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Agon and Ag Ec: Styles of Persuasion in Agricultural Economics

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It is a tradition that these addresses be given by an outsider. I speak as an outsider in praise of agricultural economics.

An outsider presuming to praise could be accused of condescension, but I assure you I come by a favorable opinion of the field honestly. I am practically an insider. Maternal relations own farms in Illinois, near Watseka. The same Roger Gray, of the Food Research Institute, whom President-Elect Johnston mentions in his paper, is my second cousin once removed. In England I have worked as an agricultural laborer. Once, in Vermont, I milked a cow. Ag econ is in my blood.

It is certainly in my brain: an economic historian had better think agriculturally, since the past was 80% agricultural. In a graduate course last spring, we spent a good deal of time discussing medieval sheep as manure spreaders, and I am finishing a book for Princeton on scattered parcels as portfolio balance in the fourteenth century. My education as an economist was much influenced by agricultural economists such as Theodore Schultz and by people working on the agricultural aspects of historical economics such as Robert Fogel. To cap it off, since 1980 I have lived in Iowa. That surely qualifies me as an expert on agriculture and its science, at any rate in the eyes of the *New York Times*.

My points of praise are four, with a moral to follow:

First, agricultural economics invented econometrics. You can look on this as a good thing or a bad thing; I consider it very good. The career of Holbrook Working alone would justify the claim, but one could mention, among others, the bevy of econometricians at Iowa State in the early 1940s, when that department was second only to Cambridge, England, among the world's collections of economics. Frederick Waugh served as president of this association in 1946 and was in the first graduating class of fellows; Mordecai Ezekiel was in the second graduating class. Marc Nerlove and Zvi Griliches cut their teeth on agricultural economics.

Second and more generally, agricultural economics takes economics seriously. When one thinks of the quintessential "applied economics," one thinks of calculations of the value of tobacco allotments or of the elasticity of demand for oleomargarine. One might claim that the perfect markets in most agricultural commodities were invitations to "believe in the market," and therefore to believe in the applicability of economics. But not everybody in agricultural economics believes in the market—Lauren Soth of the *Des Moines Register*, for example, another of your fellows and another student of T. W. Schultz, does not especially. Regardless of their ideology, agricultural economists, long before other economists, were serious enough about whatever argument was being used to put it up against the facts of the world. Economists are philosophers and engineers—certainly not the social physicists they imagine themselves to be. Agricultural economics was the first field of economics to take the engineering model seriously.

Third, agricultural economics, more than many other parts of economics, is serious about institutional details. The students of international trade, for example, hardly ever pause on their way to the blackboard to examine an institution. If economists are philosophers and engineers they are also social historians. Most of what economists do is tell stories about the recent past, explaining why the Corn Belt went bankrupt in the early 1980s or why agricultural policy favors bigger farmers (McCloskey 1990). To do so sensibly, they need to know what they are talking about.

Fourth, agricultural economics is therefore

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more open to other fields than many other parts of economics. The department at Iowa State was once the Department of Economics and Sociology, harboring rural sociologists and home economists. As Vernon Ruttan argued a long time ago, the permeability of agricultural economics has been its advantage.

The moral I want to draw arrives at my strange title, "*Agon* and Ag Econ." *Agon,* as in "ant*agon*-istic," is the Greek for "contest," as in the Olympic games. The Greeks, like American men, thought of their life in sporting terms, and so the word also meant any assembly where people meet to argue, as in a court of law or in a seminar at the academy. What is striking about the conversation of agricultural economics, viewed from the outside, and what may help to explain its success, is that it is polite—more polite than the conversation in macroeconomics, say, or even in economic history. There is amazingly little *agon* in ag econ.

You will know better than I what the reasons for this are. But, as an outsider to the field, I have a thought. I think the American and even midwestern origins of agricultural economics have something to do with it. Americans are less comfortable with *agon* than are Europeans. On this side of the Atlantic, we like to think of ourselves as getting together to raise barns and hold church picnics, achieving a common purpose by cooperating. The Civil War plays a surprisingly small part in the mythological life of Americans; and Canadians, who for this purpose share attitudes with Americans, never had a civil war. Europeans find such attitudes bizarre. Small wonder. Their civil war, which began in August of 1914, is only just coming to an end in 1990. Within Europe, other wars rage. The French since July of 1789 have not stopped fighting their revolution with each other. The recent bicentennial was an occasion for a flood of conservative reinterpretations. No wonder the Europeans, and most particularly the French, carry an antagonistic style of argument into the academy.

I would like to persuade you that this American and Canadian attitude is a good one, nothing to be ashamed of. In particular, the rhetoric of agricultural economics does not square with a European attitude toward argument. The European attitude shows up in economics in existence theorems and crucial tests of hypotheses—timeless, universal proofs using unreasonably narrow arguments. The form of argument came to prominence in the seventeenth century. Men had been killing each other in large numbers over such doctrines as transubstantiation, and it seemed therefore a good idea to discover grounds for certitude, even if narrow, that could prevent further bloodshed. Good for them. But the narrow arguments by themselves would not suffice to get you across a busy street in town. Properly, they would not change minds in the Ten-O'Clock-Club at the donut shop on Main Street. I would like to persuade you that the narrow and European styles of argument in economics should not persuade a reasonable person.

Actually, the most advanced thinkers in cognitive and computer science have grasped the point that common sense is required for thought. The computer scientist Doug Lenat, supported in Austin, Texas, by a consortium of big companies, has been trying for six years to teach a computer named Cyc the common sense necessary to handle the simplest real-world problems without human spoon feeding (Freedman). He reckons that Cyc will need 100 million pieces of information, 2 million of which have been fed in so far, with great labor. If the computer could read natural English, the learning would go much faster. But reading requires common sense, too, and in human terms Cyc is now only about four years old, too young for school.

The opposite view—that following some 3-by-5 card formula of "scientific method" is how to be a good scientist, or even a five-year old with the common sense necessary for reading, is prevalent among the normal scientists of most fields and even, I suppose, in agricultural economics. But the leading scientists do not buy into the childish hope for simplicity in life. The chemist and philosopher Michael Polanyi once characterized the 3-by-5 card notion as "voluntary imbecility." The psychologist Jerome Bruner, speaking of psychology in the late 1930s, wrote recently that, "For reasons that now seem bizarre, you *had* to convert contested issues into rat terms in order to enter the 'in' debates" (p. 29). The voluntary imbecility, this cutting off the richness of economic argument available to us if we do more than work our own little technique over and over again for the "in" debates, is slightly nutty. As we say in Iowa, it is a few bricks short of a load. The joke among psychologists these days about the narrowness of old-fashioned method is this: Two strictly behaviorist psychologists make love. One says to the other, "*You* enjoyed that. Did I?"

Neither I nor the cognitive scientists are saying that there is nothing at all in what I have called "European" styles of argument, or that to be properly North American we must become illogical. I myself am a quantitative economic historian, and I thrill to the blackboard arguments as much as the next guy (I say "guy" advisedly, because our female colleagues in economics do not seem to get quite as much of a kick out of them). The European—one might say especially French—error is to reduce all argument to one especially simple kind, the formal proof. It is Descartes's program of the past four centuries. In its own terms it has failed. No science has in fact gotten along with the blackboard proofs that the Cartesian method holds up as the ideal.

Blackboard economics has had a long run. Like modernism in architecture, it is coming to a dead end. But economics will prosper. We in economics are going to broaden our arguments, without throwing away any of the gains from European precision.

Our official rhetoric, however, expressed in journal articles, is still pretty much stuck in a philosophy of science current in Europe around 1930.

In the August 1989 issue of the *American Journal of Agricultural Economics* there are twenty-four articles. Of these, fifteen have the standard outline of formal model followed by a serious empirical implementation, almost invariably regression analysis. Four of the twenty-four have no formal model yet engage in serious empirical inquiry (all four of these also use regression analysis). One other is a review article. Only two of the twenty-four have a formal model without any gesture at empirical implementation, and only two more have a formal model with merely illustrative implementation, directed at the new method proposed rather than a problem in the world.

The ratio of articles with serious empirical work to articles with a merely theoretical purpose is typical of the applied fields, such as labor economics or economic history. But of course the ratio is well above that in the so-called general-interest journals of economics. Wassily Leontief, a famous friend of agricultural economics, recently calculated that over half the articles in the general journals of economics and sociology were theoretical. What do you suppose the share of such articles was in comparable journals of physics or chemistry? Ten percent.

Compare the 1989 issue with the *Journal of Farm Economics* (as it was called before 1968) in 1929, sixty years before. What are the rhetorical differences between agricultural economics then and now? The ten articles in the August issue of 1929 hardly overlap at all in type with those of 1989. Only one article is a formal modeling and simulation of behavior, another (by Howard Tolley) is a piece of empirical accounting. There are five articles offering policy assessments and proposals, usually with an accounting framework. There is one outlook piece, one institutional description, and one extended appeal for more fact collecting. Only the four nonmodeling articles out of the twenty-four in 1989 look much like any of the articles sixty years before.

Most of the 1929 articles, however, use quantitative thinking. It is false to say that economics has become more quantitative over the past sixty years. Counting, after all, has been the character of economics since its beginnings in political arithmetic three centuries ago. Indeed, what is apparent in 1929 is something largely hidden in 1989, although it is there to be seen if you look hard enough: namely, that economics depends for much of its arguments on accounting. Accounting is the master metaphor of economics, determining most of its quantitative findings. It is an accounting decision, for example, to value family labor on farms at market prices. The decision alters radically how we view the efficiency of family farming.

The most striking change in method down at the practical level is of course that virtually all the empirical work in 1989 uses regression analysis. This a little peculiar when you think about it. When we as economists make policy arguments, we use accounting, as I just said, together with simulation—all the way from back-of-the-envelope calculations of elasticities to formal simulations on computers. But when we seek the facts of the world, we pretend that only the "experiments" suitable to regression analysis are appropriate. I once had a graduate student who thought that the very word "empirical" meant "regression analysis on someone else's data." Regression analysis seems to have a tighter hold on the empirical imagination in agricultural economics than it has in other applied fields, probably because of the agronomical origins of the statistics. R. A. Fisher, who named most of them, worked at an agricultural experiment station.

There are some problems with this rampant regressionitis. It means that agricultural (and other) economists do not scrutinize the other parts of their quantitative rhetoric, such as the accounting systems that force most of the results or the data collections that allow quantitative thinking in the first place. The very word *data* shows up a problem. The word means in Latin "things given," which is the attitude of modern economists. Someone else is going to give them the facts. They would do better to think of the facts as *capta,* things to be seized and in the seizing examined closely for flaws.

And there is a quite serious, one might even say devastating, technical problem with the way economists use regression analysis, even the agricultural economists who pioneered its use in social science. Every one the twenty-one articles that use regression analysis in the 1989 issue of the *Journal* grossly misuse it. They take statistical significance to be the same thing as scientific significance. Professional statisticians have understood since, at the latest, 1919 that the two have little to do with each other. That a coefficient is statistically significantly different from zero says merely that a sampling problem has been solved. Some scientific problems are sampling problems, but most are not. Most are problems of how large is large. We decide as economic scientists whether a coefficient is large for our purposes; we cannot hand the task over to a table of Students-*t*. Some day, in other words, all the econometric work in the 1989 issue will have to be done over again because it depends on this confusion. Have you ever wondered why regression analysis in economics never seems to settle an issue as decisively as its rhetoric would lead you to expect? Here is why: statistical significance has almost nothing to do with scientific significance (see Boring; Neyman and Pearson, p. 296; Wald, p. 302; Arrow; Griliches; Freedman, Pisani, and Purves, pp. 501, A-23, and throughout; Kruskal; Leamer; McCloskey 1985; Denton).

The regression analysis, though, as much as agricultural economists love, honor, and obey it, is merely a detail of method. A deeper content analysis of the articles in 1929 and 1989 would show them to be more similar than my listing of nonoverlapping types suggests. Agricultural economics is still concerned at bottom with how farmers behave and whether their behavior is good for them or for anybody else.

Yet, the rhetorical spirit of the articles definitely changed in sixty years. The big change is the rise of Cartesianism. That is to say, the big difference between 1929 and 1989 is, oddly, philosophical. The push for "testable hypotheses" is palpable. Just below the surface in 1989 lies a commitment to a bankrupt model of scientific method. We economists all think that what we do is similar to what physicists do. Actually, we know next to nothing about how physics operates as a field. An article in the magazine *Science* in the fall of 1989 told how the physicists at the new Santa Fe Institute are amazed at what the economists there consider to be science. The economists, who are mainly theorists, think that science involves mathematical proofs of the theories and then the equivalent of econometric tests. In truth, the physicists could care less about mathematical proofs; even the theoreticians in physics spend most of their time reading the physical equivalent of agricultural economists or economic historians. Milton Friedman's famous article of 1953 on positive economics is most of what we economists know about philosophy of science, which we think prevails in physics. The more venturesome have acquired their erroneous 3-by-5 cards from somewhat fancier sources, such as Karl Popper or Thomas Kuhn (hastily read if read at all).

The methodological thinking of economists is a scandal. It is surprising that economists, who say that they admire physicists and philosophers of science so much, do not know what is going on in these fields. The narrow philosophy of science that underlies most of the articles in the *American Journal of Agricultural Economics* and its sister journals in other fields has been exploded for decades. The history and sociology of science have shown again and again that no scientist has followed it—that Pasteur, for example, kept double laboratory books and that Darwin had his theory before he examined the facts. An economics that really imitated physics would look a lot more like agricultural economics than like the latest formalism in the *Journal of Economic Theory*.

How have we gotten so far off base? Why has economics, and even agricultural economics, failed to hear the most elementary message from statistics or physics or the philosophy of science? Well, the same way we got to be so smart at what we do: by specialization.

You will hear from deans—I hear it from some of my own—the argument that what we need is more specialization, "building on strength." We are to be shoemakers sticking to our lasts. We are to build strong walls around disciplines, failing to emulate the breadth of learning in the older generation of agricultural economists or labor economists or economic historians.

Superspecialization in academic life is not natural or productive. It is caused by an administrative decision in favor of the invisible college, the college of one's narrow subspeciality. The faculty of the invisible college have become the only voters on tenure and salary. Outside opinions in letters of recommendation count for more than the opinion of colleagues down the hall. With a changed audience, naturally, the written products and the policy thinking of the superspecialized fields have changed. The audience for most agricultural economics is not even other agricultural economists, not to speak of policy makers or (perish the thought) actual farmers. It is the handful of other specialists, for the purpose of their specialization and no other.

An anonymous respondent to a survey put his finger on what drives the character of academic journals now even in agricultural economics: the societies and their journals "have become agents to establish professional credentials for tenure, promotion or a job offer" (quoted in Just and Rausser, p. 1189). As Just and Rausser argue, "Many of our recent graduates spend most of their time wondering about the application they can make of standardized solution frameworks rather than finding interesting problems that require the development of customized frameworks" (p. 1179). That is how you get tenure when the visible college gives way to the invisible one.

The superspecialization in economics is not justified by results. If you think it is, tell me, please, what economics has learned since the War from the more spectacular superspecializations. No fair claiming the number of publications as what we have learned, regardless of whether they will matter to anyone in six months. I am looking for ideas that matter. We have learned more in economics from our continuing traditions of political arithmetic and economic philosophy. Human capital, the economics of law and society, historical economics, and the statistics of economic growth have come from economists who trade with the rest of the intellectual world.

The superspecialization in economics and elsewhere has been defended by an erroneous piece of economic argument. Specialization is an economic idea. But it is misused by academic planners (and even by some economists when they become academic planners) to justify high tariffs in academic life. The key economic point is this: specialization in itself is not good. In fact, Adam Smith himself (not to speak of Marx, you see) was eloquent on the damage that specialization does to the human spirit. What is good is specialization and then trade. As Adam Smith remarked famously, "Consumption is the sole end and purpose of all production; and the interest of the producer ought to be attended to, only so far as it may be necessary for promoting that of the consumer" (Smith, vol. 2, p. 179). There is no point in a feed grain farmer piling up corn and soybeans in the back yard unless he is going to sell them some day in order to consume the fruits of other people's specialization.

The trade in intellectual life is precisely the use of other people's work for one's own. It is what goes on in interdisciplinary activity, if the activity is something more than polite acknowledgment of the other's expertise, insulated carefully from disturbing one's own. If we actually read each other's work and let it affect our own, then we are well and truly following the economic model of free trade. If we do what most academics do—never crack a book outside their subdisciplines—then we are following the economic model of Albania, specializing in ox carts and moldy wheat. Modern academic life has whole fields specialized in ox carts and moldly wheat.

Understand, the argument is not against all specialization but against the failure at last to trade. It will be sweet work for one part of agricultural economics to talk long and hard about fitting translog production functions. A great many of the articles in the August 1989 issue, as it happens, centered on the translog. Maybe there is something important for economics in them. Like abstract general equilibrium and most econometric fittings, it is well worth a try. For the moment, for purposes of specialization, the researchers should stick with the figures from the Census of Agriculture and ignore what we know from agronomy or rural sociology or from the living of farm life. My argument does not attack systematic work. No one would wish to stop systematic specialization.

The problem comes when the narrow, temporary agreement hardens into a methodological doctrine for all time. Then the feed grain starts piling up unsold in the back yard and begins to get moldy. If the agricultural economists specializing in translog production functions make the temporary rule permanent, throwing everything that cannot be said in a translog function into a nonscientific outer darkness forever and ever on merely philosophical grounds, they are joining the voluntary imbeciles.

The failure of specializing modernism in psychology, economics, and elsewhere to fulfill its promises does not say it was a bad idea to try. And it certainly does not say that we should now abandon fact and logic, surface, and cube, and surrender to the Celtic curve and the irrational. We are all very glad to keep whatever we have learned from positive economics or the running of rats or the latest identifying move in the econometrics of agricultural production functions. It says merely that we should now turn back to the work at hand equipped with the full resources of human reasoning.

The anthropologist Roy D'Andrade, writing about psychology, put it well: "One cannot expect to improve upon Freud by observing less about human beings than he did" (p. 39). We cannot expect to improve upon Smith or Keynes, or for that matter T. W. Schultz, by observing less about economies than they did. The point is economic again: we will do better with fewer arguments ruled out, with fewer arbitrary constraints on our intellectual maximization. It entails less sneering in academic life, less ignoring of chemists by physicists or of sociologists by economists or of statisticians by mathematicians or of agricultural economists by economic theorists. Considering that other scholars read different books and lead different lives, it would be economically remarkable, a violation of economic principles, if nothing could be learned from trading with them. The notion that something can be learned from trading with others merely applies consistently the economics of intellectual life. Just as differences in tastes or endowments are grounds for trade, disagreements about the causes of crime or the nature of capitalism or the causes of excess farm populations in rich countries are grounds for serious conversation.

The way to inaugurate the intellectual trade and intellectual modesty that will I hope characterize the world after modernism is to focus on rhetoric. It is an anti-epistemological epistemology that breaks down the walls between disciplines. The common ground is argument. We have discovered at Iowa that what professors have in common is not some subject or social problem but the art of argument. We have a group of over a hundred faculty in fields ranging from hydraulic engineering to late-medieval English poetry that has met a couple of hundred times winter and summer to scrutinize a professional paper by one of the group. Iowa's "Project on Rhetoric of Inquiry" has been expanding exponentially since 1980. Not epistemology or game theory or even econometrics, as much as I love them all, creates real conversations across disciplines. A focus on the rhetoric of science does, a focus on the very words of how we argue. A professor of Spanish cannot give her colleague in mathematics any advice on the substance of his paper. But she can point out to him that the form is part of the substance and can remind him that the appeals to authority so important in mathematics can be found in seventeenth-century Spanish plays. From this would come a revitalized social science, and a rehumanized one—without giving up even one of the quantitative insights from Ames or Cornell or Maryland.

The broadminded conversation in agricultural economics is a good place to begin. Agricultural economics, I say in praise, is more scientific than many parts of economics. It has a tradition of non-*agon*istic conversation that has produced thinking more important for society than the latest ruminations from the blackboard. As Johnston says, you should keep that sturdy three-legged stool for the future, ready for serious scientific milking, and with no quarreling among the legs.

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