

Vision, Judgment, and Disagreement among Economists

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It is often said that if you laid all economists end to end, they still wouldn't reach an agreement. The implication of that and similar statements about economists is that there is too much disagreement among economists. I disagree; in my view, given the nature of the questions economists ask, significant disagreement is both to be expected and desirable.

Unfortunately, while I believe that the total amount of disagreement may be approximately the right amount, I also believe that the areas and reasons for disagreeing are often wrong. In my view, we often disagree about the things we should agree about, and agree about things we should disagree about.

For example, on the one hand, economists generally agree to use a mainstream model that, in my and in many economists' view (even those who use it), does not relate to the real world, and embodies innumerable ad hoc assumptions. Here is an example in which more disagreement would seem warranted. On the other hand economists spend enormous amounts of time disagreeing about what I consider relatively minor modeling issues--such as whether the Pigou effect logically exists, and what is the shape of the LM curve. I consider these relatively minor issues because the answer to them does not make a large difference to our policy advice.

The different models that economists use, and the different policies that economists recommend, are not dependent on minor modeling issues; they are

dependent on differences in vision of how an economy operates, and in differences in judgments about how the political system would implement a policy, or on what effect the policy will have on existing institutions. But most economists' debate concerns minor issues of modeling. It is as if economists use disagreements about minor modeling issues to mask differences in vision and judgments about dimensions of economic problems that cannot be precisely quantified.

Artiphobia

This fear of discussing differences in judgment and vision can be called artiphobia. Economists have a fear of being considered artists, who use judgment and vision to come to a conclusion, rather than being considered scientists. Artiphobia shows up in many ways. One recent example is a "slam sheet" on my introductory textbook prepared by a competing textbook author.¹ One of the major reasons why this competing author believed people should not use my book is that "Colander takes a whimsical approach to principles. In his view economics is more art than science."

Similar views of my treatment of J.N. Keynes' tripartite art/positive economics/normative economics distinction have been conveyed to me by economists who share my view that many of the differences among economists concern differences in judgment and vision, and not differences that can be captured in formal models. But they object to, or at least strongly discourage me from, using the

¹A slam sheet is a list of reasons a professor shouldn't use a competitor's book. Slam sheets are not compiled by most textbook authors; most prefer to highlight the strengths of their book. They are compiled by a few textbook authors whose egos and incomes are significantly intertwined with their own introductory books.

term art" to capture that judgment and vision aspect of economics. They believe, probably correctly, that for me to tell economists that much of what they do is art will insure that my views will not be taken seriously. Actually, I have no problem with dropping the term art; labels are unimportant. I mention the reaction my treatment provokes because it helps explain the perverse nature of economists' disagreement; our disagreements are generally in matters of judgment about relevance of models, not technical issues within those models. But such disagreements are not allowed to be expressed explicitly since they might be considered artistic disagreements. Instead, the disagreements get reflected in disagreements about models—disagreements that "obviously" fall in science.

The Need for Vision and Judgment in Selecting among Ad hoc Assumptions

To see what I mean about the importance of differences in vision in determining a theory, consider the economic problem in its full complexity: You have five billion people—all pursuing ends that are partially endogenously determined—interacting in a variety of institutional settings—using a large variety of differentiated inputs and ever-changing technologies—to arrive at some undetermined output. What will be the nature of that interaction, and what output will it lead to?

Faced with the problem of designing a core model of the aggregate economy to capture that interaction, imagine someone put the following propositions to someone:

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1. That we forget about the multitude of inputs, and use a model with only two—capital, a fixed input, and labor, a variable input.
2. That we assume away technological complexities leading to infinitely varied adjustment costs and focus only on diminishing marginal returns.
3. That we use a representative agent approach to analyzing aggregate phenomena, eliminating questions of strategic interdependence.
4. That we talk about money as a fixed concept, even though we're not sure which of the constantly changing empirical measures of money to use.
5. That we assume, for the sake of analytic convenience, that tastes are exogenous.
6. That, again for the sake of analytic convenience, we assume a simple utilitarian psychological basis for our analysis of individual decision making.
7. That when talking about the aggregate economy we aggregate up millions of different goods into an aggregate output concept, and that we can assume technical efficiency of the aggregate production function.
8. That we assume costs of rationality are zero, and people can make rational decisions intertemporally and across the full domain of goods.
9. That we can assume a unique aggregate equilibrium.

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I could continue with this list, but the list I have just given should be sufficient to make my point. Given the complexity of the problem to be solved, disagreement in the sense that diversity of research programs should be expected.² That isn't what one sees. While there are small pockets of non-mainstream critics--such Institutionalists, Post Keynesians, Austrians, and Radicals--the large majority of the profession makes up an almost hegemonic research program in which most researchers accept the broad outlines of assumptions, and then modify one or another slightly or ask questions that can be structured within the standard model.

I believe the reason we don't see major disagreements is that because such disagreements would involve differences in vision and judgment. The guiding light in choosing among assumptions cannot be pure logic—the decisions are too complicated for formal logic. Instead one must rely on vision to choose that combination of simplification and admission of complexity—the ad hoc assumptions—that produce a model that leads to relevant insights, and that is convincing to others. Yet there is almost no discussion of disagreement about economists' vision.

The evolution of the Keynesian revolution is a case in point. There were many ways in which the Keynesian challenge to neoclassical economics could have been structured; it could have been seen as a vision fundamentally different from the Classical vision—one involving multiple aggregate equilibria, strategic interdependence, and dynamic path dependency. The way it was structured was within a unique aggregate equilibrium framework in which the only difference

²The competition among these competing paradigms need not be based on disagreement, as much as on following different leads. By that I mean that a group considers the choices made by the other group legitimate, and complementary to their own approach.

between Keynesian and Classical was a fixed nominal wage assumption. This meant that Keynesian economics was simply a special case of the Classical model. All the disagreements between Keynesians and Classicalists were forced to be placed in this fixed wage framework, and thereby many of the issues raised by the Keynesian revolution were trivialized. Why? Because structuring the question this way fit a relatively simple model, whereas other frameworks raised far more fundamental questions.

The Need for Judgment in Choosing Among Policies

The policy debates, similarly, involve disagreements on issues that are not central to the reasons the profession disagrees. In policy, the true debate generally relates to judgments about non-quantifiable effects. Yet, economists' debate about policy is generally formulated in terms of applied models and empirical specification. For example, we see most economists' discussion of economic policy focuses on efficiency and on quantifying "efficiency." But efficiency is not an end in itself; it is not even a meaningful concept except as a description of achieving some other goal. The implicit assumption used in economics is that more consumption—higher standards of living—makes people happier, with little discussion of how that "more consumption" leads people to be happier, or what problems that might involve, or whether the process of fulfilling these desires creates further desires.

Let me give a specific example. One of the most powerful economic policy arguments I have seen lately is made by Charles Murray. Murray, who is not an economist, used standard economic arguments to argue that public welfare is bad for people because it destroys the social fabric of society. He reasoned that, given

current U.S. institutions, because people do not have to face the consequences of their actions--having to feed their children, for example--the number of unwed mothers has increased significantly, and will continue to increase. Eventually, he argued, the institution of the family will be destroyed. Many economists agree in part with Murray's argument, but discussions about the effect of public welfare on the structure of the family have not filled the economists' journals because the argument cannot be quantified and placed in a formal model. How does one specify the "efficiency" of the family? More generally, institutions cannot be easily modeled so they are typically left out of economists' models.

Resolving questions about assumptions and in differences in judgment has little to do with formal empirical tests as economists usually focus on them. As Thomas Mayer (1993), and Edward Leamer (1978, 1991) have convincingly argued, given the nature of observation in economics, empirical tests at this level are inevitably indecisive. Numerous theories and judgments can be interpreted as consistent with the data.

One example of the way in which statistics are used can be seen in a recent study of the effect of the social security system on savings rates. Martin Feldstein (198) published a study in a major economic journal providing empirical support for his position that the existence of an unfunded social security system in the U.S. was significantly decreasing the savings rate. Two researchers (Leimer and tried to replicate his study, but could not. It was found that a data input error had led to the result, and that when that error was corrected, Feldstein's model showed that the unfunded social security system had a positive, not a negative effect, on the savings rate! Did this result cause Feldstein to change his policy proposals? No, instead he

redid his empirical study, adjusting the model, and came out with a slight negative effect of social security on the savings rate.

The point of this example is not that a data input mistake was made, although as Dewalt et al. (1986) have shown, such mistakes happen relatively often) the point is the way in which "empirical evidence" is used. To structure a model that will capture the effect of social security on the savings rate, enormous ad hoc assumptions must be made to develop the data. These ad hoc assumptions make it impossible for the empirical analysis to be definitive. Feldstein knew that, and he, correctly, did not change his policy position when the data input error was found. His policy position was based on a much broader combination of vision and judgment. But in the study he did not make his argument based on that vision and judgment; instead a position based on judgment was translated into formal empirical evidence that looked scientific and definitive, but was not and could not be.

Admitting that most policy conclusions are arrived at by a combination of judgment, vision, formal and informal empirical evidence, and a knowledge of history and institutions, has a cost; it raises what might be called the "expert question." Why should policy makers, or anyone, rely on economists' judgment rather than someone else's? I believe the answer to this question should be the following: One should rely on economists' judgment because they have studied the history, the institutions and have training in interpreting the empirical evidence. This training in judgment makes them better qualified than most other people to offer advice on policy, and when one understands the economists' reasoning, it will be more convincing than other people's reasoning. The answer to which the current structure of the profession directs economists is that the mantle of science

legitimizes economists' policy pronouncements. Instead of admitting that policy decisions must be made on informed judgment, and that judgment is best left to experts, the economics profession has chosen to look impressive, and to try to "snow" policy makers.

Institutions, Economists, and Disagreement

Economists are not born with artiphobia; it is bred into them through selection mechanisms limiting who becomes an economist and who advances as an economist, and through constant institutional reinforcement. Thus, the artiphobia explanation of the perverse nature of economists' disagreement is only a surface explanation. To explore beneath that surface and explain why this perversity of agreement exists, one must explore the incentives in the academic institutional environment within which economists operate.

I believe that that institutional structure channels their self-interested behavior into what might be called micro-disagreements—disagreements that don't really matter—and away from macro-disagreement—disagreement about core issues, which would significantly change their analysis. The reason is that the micro disagreements avoid the appearances of art, whereas macro-disagreements would be considered directly as differences in judgment and thus would fall into what I have classified as art.

An Economic Approach to Agreement

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To see my argument about how and why institutions reinforce economists' antipathy, it is useful to consider the question of agreement and disagreement among economists within an economic framework. In this framework there are costs and benefits to agreeing and to disagreeing. Individuals weigh those costs and benefits and choose their optimal private level of disagreement. When it is in a person's interest to agree, he or she agrees, even if what he or she is agreeing about is quite intuitively disagreeable and far-fetched. And, when it is in a person's interest to disagree, he or she disagrees, even if the disagreement is about something relatively small. Thus the institutional structure that determines the costs and benefits of agreeing is central to an understanding of the nature of disagreement in economics.

The recent round of GATT talks provides an example of this economic cost-benefit analysis of agreement and of the importance of institutional structure. These GATT talks dragged on interminably, with the same issues being discussed over and over again. They went on two years longer than planned. Why the continued disagreement? Clearly part of the reason has to do with the contentiousness of the issues involved. But the economic approach to agreement directs one to consider the incentives created by the institutional structure: What were the costs and benefits of agreeing and disagreeing?

To answer that question, consider where the talks were held: in the pleasant surroundings of Geneva over expensive meals. Given these surroundings, the benefits of disagreeing to the participants were high, and the costs were low; the economic approach suggests that the disagreement would be continued as long as possible. Had those talks taken place in Buffalo in winter (and all the excellent

restaurants in Buffalo had been closed down), the GATT talks would have come to a quick resolution.

GATT, of course, is as much about political, as it is about economic, agreement. But the issues are the same for any economic issue, be it one of theory or of policy. People agree and disagree when it is in their interest to do so, and the institutional structure within which they operate determines their interest.³

The statement that agreement depends on the costs and benefits of agreeing is, as is much economic analysis, logically correct, but empirically vacuous, in the sense that it simply pushes the analysis back to what determines the costs and benefits. Thus in no way am I arguing that the economic approach is sufficient. The economic approach simply provides a framework of analysis that is useful in the same way that Arabic numerals are useful: One can build an elegant, logically correct system of analysis around them which sheds more light than would an analysis built around some other number system, such as the Roman system. Similarly with economic analysis.

Incentives for Agreement and Disagreement

What the economic approach does is to direct us to look at how institutions structure the incentives for agreement and disagreement of economists. How will

³The economic model of disagreement and the above discussion of the private costs and benefits of disagreeing cannot be applied to a "representative agent" model of economists. The reason is that there is a distribution of optimal disagreement in a field. The overall amount of disagreement depends on the character, independence of thought, personality, training, and ability of economists. An individual's optimal amount of disagreement depends on the total amount of agreement in the field. When most economists agree, there are enormous gains to be had in disagreeing. Thus, the odd contrarian can do very well for him or herself.

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agreeing or disagreeing on an issue help an economist achieve his or her goals within the institutional framework within which he or she operates?

The institutional framework within which academic economists operate is one of tenure and quantitative publication requirements based upon rankings of journals. Advancement in the profession depends on publication. It follows that if a disagreement can be stated and resolved in print, it is much more valuable for the participants than if that same disagreement is settled elsewhere. For example, there is little incentive for one economist to call up another with whom he or she disagrees, and try to work out the disagreements over the phone. Thus, there is much disagreement based on misinterpretation. This interpretative disagreement could be relatively easily resolved by letter, or by phone, but instead it fills the pages of journals with models which are not getting at the true nature of the disagreement.

One of the most insightful pieces in economics that I have seen was Arjo Klamer's *Conversations with Economists (1984)*. In that book Klamer published his interviews with various top macro economists, asking questions about debates New Classicals were having with Keynesians. What was amazing to me about these conversations was how little effort was made by these economists to actually specify the precise nature of their agreement and disagreement with their "opponents." Often one side's view of the arguments was quite different than another side's. Each side had a caricature of the other side, and it debated that caricature, not the other side. The only serious interaction among these competing sides occurred in formal journal articles, and that required maintaining a caricature of the other sides. No serious attempt was made to resolve the differences efficiently. The economic

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approach to agreement explains this phenomenon; discussing the issues privately would have reduced private benefits to disagreement.

A second way in which the academic institutional structure influences the nature of the disagreement among economists is that it focuses that disagreement on technical and formal empirical issues, and away from issues of judgment. Technical and empirical disagreements look more impressive, and can generate more "resolvable disputes." The problem with disputes in judgment and vision are that they are unresolvable in articles. Thus, self-interest channels disagreement away from disagreement of judgment and vision, and toward "resolvable disputes" about technical matters.

The publication institutions are not structured for major disagreements about assumptions—they are much more structured for definable disagreements, given the major assumptions. Such technical and empirical disputes have another advantage; since they do not resolve the issue, they leave open the possibility of re-examining the issue again and again, creating more grist for articles leading to tenure and promotion. One of the most telling remarks was made to me by an extraordinarily bright, but relatively unknown, Oxford economist who combined judgment with technical expertise. When I asked him why he wasn't better known he said that it was because when he tackled a subject, he answered it to the degree it could be answered; thus, his work stopped research in a topic.

Let me give an example of the profession's proclivity to avoid fundamental disputes, an example that involves the use of macro econometric models. In 1972 Robert Basmann (1972) strongly condemned the structure and use of macro econometrics in general, and the Brookings model in particular, arguing that it was

pretending to be something that it wasn't and that it lacked a scientific basis. This was a challenge to the major assumptions. His critique was essentially ignored. Reflecting on Basmann's criticism, Larry Klein et al. (1992) again dismissed Basmann's critique, reiterating Fromm and Klein's (1972) statement that if one "took Basmann's critique seriously, inductive science must perish" (p. 79). This is one of many examples. A disagreement that cuts to the core is ignored, not because it may or may not be correct, but because the institutional structure strongly discourages such major disagreements.⁴

Why Artiphobia Developed

The institutions that determine the incentives for agreement and disagreement are not ad hoc. They have developed for reasons and, if they work effectively, they should be designed to create incentives so that disagreements are optimally resolved. But there is no reason why one should expect optimal institutions to develop, and there is a strong reason why one should expect that suboptimal institutions will develop. The reasoning that supports me here is similar to the reasoning that public choice theorists use in explaining why government spending programs become too large.

The costs and benefits of developing institutions tend to reflect the needs of individuals within the institutions, rather than the needs of society. The reason is that the benefits of creating institutions, while overall large, are broadly distributed

⁴Part of the reason for this dismissing of fundamental critiques is the legitimate need to get on with what one is doing. In 1972, perhaps temporarily dismissing Basmann's critique made sense—in the hope that future work would show the attributes of the chosen path. But in 1992, to continue to ignore such criticisms, in spite of the failure of many econometric models, suggests a problem in the profession.

and small on a societal per-person basis, but are relatively large on a per-person basis for the individuals who work within that institution. Thus, as a general rule institutions develop that have, from society's point of view, incentives that direct people's private interest away from society's interest. This might be called The Theorem of Perverse Institutions.

Academic economic institutions are more perverse than most institutions because they were designed around a technology that did not pan out. In the 1950s it was believed that econometrics was going to provide a technological change that would transform the nature of disagreement among economists by providing definitive tests of theories. Much of the modern structure of economic argumentation and methodology that I find perverse was designed around that belief. That methodology directed researchers toward highly formal analysis—structured in a way that would be susceptible to empirical tests. Incoming economists were taught what might be called "classical econometric intuition." They were taught to replace their own intuition with a refined intuition based on econometric empirical tests. They were taught to limit arguments to those susceptible, at least in principle, to formal econometric testing, and to structure arguments in a formal way so that eventually they can be resolved by empirical testing.

Unfortunately, econometric testing has proven far less definitive than was initially hoped. Dealing with this failure has been difficult for the economics profession. The institutional incentives in the profession are to base your argumentation and analysis on this econometric testing, and not on debatable judgment and sensibility. Yet, most of the issues at debate concern judgment and sensibility, and are not susceptible to formal empirical tests. The problems this

creates are predictable. When industry has designed its structure around a technology, and that technology doesn't pan out, there will inevitably be serious problems.

Institutional Screening Effects

Another way in which the institutional structure has played a role in structuring the nature of disagreement in the profession is in its effect on the personality of the individuals within the profession. In this world there are contentious people who will make big challenges, and there are uncontentious people, who will focus their energies more narrowly. Similarly, there are people who excel in broad vision, and judgment, and those who excel in more technical areas. The screening process that selects which people go into a field, and which do not, plays a big role in determining the nature of agreement and disagreement in a profession.

Currently, the economic profession's institutional screening process channels a particular type of individual into economics, and that, I believe is another reason why disagreements in economics take the form that they do.

Consider the following four candidates applying for a top graduate economics program:

Candidate A1: an economics major--a bright generalist. GPA - 3.9; SATs: math-740; English - 760; two courses in calculus and one in statistics;

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wide range of extracurricular activities. Relatively uncontentious; he generally goes along.

Candidate A2: an economics major--a bright generalist. GPA - 3.9; SATs: math-740; English - 760; two courses in calculus and one in statistics; wide range of extracurricular activities. Relatively contentious; she challenges everything.

Candidate B1: a physics major. GPA - 3.9; SATs: math - 790; English - 620; minors in both mathematics and economics; (seven courses in math); few extracurricular activities. Relatively uncontentious; she generally goes along.

Candidate B2: a physics major. GPA- 3.9; SATs: math - 790; English - 620; minors in both mathematics and economics; seven courses in math; few extracurricular activities. Relatively contentious; he challenges everything.

All four of these candidates are what I would consider excellent candidates. A strong profession would be made up of a combination of the four types; their interaction and disagreement would strengthen the profession. Unfortunately, the current institutional structure does not bring such disagreement among these four types about for two reasons. The first reason has to do with the probability of acceptance.

My reading of the current selection process used in top graduate schools is that Candidates B1 and B2 have a higher chance of getting into a top graduate

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program with financial support. That, of course, depends on who is on the selection committee, and, generally, I believe, that the "A" candidates can get into a top school if they have strong undergraduate faculty support. Specifically, I would suspect that with some phone calls, some strong letters of recommendations, and some luck in who is on the selection committee, both the A candidates could get in, and possibly could get financial support. Still, there is a bias toward B type candidates; they are the ones most graduate schools would prefer because they are the ones most likely to excel initially.

Let's now consider the choice from the candidates' side.

Candidates A1 and A2 most likely have a wide range of choices in business, in law, or in business school. Their choices may narrow down to something like the following: (1) a \$45,000 job on Wall Street with significant opportunities to be challenged; (2) law school without support, but with high earning expectations in the future; and (3) a Ph.D. economics program in which they may have barely gotten financial support, and in which it is almost assured that they will have an extraordinarily difficult time in their first two years. Given these opportunity costs, very few of type A candidates will choose economics, and of those that choose it, type A1 candidates are more likely to choose it and stay with it through the first two years, than are type A2 candidates.

The choices facing B type candidates will likely be quite different. They may be choosing between a graduate physics program and a graduate economics program. Of these two they will probably see the graduate physics program as the intellectually more challenging, but may be enticed by a higher level of financial support to go into economics. Type B 1 candidates are more likely to choose

economics than are B2 types; in physics one is asking questions about the nature of matter, and the sky is no limit. The subject matter of economics is tame relative to physics.

A few type A2 and B2 candidates will get their Ph.D.s. They, however, will likely be weeded out of academic careers at the 2-year, 4-year, or 6-year review stage of the tenure process, which gives highest weight to work that exhibits technical mastery, and not to policy issues related to judgment. Any economist who really tries to challenge the underlying foundations of the assumptions will almost assuredly be weeded out. Major contributions take gestation time and the tenure and promotion system in academic economic institutions do not allow such long gestation periods.

The result of this selectivity bias is that the interaction among the various types of students never materializes. Type A1 candidates predominate in the profession, and the approaches that are used are not challenged by the contentious generalists or contentious mathematically inclined students. This selection system causes the institutional structure to be self-reinforcing, and brings about the current state of affairs in the profession. I am known as a critic of graduate school; the reason I am is that its screening weeds out individuals that I believe the profession needs.

Conclusion

The economics profession is not in a crisis. It is simply in a slow decline, as is suggested by the declining number of U.S. citizens receiving Ph.D.s in economics

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over the last twenty years. Eventually, the problems in the profession will cause the current institutional structure to break down, or to change, to better accommodate disagreement in judgment. But any change is unlikely to occur anytime soon.

Nonetheless, the current institutional structure of the profession has short-run costs. To be sincere in one's disagreements, as I believe economists are, and simultaneously to hide the true nature of the disagreement requires a certain detachment from the analysis. Hiding the true nature of the disagreement makes it impossible to arrive at intuitively satisfying resolutions to debates. Moreover, it makes the resulting research less valuable than it could be.

Another effect of the institutional structure in the profession strongly discourages disagreement based on judgment and sensibility, where much of the disagreement about economic theory and policy resides, is that it encourages economists to surround themselves with like-minded economists, rather than encouraging interaction and debate with economists who have differing sensibilities and judgments. This leads to geographical pockets of agreement.

For example, in a survey asking students their views on the statement: "Can fiscal policy be an effective tool in stabilizing policy?" only 6 percent of Chicago students agreed with the statement; 60 percent of Yale students strongly agreed with it. Or alternatively, 70 percent of the Chicago students strongly agreed with the proposition that a minimum wage increases unemployment among young and unskilled workers; only 15 percent of Harvard students strongly agreed to that. On a third issue, 84 percent of Chicago students strongly agreed with the proposition that inflation is primarily a monetary phenomenon; 7 percent of MIT students strongly agreed. This clustering of agreements strongly suggests that the

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interchange of ideas is not taking place, and that empirical work is not eliminating the disagreement.

It is my belief that a more open treatment of the reasons for disagreement would encourage discussions to proceed beyond formal empirical testing, and focus more on informal evidence. This more open treatment of reasons for disagreement would accept that evidence will often be limited or inconclusive, and that much disagreement is likely. But it would lead to more precise statements of where and why economists disagree, and to more intuitively satisfying states, if not resolutions, to debates.

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