique model and use data only to test that model. Such an approach takes the model too seriously and the data not seriously enough. During the same time, there has been an enormous improvement in computer capability, and new techniques useful in model selection have been developed. Defenders of the Cowles methodology were appalled by these developments in econometric practice, and terms such as data-dredging were used to suggest the unsavory nature of what was happening.

By now, many econometricians have become much more comfortable with model selection, specification search, and learning from the data. Nevertheless, there remains a variety of views, each enjoying the support of prominent individuals or schools. Leamer discusses the characteristics and validity of various kinds of specification searches and, in doing so, does much to relieve the phobia about specification searches.

Hendry and Hendry et al. argue that one should specify the most general model—general with respect to both functional form and the variables included—and “test down” to the best model. The Bayesian approach, with its concept of using the data to update one’s expectations, has enjoyed a revival. There remain defenders of classical, Neyman-Pearson inference. Heckman finds it unfortunate that the Neyman-Pearson paradigm continues to be so influential when, by now, it should be viewed as just one of many competing views about how to do inference; points out that Hendry et al. have not demonstrated rigorously that a unified framework for model testing and model selection can be based on Neyman-Pearson methodology; and believes that Hendry and the Bayesians impose excessively rigid, although very different, procedures for learning from the data. To Heckman, the development of econometric methodology is an ongoing process, and it should continue in the direction of learning from the data.

It is important to point out that these developments have not been a mere accommodation to model selection, specification search, and learning from the data. Along the way, methods have been developed, techniques have been improved, and errors have been eliminated. For example, testing procedures now recognize the jointness of sequential hypotheses when “testing down” from the most general model. It has, quite properly, become fashionable for economics journals to omit significance levels when publishing t-statistics, recognizing that joint hypotheses may invalidate the standard significance tables.

These developments in the LPM of econometrics did not occur in a vacuum. The current situation in econometrics is pluralistic, and the trend has been away from the idea that there are simple and universal standards for appraising econometric claims. Thus, econometric developments have reflected developments in the philosophy of science, the methodology of economics, and the theories of economics and statistics. While it is likely that econometricians write mostly for, and talk mostly to, each other, it seems true also that the econometrics discourse community has been penetrated by stimuli from a broader scholarly community.

Most of the hard work that underlies these developments was done by econometricians, theoretical and applied. While I have cited only a few of the most prominent authors, many more were involved in providing the vast body of empirical studies and technical innovations upon which the emerging interpretive generalizations are being built. Furthermore, while my account makes it all sound like a battle of the giants, these very same methodological issues were the subject of discussion in seminars, graduate examinations, and around the coffee pot, wherever people take an interest in econometric applications. At the local level, then, methodology is to some degree the work and the avocation of most practitioners. Finally, I would claim that—messy and uncertain as the process has been—econometrics is gradually getting it right.

Has econometrics developed local, provisional methodologies? Clearly, they are local; and, with several competing models of inference and Heckman’s strong feeling that none of the current contenders is the final word on the subject, clearly they are provisional. But are they
methodologies? Those for whom methodology means a DPM would worry that econometrics does not currently provide uniform and convincing rules. But an LPM is not required to provide uniform rules. It is required only to set some bounds on the discourse, to settle at least some issues (so that each argument does not have to start all over again, at the very beginning), and to improve the quality of discourse over time. The LPM of econometrics, it seems to me, is accomplishing these tasks.

**LPM: Illustration 2, Contingent Valuation of Nonmarket Goods**

The contingent valuation (CV) method for evaluating gains and losses of nonmarketed goods and services relies on self-reports of value (e.g., maximum willingness to pay) or of value-revealing contingent choices (e.g., buy/not buy at a stated price; or vote yes/no at a stated policy cost). For this reason, it encountered immediate skepticism from economists who often distrust data about “what people say, rather than what they do,” and from other social scientists who worry that the CV process imposes a welfare change measurement framework upon citizen-respondents who might more naturally invoke some other way of thinking about their preferences or values concerning public policies.

Nevertheless, the CV research program developed quite rapidly through the 1970s and 1980s. More and more scholars became involved; theory, methods, and techniques proliferated, and a fragile consensus began to emerge concerning what worked and what did not; refereed articles appeared in mainstream as well as specialized economics journals; and the CV discourse community established patterns of communication with researchers in incentive theory, econometrics, psychology, and survey research. CV researchers sought to test methods and validate results in a variety of ways. Tests were conducted for internal consistency, replicability, and the presence of various biases. External validation would be more convincing, but external validation is inherently elusive in nonmarket research. Nevertheless, various imperfect external tests were applied: e.g., consist-

tency of empirical results with theoretical expectations; and comparability of CV results with welfare estimates from alternative techniques such as hedonic price analysis and the travel cost method, techniques that have their own difficulties. While these tests generated some persistent results that seemed anomalous, the body of findings supportive of CV was impressive enough that applications continued to increase and government agencies began to accept CV as an appropriate technique for use in project evaluation.

In the 1980s, federal laws and regulations permitting public trustees to claim monetary compensation for environmental damage allowed claims for lost passive-use-values (e.g., existence values) and recognized CV as an acceptable valuation method. Private individuals and corporations could find themselves liable for compensation payments based on CV results. The Exxon Valdez oil spill of 1989 brought the issue into sharp focus: environmental damages can be enormous, and lost passive-use values may well dominate other categories of damage when distant and pristine environments are involved. CV had become too important to leave to a small discourse community of practitioners.

The Exxon Corporation put to work a group of scholars, mostly economists, to examine CV critically. The leading lights among this team were outstanding mid-career economic theorists and econometricians from elite economics departments, who had little prior involvement with CV. In April 1992, this group released their findings, trenchantly critical of CV, at a public meeting in Washington, DC (Cambridge Economics, Inc.). Soon afterward, the National Oceanic and Atmospheric Administration convened a “blue ribbon panel” of experts, co-chaired by Nobel Laureates Kenneth Arrow and Robert Solow, to report on the validity of the passive-use value concept and CV. In the panel report (Arrow et al.), CV was endorsed, a result disappointing to its opponents. However, some CV proponents thought the report gave too much credence to recent attacks on CV.

This sudden and dramatic expansion of the CV discourse community, before a backdrop of litigation and unaccustomedly large compensation claims for a newly-recognized category of compensable damages, provides an opportunity to observe the reasoned discourse process in upheaval, before the dust settles. Much of what we see is unappealing. In some cases, impressive credentials have been substituted for careful scholarship, and sweeping generalizations turn

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11 My objective here is to provide an introspective but nonpartisan commentary. Nevertheless, readers will recall that I have been, and remain, deeply involved in this research program.
out sometimes to hinge on special-case models and relatively arbitrary research decisions about data handling and statistical methods.

One would expect such problems to be mitigated as the process of reasoned discourse continues. However, appraising the reliability of CV is inherently difficult and it seems that some of the standard suggestions from meta-methodology have generated at least as many problems as solutions. For example, meta-methodology promotes the external test, the refutable hypothesis, and the crucial experiment. Yet, these ideas have proven problematic in the case of CV. External tests against expectations deduced from economic theory have remained controversial, because economic theory is generally rather permissive, and because the economic theory of ideal valuation of public and policy goods remains incompletely specified. Case study findings that CV values are relatively insensitive to the quantity of environmental protection offered have been presented as devastating evidence against CV (Cambridge Economics, Inc.).

However, other observers question the CV procedures used in these tests, and point out that theory is unclear about how quantity-sensitive such values should be. Given that policy is inherently multi-dimensional, there is no basis for identifying a priori the particular quantity dimension(s) that should elicit the most sensitivity. Empirical results showing that willingness to pay for a package of programs is typically much less than the sum of WTP for the same programs evaluated independently have been interpreted, alternatively, as the predictable consequence of limited budgets and substitutability among programs (Hoehn and Randall, Hoehn) and as evidence of "embedding," a pathology that is an artifact of CV itself (Kahneman and Knetsch). In both of these cases, the empirical test against theoretical expectations founders on the imprecision of the theory, leaving CV proponents to protest overblown conclusions from weak tests while the opponents complain that proponents seem determined to explain-away any inconvenient results.

More mischief than good has come from attempts to test the hypothesis that CV is a reliable welfare change measurement device, or to perform the crucial experiment that would validate or invalidate CV. Given present knowledge, the hypothesis that CV is reliable is untestable: we don't have precise notions of the performance characteristics of an ideal measure; and there is enough variety among CV procedures that no test using a particular CV exercise would produce results applying to CV generically.\footnote{The hypotheses that "attitude surveys are reliable" and "public opinion polls are reliable" are no longer taken seriously. Such hypotheses are recognized to be untestable and perhaps meaningless. Research programs about measuring attitudes and public opinion have moved on to mapping the performance characteristics of alternative approaches and techniques. The CV program should follow suit.} Generalizations based on experiments comparing contingent voluntary contributions and contingent purchases with their actual counterparts have received a very mixed reception: for both theoretical and evidentiary reasons, it is unclear if such experiments have anything at all to say about the performance of the contingent policy referendum (a preferred form of CV). More generally, the concept of the crucial experiment is fraught with Duhemian confusion; that is, the side-conditions required to reach a definitive conclusion about the generic reliability of CV are so demanding that the interpretation of the test results will likely remain contentious. There has been altogether too much posturing about "crucial experiments" that turn out to be much less than that.

The present situation regarding CV illustrates some characteristics of LPs. First, a discourse community and the theories and methods that it develops cannot become influential without attracting scrutiny from "outsiders." Second, immediately following the recent and sudden expansion of the CV discourse community against a backdrop of big-money litigation, the quality of discourse and rhetoric is not comforting. Claims and counterclaims seem exaggerated, appeals to authority have been substituted for more reputable forms of argument, and a disturbing lack of both convincing argument and agreement among participants attends the crucial issue of what counts as evidence for or against the reliability of CV. And, this situation cannot be blamed entirely on the obtuseness or the ill-will of the participants: the questions are difficult, their answers are elusive, and insufficient time has elapsed to allow an informed consensus to emerge. Third, standard concepts from meta-methodology are insufficient to resolve all of the relevant issues. One must look instead to reasoned discourse to save the day, and one must hope that the controversies are eventually resolved by a preponderance of the evidence.

Concluding Comments

So, what do practicing agricultural economists really need to know about methodology? The
comforting certainties of the old methodological orthodoxy are gone and there is little hope that a new demarcationist prescriptive methodology will arise to replace them. There are those who worry that this development has made science and scholarship too easy, by weakening or repealing all of the rules. I believe that just the opposite is true: rhetoric, critical rationalism, and reasoned discourse are serious enterprises that imply a more painstaking form of scholarship.

Furthermore, prescriptive methodology is now largely a local enterprise for particular disciplines, schools of thought, paradigms, or research programs. Meta-methodology provides some principles and methods useful in appraising theories, methods, and empirical conjectures. However, it seldom provides ready-made solutions to local problems in applied economics; and the principles of meta-methodology, applied without subtlety, may serve only to restrict the scope of inquiry and increase the confusion. Local provisional methodologies draw upon all of the tools of scholarship (including meta-methodology), along with theories and procedures specific to the research program and its related disciplines. Despite the essentially local nature of methodology, the discourse community is and must be permeable to stimuli from the larger scholarly community. A research program that gains influence will assuredly attract scrutiny from the outside (perhaps rather abruptly, as was the case with contingent valuation of nonmarket goods and services).

Because researchers are the front-line methodologists for their own research programs, it follows that the serious study of methodology and rhetoric is desirable for researchers. Such study is unlikely to yield hard-and-fast rules for research, but is likely to yield a rich lode of insights that will raise the level of argument. Furthermore, it tends to make researchers more introspective about their own work, which is all to the good.

Finally, there is little reason to lament the demise of DPM. The failure of demarcationist methodology to establish a sharp boundary between science and nonscience tends to unify scholarship by bringing science into the fold. This is a blessing for agricultural economics, which was always an uncomfortable fit in the science box. Agricultural economics is at once a theory-based deductive exercise, an empirical science, an active contributor to the discourse on policy matters and ethical issues, and a learned profession offering analysis and advice to clients.

A framework of reasoned and critical dis-

course serves, among other things, to enlighten and improve our discussion of ethical and value questions. These issues pervade agricultural and resource economics; so, surely, it would be better for us to deal with them forthrightly. There is a great tradition of scholarship—which demarcationist methodology instructed us to ignore—that we can draw upon, and occasionally contribute to, in this quest.

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