

The Cost of Being Queen: Some Reflections on Economist's Views of Coordination

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Although all the social sciences deal with aspects of coordination, one of them, economics, is generally considered the most successful (especially by economists). Its success has earned it the moniker “queen of the social sciences,” although precisely what its kingdom is, and what being queen means, is generally left unstated. I interpret the phrase as meaning that, compared to the other social science disciplines, economics has been the most successful within the institutional setting—its departments are bigger and its influence on public policy larger, than the other social sciences. Edward Lazier (1999) summarizes the view of many when he writes, “By almost any market test, economics is the premier social science.”

The two questions, which I see as related, that I address in this talk are

- (1) Why is economics queen? and
- (2) What does economics have to say about coordination?

Why is Economics Queen of the Social Sciences?

My answer to the first question can probably best be conveyed in reference to all-powerful king in St. Exupery's *Little Prince*. Like that king, Queen Economics has absolute power; she tolerates no disobedience. But, also like the King, she achieves her absolute power by limiting her methodological commandments to those telling economists to do what they want to do, and the only constraint on economists is that their work maintains its regalness (interpret: perceived rigor). As Jacob Viner quipped, economics is what economists do; when considered over time, Viner's is probably as good a definition of economics as one will find. This definitional openness allows a dynamic in economics' methodology and scope that lets the field of economics incorporate the latest analytical and technical developments that might have some relevance.

The definitional openness to ideas came through in my recent interviews with graduate students at top US schools (Colander, forthcoming), when I asked the students what their professors wanted from them most. Their response: “Tell me something I don't

already know.”¹ Let me just give one example of the changes in scope that this definitional openness allows. In the 1980s when Arjo Klammer and I (Colander and Klammer, 1987, 1991) interviewed students at top US graduate programs, sociological questions were not part of what economists studied; they were outside the scope of economics. In my latest set of interviews with students at top schools that was not the case; sociological questions were definitely considered within the scope of economics. In fact, students told me that the primary difference between an economist and a sociologist was the stronger set of analytical tools that an economist has, and the topics that they would attack. One student summarized the difference as follows: sociologists do research on important questions for which they don't have the tools to answer; economists do research on unimportant questions for which they do have the tools to answer.

Essentially, my argument is that economics remains queen of the social sciences through its commitment to perceived rigor, and not through any set of specific beliefs, tools, or approaches. In terms of ideas, it is “anything goes.” For this reason the definition of what economics is, and what its scope is, change over time. At any one time, there is a mainstream that is much broader than what will later be classified as the orthodoxy. Generally, the existing economics mainstream will include individuals who hold diverse and even conflicting ideas, and one can only understand the mainstream of a time period by considering the profession in its entirety. What becomes known as the orthodoxy is what wins out in the institutional competition. Thus, for example, the ideas explored during the Classical economics period were much broader than those now considered Classical.² Similarly, the ideas held by economists during the periods now called Neoclassical and Keynesian were much broader than the ideas we now think of as Neoclassical and Keynesian. It is important to recognize this openness of economic thinking in order to understand economics and its evolution. Instead of thinking of economics as unchanging set of ideas and methods, it is best to think of economics as an evolving complex system of multiple streams of ideas each competing within the institutional structure.

Methodological openness to ideas does not extend to all methods. Economist's commitment to perceived rigor gives economics its regality, and that commitment limits literary methods in formal analysis. It is this sense of regality that critics of economics often react to; they criticize what mainstream economics does and how it does it. But, in my view, these critics generally error in focusing their criticism on ideas currently being taught or currently being worked on, rather than on the broader set of ideas that economists have explored over the years and are still part of the broader discipline of economics. The reason I say “error” is because this approach allows the mainstream to ignore its critics.

Criticisms about ideas in the texts are generally dismissed by the mainstream as irrelevant; what is taught in the texts is a pedagogical decision, which can differ significantly from what good economists believe. Similarly, criticisms about the

¹ Telling economic professors something they don't already know is, of course, quite hard since economists, especially those at top schools, tend to believe that they know quite a bit.

² Dennis O'Brien (2004) in his masterful overview of Classical economics nicely makes this point about Classical economics

limitation of ideas currently being worked on generally bounce off mainstream economics, because mainstream economics doesn't define itself solely by ideas currently being taught or worked on. It defines itself by a broader set of ideas that includes much of what has been previously explored. Even when most economic researchers may be working on one small branch of economic thinking, good researchers remain open to a much wider set of ideas and approaches. This means that the economics field will incorporate any idea that meets the "in a rigorous manner tell me something I don't already know" criteria. Most criticisms of economics don't meet that criteria in the eyes of mainstream economists; they are cast off as being unrigorous and/or obvious-- something that any good economist knows. Of course, what is perceived as rigorous changes over time, both because of changes in perception, and because of changes in analytic and computing technology. This means that ideas move in and out of active consideration in the mainstream as changes in analytic and computational technology are incorporated into the mainstream.

Let me explain how I see the process working by briefly discussing the rise and fall of Keynesian economics. When I learned macro, Keynesian economics was presented as a fundamental scientific revolution that was part of the core of economics. Today, Keynesian economics is simply seen by many economists as an episode of economic thinking that is best forgotten. I see it as neither of these. Instead, I see "Keynesian economics" as the name that was given to a multifaceted set of developments in theory, pedagogy, policy, and empirical work, each of which was part of an ongoing evolution in economics that was occurring independent of Keynesian economics.³ Each of these developments would have influenced economic thinking whether or not these developments had been placed under the Keynesian name.⁴ These developments reflected the changing technology and changing institutions of the time, and often worked at cross-purposes.⁵

The Keynesian revolution, like other revolutions in economics, was very much a revolution that followed the path of least resistance. It combined various developments that were occurring in the profession in a way that would insure its short-run existence. It was continually metamorphing into something other than it had been. If there were an opening for monetary and fiscal policy, those policies would become Keynesian. If there were developments in econometrics, the Keynesian revolution embraced econometrics. If there were an opening for a simple pedagogical model, the Keynesian revolution would embrace a simple pedagogical model. If a workable synthesis between Keynesian and Classical economics required basing the synthesis on fixed wages as the fundamental problem with a macro economy, then fixed wages would be the foundation of the Keynesian position, rather than the nonergodic uncertainty that some people believed was its foundation. Were it not for that metamorphing tendency, the Keynesian revolution

³ I develop these arguments in Colander (2006).

⁴ Why all these disparate developments came under the Keynesian moniker is a difficult question in social thought, and one that I will not deal with here. I point it out here because in order to understand the history of macro one must recognize these disparate elements of the Keynesian revolution and that many of these developments worked at cross-purposes.

⁵ It is precisely because the Keynesian revolution involves so many different elements, that there was always, and still is, enormous ambiguity about what precisely Keynesian economics was.

would have been unlikely to be the success it in fact was. And were it not for economics' metamorphing tendency, and its ability to continually incorporate new analytic and computational technologies, economics would not remain queen of the social sciences.

What Does Economics Have to Say about Coordination?

The above discussion is relevant to this conference because it underlies my answer to the question: What does economics have to say about coordination? My answer is that economics has many different things to say about coordination. Some of these have been formalized; others have not, and while both are included in what economists have to say about economics, only that which can be said in a perceived sufficiently rigorous manner is discussed in depth. I emphasize this diversity because economics is currently incorporating new analytic technologies that allow it to formally explore issues of coordination that have not been discussed in economic for decades, but which are part of its broader Classical tradition.

Behavioral economics, experimental economics, complexity economics, new institutional economics are all becoming part of mainstream economics, and as they do, both the scope and method of economics is changing. The rigid rationality assumptions that previously were characteristic of economists' work in what was called neoclassical economics, and which remains in many textbook presentations of economics, are fading as limitations on research. Economics is now moving away from a neoclassical economics, which limited analyses to models based on assumptions of rationality, equilibrium, and selfishness, toward what I call complexity, or Post Walrasian, economics, which is based on the broader assumptions of purposefulness, sustainability, and enlightened self-interest.

This change is important for the conference because the new and the old economics have quite different things to say about coordination. Specifically, the old economics said that coordination is essentially a transfer of the lessons learned from constrained optimization theory to economic policy. Deirdre McCloskey calls it the MaxU approach. Most of its lessons for coordination are incorporated in the optimality conditions taught in principles and intermediate micro theory. Good coordination requires getting prices right, and the work leads to a set of rules for "right prices." Achieving those "right prices" leads to efficiency, and the focus of the analysis is on optimality.

Economists differ in their view of how far those constrained optimization lessons can be extended. Economists working in the Marshallian tradition see the extension as limited; those working in a Walrasian tradition see the extension as much broader. Perhaps the broadest extension of the ideas about coordination currently is occurring in macro, where recent work has analyzed the macro coordination question within a dynamic stochastic general equilibrium (DSGE) model. It has then used the results of that analysis to guide their views of macro policy.

Economists within a Marshallian tradition are far less satisfied with the DSGE model. There were happier with the loose set of models that had developed within macro in the 1950s and 1960s, but the found even those models as giving too much pretense of

understanding to economist's analysis. However, their dissatisfaction was not an active part of the economic discussion in the 1970s or 1980s, because their alternative models could not be formalized. Economists who persisted in pushing such views, such as Austrian, Institutional, or Post Keynesian economists, were seen as dissidents, and outside the mainstream, not so much because their views on coordination were unacceptable, but because their analysis of the issues did not meet mainstream economics' "perceived rigor" criteria. Today, that is changing; because of technological changes, work in complexity economics, which I call Post Walrasian work, is developing an alternative to the Walrasian DSGE model. That research is on the cutting edge of macro theory.⁶

Complexity, Post Walrasian, (or whatever you want to call it) economics sees the problem of agent coordination as much more complicated than does Walrasian economics. In the formal Walrasian work, one does not discuss how the Walrasian auctioneer (whom critics sometimes facetiously call "the little green man") determines the equilibrium set of prices. Walrasian general equilibrium theory shows that once those prices have been determined, the optimal coordination is achieved. In the informal Walrasian work, such as found in Phelps's or Friedman's conception of the natural rate of unemployment, frictions, and hence disequilibrium are accepted, but they generally assume that the dynamic equilibrium arrived at approximates the Walrasian equilibrium in a state of competition, and thus achieves some type of optimality.

In the Post Walrasian view such efficient coordination is far beyond what one can hope for society; instead, one is amazed that the economy operates at all, and is not in continual chaos. It sees competition as a process, and the economy being coordinated by bottom-up local coordination activities by agents. That coordination requires the development of conventions, which are institutionalized into markets, customs and other institutions. These all become part of the environment within which agents optimize. There are many layers of these conventions, which are continually changing and interacting with political and social variables. These institutions then are coordinated with higher-level customs and institutions, and so on. It is an organic type of coordination that involves interdependent multiple levels of coordination. This latticework on interdependent layers makes sorting out the process extremely difficult, if not impossible, and makes it only possible to understand the economy within an institutional and historical perspective, not in the abstract.

While "as of yet" the new economics has nothing formal to say about how this coordination works out in practice, it can, however, make some vague observations what doesn't work. First, it gives far less credence to finding the right prices; insights based on optimization models may be an input into a consideration of coordination within the new economics, but it is not the final word.. It sees little hope of finding "right prices," but some possibility of specify some likely "wrong" prices. Instead of directing the search to find "right prices" the new economics focuses more generating functions--on getting institutions right, or carried out to a higher level, getting the institutions that design institutions right, or even "institutions that design institutions that design institutions"

⁶ Colander (2006) provides an overview of that work.

right. While it cannot specify what those “right institutions” are, the new view does suggest some general characteristics of those institutions—institutions should allow individual’s significant freedom of action, and should include checks and balances to see that any institutions does not grow too powerful. Instead of trying to achieve efficiency, it concentrates on trying to establish policies that will let the economy muddle through as effectively as it possibly can.

Although I am calling Post Walrasian economics “the new economics,” viewed within a longer-term perspective, it is not new at all; instead, the new approach is a redirection economics from Neoclassical to Classical themes. Thus, what I am calling the new economic approach to coordination has its antecedents in the Classical policy of *laissez-faire*, *laissez-passé*. That policy, correctly understood, is a much more sophisticated policy than the “leave it to the market, anti-government intervention” that textbook economics sometimes presents it as being. It is a policy view consistent with a wide range of views in the debate on government intervention, as long as (1) any argument for government intervention takes into account the major problems attempting to direct independent purposeful individuals, and (2) any argument for no government intervention recognizes that markets only operate within complex institutional structures, and cannot flow directly from economic theory. At best, economic theory can provide some guidance an input into a much more complicated policy analysis.

Keynes, Hayek, Smith, Mill, and Thornton all held positions consistent with a broadly interpreted *laissez faire* philosophy of coordination. That philosophy was not based upon solely economics. The Classical analysis of coordination involved an integration of economics with sociological, political, and psychological issues. Similarly, the reasoning for it did not rely on strict assumptions of rationality, selfishness, or equilibrium, and was willing to consider any issue that made sense, and indeed incorporated such analysis in its argumentation. For example, as Albert Hirschman has argued in *The Passion and the Interests*, the Classical arguments for markets included the argument that they directed people away from activities that could undermine the stability of society.

Another example can be seen in the work of Adam Smith, who built his analysis in the *Wealth of Nations* on his *Theory of Moral Sentiments*, which had a much more complicated analysis of human nature than is captured by any theory built on rationality, equilibrium and selfishness. Similarly, J. S. Mill’s analysis of coordination went far beyond those assumptions, and involved a rich understanding of the integration of social, political, and philosophical reasoning with economic reasoning. Consider his discussion of people’s desires. He writes: “Men do not desire merely to be rich, but to be richer than other men, or than certain other men. The avaricious or covetous man would feel little or no satisfaction in the possession of any amount of wealth if he were the poorest amongst all his neighbours or fellow-countrymen.” (pg 69 “On Social Freedom”) Coordinating individuals with such interdependent utility functions is much more difficult than coordinating a world of selfish, rational, individuals that the constrained optimizing models focus on.

What Happened to the Classical Analysis?

If Classical economics' understanding of coordination was so rich, why did it give way to the neoclassical approach? My argument is that it gave way because it could not meet economics' perceived rigor criteria. The reason it could not is that in the late 1800s and early 1900s, the analytic technology available to economists increased, making a more rigorous analysis possible. At the same time, the economics profession's institutions changed as social science became more specialized, and academic institutions began creating departments of economics. More specialization gave more weight to formally rigorous analysis, and less weight the type of broad integrative reasoning that Smith and Mill employed. That change increased the degree of perceived rigor that was required of economist's work.

At the time, economists simply didn't have the analytical tools for formalize Smith's and Mill's insights, so by the late 1800s and early 1900s economists working in on what I'm calling the Classical analysis, such as Cunningham or Foxwell in England, did not meet economics' perceived rigor criteria. They found themselves being outcompeted by economists such as Francis Edgeworth and Alfred Marshall. Within the competing melting pot of ideas being explored within Classical economics, a set of ideas that became known as neoclassical ideas offered the framework that could best use the expanding calculus analytic technology to formally explore a subset of coordination issues. Because it did, the subset of ideas susceptible to that analysis moved to the fore, and with it came a much narrower approach to coordination, which considered the coordination problem as a constrained optimization problem. To create tractable formal solvable models required assumptions of rationality, equilibrium, and selfishness, and so those assumptions were made. The result was what we know as Neoclassical economics.

This meant that by the 1930s economics has two quite different stories about coordination, both of which were part of mainstream economics. One, the Classical story of coordination, was a highly complex one with no analytic solution. The other, the Neoclassical story of coordination, was complicated to those who did not know math, but not especially complicate to the best mathematicians of the time. It told them nothing that they did not already know. The existence of these two different stories is reflected in the story that Robert Heilbroner tells in *The Worldly Philosophers* of a dinner John Maynard Keynes had with Max Planck, the physicist who was responsible for the development of quantum mechanics. Planck turned to Keynes and told him that he had once considered going into economics himself, but he decided against it--it was too hard. Keynes repeated this story with relish to a friend back at Cambridge. "Why, that's odd," said the friend. "Bertrand Russell was telling me just the other day that he'd also thought about going into economics. But he decided it was too easy."

Up until the 1950s, both these stories formed an important part of well-read economist's understanding of coordination, even though the constrained optimization story was the only active area of research in microeconomics. The reason the constrained optimization model was the active area in micro is that while back in the 1920s and 1930s the outlines of that coordination story were clear to top mathematicians, to most mortals it was not, and much remained to be done to formally complete the work. Economists such as Frank Ramsey and Irving Fisher, John Hicks, and Paul Samuelson, and later

Tjalling Koopmans, Kenneth Arrow, and Gerard Debreu did that work, and by the 1970s, economists had essentially worked out the Walrasian Neoclassical story of coordination.

Even as that technical work was continuing, the Classical view of coordination remained, and the standard economics methodology was presented as dealing with two different branches of economics—the art of economics or political economy, and the science of economics, or pure economics. The art of economics was a type of applied policy/engineering branch of economics; positive economics was the scientific branch of economics. In his famous book on the scope and method of economics, J.N. Keynes (1891) carefully distinguished these two branches and stated that the two branches of economics had different methodologies and approaches. Positive economics, or the science of economics, required rigor, and produced theorems—which logically followed from first principles that were carefully spelled out. Achieving that rigor required leaving out many of the complications, but that was seen as the cost of only working on rigorous models. The art of economics required less formal rigor and thus could include many of the issues that were obviously important for consideration of policy. Keynes said that failure to separate out these two branches would cause much confusion.

To differentiate the science from the art, Keynes argued that economists produced two types of conclusions: precepts and theorems. Theorems followed from pure theory or positive economics; they were derived from rigorous models, and were scientific statements. They would be of the form: Given the assumptions, the following conclusions hold.⁷ Precepts followed from the broader reasoning within the art of economics. It included an understanding of history and of social and political issues as they interrelated with economics. Classical economists' support of *laissez faire* was a precept, not a theorem.

Over time, as older economists trained in the older Classical literature died out and younger economists, whose training was more technical, replaced them, awareness of the Classical view of faded. Whereas the broader Classical coordination story was second nature to older top economists, even as they worked on much narrower technical problems, it was not second nature for younger economists. Training in the literary/historical tradition of economics, within which the complexity view of coordination resided, slowly faded away. That fading away was further augmented by the expansion of statistical and econometric work, which opened up a new type of applied economics. By that I mean that whereas before the 1960s, “applied economics” was taught in reference to the historical/literary traditions, since the 1960s increasingly “applied economics” has become associated with econometric work, and not the broader literary tradition. This has been reinforced in economist's training as training in statistics has slowly replaced training in the broader literary tradition, and applied economics became more integrated with formal economic modeling. In the early 2000s in the US few graduate schools teach any elements of the broader Classical literary tradition. As that happened, the broader Classical views on coordination have been progressively lost, and pronouncements about coordination were derived more and more to the theorems

⁷ Keynes did not have much to say about empirical work, which was not highly developed at the time.

developed in positive economics, rather than from the precepts developed in the art of economics.⁸

As I discussed above, macro experienced a different historical evolution, but eventually it too lost the Classical story of coordination. Initially, that was not the case. Throughout the neoclassical period, macro theory was far less rigorous than micro. It escaped the “perceived rigor” requirement because trying to formulate a meaningful model of the macro economy in a rigorous fashion was seen as beyond the analytics of the day. Thus, even as micro was formalized in a neoclassical framework, macro theory remained within a broad informal Classical framework. Through the 1930s macro was loosely organized set of principles that guided policy within a broad Classical laissez-faire framework. It emphasized precepts of balanced budgets for political reasons, stable money, and maintenance of the gold standard to provide a stable international financial system. These were precepts, not theorems, and subject to change. The depression led to many Classical economists to change their views about what policies should be followed, and Keynes was one of the most vocal.

In understanding Keynes’ work it is important to remember that he wrote within that Classical framework that accepted the complexity of the aggregate economy. However in he felt that he had discovered a flaw in Classical reasoning about the self-correcting mechanism in Classical thought, and in the *General Theory* he attempted to develop a general theory of macro coordination failures. His explanation was primarily literary, and did not meet economics’ perceived rigor criteria. Most established economists felt Keynes’ *The General Theory* was an interesting, but flawed attempt that cut the Gordian Knot. Were it not for a set of fortuitous circumstances, Keynes’ “general theory” would have faded away. As I discussed above, that did not happen. Instead, it started what was then seen as a revolution. That revolution, however quickly morphed into something quite different from an exposition of the broader Classical understanding of dynamic macro coordination failures that I believe Keynes was attempting to get at. Instead it became embedded in a NeoKeynesian macro model that placed the story within a general equilibrium Walrasian framework. Instead of being a more general theory, Keynesian economics became a story of Walrasian general equilibrium with rigidities.

In the 1970s and 1980s, NeoKeynesian economics came under attack for lacking consistency with the constrained optimization model of individuals, which it definitely did. That led to the micro foundations, New Classical, Real Business Cycle, New Keynesian, and ultimately DSGE, models, all of which were designed to better integrate macro into the neoclassical framework. With the development of the now generally accepted DSGE model, macro theory has become integrated with Walrasian micro theory, and the coordination story it tells is essentially a constrained optimization with rigidities story.

Impressive as that story is, as a guide for macro policy, that DSGE model is unsatisfying for a number of reasons. The problem with it is that it only becomes tractable by assuming away central aspects of the macro coordination problem. For

⁸ In Colander (2006) I discuss this evolution in more detail.

example, it assumes a representative consumer and hence misses all the coordination problems that heterogeneous agents create. It assumes a unique equilibrium, and misses all the coordination problems that multiple equilibria create. It assumes away path dependency, and hence all the coordination problems that path dependency creates. Finally, it assumes that agents hold a single correct model of the economy, and in doing so assumes away all the problems that agents holding multiple models creates. In short, the model becomes analytically tractable only by assuming away many of the problem that are central to most macroeconomic questions.

If the DSGE model were only used as a way to generate macro theorems, and was accompanied with a discussion of the limitations of the model in guiding policy, I would have no problems with it. It is an impressive contribution. But, by many of the younger macro economists the model is used for more than that—and its results are directly applied to policy, significantly reducing the insights that the economics profession provides policy makers.

The Post Walrasian movement is an attempt to go beyond the DGGE model in theory, developing models that take seriously the characteristics of an aggregate economy that the DSGE model assumes away. It includes work in evolutionary game theory, non-linear dynamics, agent based modeling, experimental economics, experimental economics, new institutional economics, ultrametrics, random cluster analysis, cointegrated vector autoregression, and a variety of other new analytical and computational techniques that meet economics' "perceived rigor" criteria, which is why I predict that it represents the future of macroeconomics, and as that happens, what macro has to say about coordination will change. Within a Post Walrasian world achieving coordination is a much more complicated goal than it is a Walrasian world.

Conclusion

As I conclude, let me go back to my explanation of why economics remains queen of the social sciences. I argued that it is not any particular model or set of assumptions—not its assumption of rationality, or any commitment to equilibrium—that limits economics, but instead "perceived rigor." The history I discussed suggests that the analytical and computational tools available to economists drive cutting edge research, and determine what economics has to say about issues. Available tools, not interesting questions, are the primary determinants of the direction research in economic science. Assumptions of models are chosen because the analytic technology of the time required them to make the formal models tractable.

The approach has worked, and has kept economics the queen of the social sciences. But that regality comes at a cost, a cost that is demonstrated by the evolution of macro that I described. That evolution involved almost 70 years of research that gave us far less insight into the macro economy than we would have gotten had the limitations of our analytic methods to shed light on complex problems been recognized. In fact, I would argue that in terms of providing useful precepts for policy makers, macro at the turn of the 21st century is far below where Classical economics was back at the turn of the

20th century. It has taken us a hundred years to get almost back to where we were, all to protect our regalness.

The question I want to leave you with is whether maintaining economics' regalness is worth it. Should the coordination story economists tell policy makers be so dependent on the vagaries of analytic and computational technologies? My view is no, it shouldn't. I argue that modern economics should follow the Classical solution to the problem that our formal models are far simpler than the complex models that we need to capture reality. They had two distinct branches of economics. The first was pure or positive economics, which retained its regalness through its commitment to rigor, and which guided analytic theory. Its kingdom was the science of economics and it delivered its results in theorems. The second was the art of economics, or political economy. Its kingdom was the real world. The art of economics made no pretenses of being regal; it operated among the commoners, and, although it kept close contact with its regal cousin, positive economics, was not limited by it. It delivered its results in precepts, not theorems. The two branches of economics had different coordination stories that could exist simultaneously, because they operated in different kingdoms. In Classical economics applied economics existed in the art, not the science of economics, and the coordination story it told was a less rigorous, but far richer story than the coordination story told by the science of economics.

Modern economics combines the two kingdoms, and in doing so undermines its queenship. Instead of commanding economists to use whatever method they find most appropriate for the problem they are considering, it commands them to maintain rigor. In the science of economics, that works, but in the art of economics where rigor is almost impossible, it forces them to make connections between theory and policy that are tenuous at best. The strength of the "anything goes" approach that economics has in ideas is undermined. The Classical coordination story was maintained in the historical/literary branch of economics, but as the technical tools of economics have become more and more complex, the teaching of those technical tools has edged out the teaching of that historical/literary branch of economics. Today, that historical/literary branch of economics is being lost, since it is no longer part of economist's training. That, to me, seems too high a price to pay for regalness.

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