

# **Is Milton Friedman an Artist or a Scientist?\***

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Milton Friedman is a brilliant economist. He has significantly influenced microeconomic policy, macroeconomic policy, and economic methodology. Despite this influence, he is not seen as one of the fathers of modern economics. In fact, he is seen by many young economists as outside the mainstream in both methodology and in theory, even as the mainstream is supporting and adopting many of Friedman's policy proposals that it viewed as radical and outside the mainstream when Friedman first presented them. Underlying this majority assessment of his theoretical and methodological approach are the views that (1) his work is flawed by ideological biases, and (2) his particular brand of Marshallian economics is outdated, long ago superseded by modern Walrasian economics and advanced econometric techniques.<sup>1</sup> In the eyes of the mainstream, Friedman is passé.

In responding to these criticisms, Friedman's many supporters argue that he has been misunderstood, that he does not have an ideological bias, and that he has not been unscientific; they contend that his work is not obsolete but remains directly relevant. For example, in a recent article (1993a) and book (1993a) Thomas Mayer argues that in the context of its time, Milton Friedman's "Methodology of Positive Economics" was a "plea for a positivistic interplay of theory and observation." Giving Friedman's essay and approach a soft reading and casting it in a Mayerian reasonableness that removes its sharp edges and inherent pro-market biases that so excited critics, Mayer argues that one would expect that economists who adhere to the broad empiricist tradition of economics would applaud its influence. (p. 221) Similarly, Abraham Hirsch and Neil Di Marchi (1990) recently completed a sympathetic analysis of Friedman's work.

In this paper I consider these differing views of Friedman's supporters and of the mainstream. I propose an explanation for the differences and a modified interpretation of how Friedman's work should be viewed. Specifically, I argue that Friedman, like Alfred Marshall before him, tried to straddle a fence between policy and logical-deductive

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\* I would like to thank Daniel Hamond, Thomas Mayer, Abe Hirsch, and Kevin Hoover for comments on early drafts of this paper.

<sup>1</sup> To say that Friedman's approach is not mainstream is not to say that Friedman stands alone; he has widespread support and respect in the mainstream profession as a "grand old man"--as a pioneer who, like many pioneers, has been outrun by methodological and technical advances.

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theory, combining the artistic science of the historical and institutional school with the logical-deductive science of economics under a single category--which Friedman called positive economics. This combination worked for Marshall, in part because in Marshall's day formal logical-deductive economic analysis was still in its infancy, and so didn't need to be separated from applied policy economics, and in part because of Marshall's proclivity to avoid taking strong policy stands, hence avoiding normative issues in the art of economics.

The combination did not work for Friedman for two reasons. First, by Friedman's time, with the work of economists such as John Hicks and Paul Samuelson becoming widely known, formal theoretical work had generalized theoretical economic insights sufficiently so that the Marshallian straddle--the attempt to combine theory and practice--no longer made sense, if it ever did. Second, whereas Marshall was hesitant to make strong policy statements on specific issues, keeping his engine of analysis focused on broad policy issues, Friedman wasn't; Friedman combined a normative belief in individual liberty with his intuitive sense of theory to arrive at strong policy views with which many economists did not agree. Taking specific account of normative beliefs (beyond Pareto Optimality) is a necessary part of any meaningful applied policy work, or the artistic branch of economic science; it has no place in the logical deductive positive branch of economics.

The economist to whom Friedman attributed this positive/normative distinction, J.N. Keynes, provided an alternative to the Marshallian straddle that could have been a way out for Friedman. That way out was a separation of economics into three categories: positive economics, normative economics, and the art of economics. But, ironically, after citing Keynes' tripartite distinction, Friedman proceeded to discuss economic methodology as if there were a two part division. By not distinguishing between the methodology applicable for applied policy work and the methodology applicable for theoretical development work as Keynes had done, Friedman furthered methodological confusion in economics, as Keynes had argued such a failure to distinguish to two would do.

Since this division of economics is central to my argument, let me briefly summarize it.

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### **Positive Economics**

In Keynes's tripartite division, positive economics is a highly abstract logical deductive branch of economics; it is concerned with understanding the workings of the economy for the sake of understanding; it has no direct concern about policy implications.<sup>2</sup> Its appropriate methodology is debatable, but a case can be made for the current vogue--a highly abstract, unworldly, approach in which assumptions are unrealistic. It is to such an approach that Friedman's F-Twist is most defensible. Empirical work in positive economics is designed to test theories formally and possibly falsify them; if one can do so, one arrives at an economic law. Empirical work in positive economics has nothing to do with applying those theories to real-world problems. Almost all recent methodological discussions in economics have been directed at this branch of economics, as if it were the only branch of economics.

### **Normative Economics**

Normative economics is a philosophical inquiry into the goals of economies. It considers questions such as: What should an economy be attempting to achieve; How does one reconcile differing valuations of individuals; What moral limitations is it appropriate to place on values. Thus, normative economics is a philosophical inquiry directly focused on values.

The appropriate scope and method for normative economics received little consideration by J.N. Keynes, and has been little discussed since his time. Its method could be highly formal, as in much of social choice theory, or heuristic, as in Henry Simon's consideration of altruism. Its scope could be limited to economic issues, but could also be much broader. In either case, it is the branch of economics that interrelates with philosophy and ethics and that provides insights into the goals of economics.

### **The Art of Economics: Applied Policy Economics**

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<sup>2</sup> J.N. Keynes saw the scope of positive economic inquiry as applying only to economics, but it need not be so limited. For example, one could be advocating a positive social science and be searching for generalized rules of human action.

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The art of economics provides the connection between positive economics and normative economics. It takes the insights learned in the positive economics and the goals determined in the normative branch of economics and analyses how to realistically best achieve those goals. It concerns real-world solutions to real-world problems. It is to positive economics what engineering is to pure science. As I argue in Colander (1992b), its scope is necessarily broad; it must relate the laws of abstract theory to the real world; whatever considerations relevant to real-world policy that were ruled out in normative and positive economics must be added back in. For example, if the scope of positive economics is limited to economic issues, then the relevant sociological and political issues must be added in. Normative sensibilities, as well as explicit normative goals, must similarly be added to the insights of positive theory to arrive at policy prescriptions.

Whether or not the art of economics should be classified as part of the science of economics is not at issue. Science is many things to many different people. Thus, one can think of the art of economics as "policy branch of science" and of positive economics as "logical deductive branch of science." The point at issue is whether applied policy work should explicitly be subject to the same set of methodological rules as work designed to develop and understand the logical implications of theories. My argument is that it should not be. Separation is necessary to avoid methodological confusion.

An example of a minimum wage law may make the distinction clearer: Positive economics—logical deductive theory—tells us very little about a minimum wage law. It simply tells us that employment will be redirected away from those activities subject to minimum wages into other activities, not subject to a minimum wage. Based on positive theory alone, economics has nothing to say about whether a minimum wage law makes sense, and should be supported.

It does, however, provide a structure for judging minimum wage laws, incorporating judgments about issues, such as whether the redirected activities were undesirable, and, if so, how undesirable; what the redistributive effects have often been; and how people's attitudes are affected by that redirection. After incorporating these issues, many economists oppose minimum wage laws. That conclusion is what Keynes called a precept. It does not follow from theory, but is arrived at from a mixture of judgment and theory. All precepts belong in the art of economics, not in the positive science of economics.

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Empirical work in the art of economics is quite different than empirical work in positive economics. Empirical work in the art of economics is unconcerned with testing the theory as true or not; it accepts the theoretical insights developed in positive economics. What empirical work in the art of economics is concerned with is whether or not the theory fits the real world. That means adding back the relevant elements of the real world economy that the deductive positive science has removed. Doing so can involve significant statistical and econometric work, but the purpose of that work is not to test theories to see if they are true, but instead to “sift” information from the data, information that is to be integrated with one’s direct knowledge of the institutions so that the researcher can come to a policy recommendation based on the best available evidence.

Adding such realism requires judgment and the integration of quantifiable and non-quantifiable data. This integration reduces the degrees of accuracy and, following the law of significant digits, generally leads to highly informal empirical work. It would consider case studies, history, and common sense empiricism and statistical work, including data mining such as vector auto regressions. In this statistical work since the formal requirements of Classical statistical tests will not be met, the results of the tests will not be definitive, but will instead be simply a guide to the applied policy researcher in coming to a decision. Integrating these various forms of empirical data requires an intuitive sensibility. In the art of economics one is continually asking: Does the result make educated intuitive sense? (An educated intuitive sense differs from a lay person’s intuitive sense because it assumes that the person is aware of the insights found in logical-deductive science. For example it would involve the person knowing the equivalency of the income and excise tax, and all second and n-best arguments. These insights would be counter intuitive for the lay person.) Thus, in the art of economics, application is key and instrumentalism fits in perfectly. The purpose of the art of economics is application to real-world problems.

### **Friedman, Marshall, and the Art of Economics**

Keynes argued that it was necessary to use separate methodologies in developing theory—in positive economics, and in applying theory—in the art of economics. That separation of art and positive science would allow normative beliefs to play a key role in influencing policy, and would separate economic precepts—informal judgments that

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economists make, judgments that are debatable and based on a broad sensibility—from economic laws that follow from logical deductive theory. The Marshallian straddle, which Friedman followed, combined the two, and thereby led to "confusion" and "mischievous errors," precisely as Keynes said they would.

Had Friedman been faced with a choice of doing art--applied policy analysis--or positive economic analysis as it is currently practiced, I believe he would have unequivocally decided on applied policy economics, arguing that such applied work should be the primary concern of economists. (In fact, he might well argue that what I am calling positive economic analysis is not worth doing.) Instead he chose a methodological position that avoided that choice. He implicitly argued that theoretical analysis and applied policy analysis need not be separated. The profession followed his lead. An economist could do both theoretical analysis and applied policy analysis simultaneously. The problem this caused was that both were subjected to the same methodological rules, which quickly became the rules that science had developed to govern the choice of theories.

This combining of the art and developmental theory science, and their joint use of positive methodological rules, gave strong impetus for more and more resources to be devoted to theoretical analysis, and for fewer and fewer resources to go for applied policy analysis, since good applied-policy work did not fit the positivistic methodology of science as it was then interpreted. The result is the current state of academic economics which gives major emphasis to logical-deductive theory, is cynical about empirical work, and gives short shrift to history and institutions.

This is not to say that there is nothing called applied economics currently being done in academic economic research. But it is to say that much of this so-called applied policy work does not incorporate real-world complications with theory in a realistic way. It does not provide a link between positive economics and normative economics, but is instead simply a recharacterization of formal models to a slightly different setting. A previously developed technique or model is "applied" to a different area, and the formal equilibrium solution is arrived at. For example, a general maximization model exists which shows the equilibrium conditions that would exist if individuals maximized subject to certain constraints. This maximization model can be formally adapted to urban economics, health economics, public finance, and so on, but in each case the general structure of the solution remains the same.

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Such formal adaptations are the result of placing applied policy economics in the same category as logical-deductive theory. These formal adaptations look like impressive scientific models, but are of little use to most real-world policy makers, and are best identified as game-playing--useful as exercises for teaching and understanding the implications of the generalized model, but not especially helpful in applying economic insights to real-world problems. The tendency to call such pedagogical exercises "applied economics" has led to sterility and complaints that much economics is irrelevant.

I am not arguing that economists who are doing this applied policy research are not behaving rationally. They are; they are responding to institutional incentives within the academic community for publication. What I am saying is that the incentives in existing academic institutions are not toward doing true applied policy work. True applied policy work quickly becomes too institutionally specialized. It must assume in the reader of it a much higher awareness of those institutional issues than the readership of a general journal has. True applied-policy work might lead to internal policy papers but it is highly unlikely to lead to large numbers of journal articles. That's why we so seldom see government and policy institute authors of journal articles.

True policy work will not have a wide audience among academic economists (who are the primary readers and editors of those journals, publication in which counts for tenure) Pedagogical exercises which are useful for teaching or are self-referential to other academic articles have a much broader readership among academic economists. Thus, given the current academic institutions, it is only rational for academic economists to initially address other academic economists, not policy makers. Once one has a academic reputation and tenure one can do more meaningful applied policy work. But that work leads to fewer articles and less recognition among academic economists.

As I discuss elsewhere (Colander 1991), the reasons why the economics profession has gone the way it has involve incentives and institutional structure of the academic profession. Promotion and success depend on publication and if one can get a publication for doing an exercise that is useful for teaching the central elements of positive theory, the costs of learning those insights are reduced. Friedman is a strong opponent of this current state; (Mayer 1992) but ironically that state is due, in part, to the profession's acceptance of Friedman's methodological position that made applied policy analysis subject to the same methodological rules that govern the developmental theory branch of economics.

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### **Economists, Friedman's Methodology, and the Art of Economics**

Most economists care little about methodology; they're interested in getting on with what they do, not in talking about what they do. This has led to a new group of economics methodologists to discuss methodology in relation to how economists do economics rather than what they say about methodology (Daniel Hammond (1990, 1992), Abraham Hirsch and Neil Di Marchi (1990), and Thomas Mayer (1992, 1993)). Their approach is to study the works of economists and to draw out methodological principles from those works, rather than to look at what the economist writes about methodology. It is their work that has led to renewed interest in Friedman's and Marshall's methodology.

Because their work does such a good job in considering Friedman's methodology in this paper I do not try to analyze Friedman's approach or work in detail. This paper presumes a familiarity with this new methodological work, which provides an appropriately sympathetic interpretation of Friedman's methodology, making the points that (1) Friedman was a Marshallian; (2) Friedman was not a philosopher of science; (3) Friedman's work is coherent and consistent if one concentrates on Friedman's actual economic work, rather than on his methodological essay (giving his methodological essay a soft, rather than hard, reading). I agree with each of those three conclusions.

While I admire this new work, and find it of more relevance to economists than the more esoteric philosophical methodological literature, it remains tangential to most economists' concerns. Most economists want to get on with their work without thinking about those broader questions of whether their work has meaning and fits into a meaningful discipline that is uncovering, if not truth, at least insights into the working of the economy. Most economists implicitly believe the following: Just as a person who continually questions the meaning of life will probably be led into a catatonic daze; so, too, will an economist who continually questions the underlying methodology. Their position is: Let us get on with our work; let us follow the institutional incentives for tenure, promotion, and success.

There is a logic to this position. Academic institutional incentives are strong, and methodologists should be under no illusion that anything they write or say is going to change that. That said, there is a certain satisfaction that comes from believing that one's work has relevance, or, at least, from not having to directly face questions of its relevance. That's what's so comforting for the normal academic economic about the

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positive/normative distinction: economists are extending science which, by nature, is abstract and esoteric--studying economics for the sake of understanding the economy; non-economists don't appreciate economist's work simply because they don't appreciate science.

Thus, the methodological conceptions that academic economists have about their discipline play a role in determining what they do, especially when considered jointly in relation to the set of incentives that they face for job promotion and tenure. They justify to themselves their work with the belief that it is contributing to the growth of economic science. From day one they hear the positive science methodological prescription: develop a formal model and formally test it.

In the new methodological literature, it is clear that Friedman's brand of positive economics has a far more complicated methodological prescription underlying it than is captured in what is generally called positive methodology. Friedman's methodology involves intuition, a blending of institutional and theoretical knowledge, and a lack of interest in doing abstract theory.

The reaction of most economists to this new methodological work will, I suspect, be, "So what?" Friedman is not viewed as a role-model by most economists, but rather as an historical figure. Economists' interest is less in how economics has been done by stars in the past than it is in how it should be done. They are interested in prescription, not historical discussion.

In looking for prescription, they continually come back to the prescriptions that they have interpreted as positive economics: develop an abstract formal model and formally test it. The new methodologists nicely show that that was not Friedman's approach. But, as I stated above, it was Friedman's 1953 essay that enshrined that distinction in economic textbooks, which is probably the only place where most economists encounter methodological issues. Thus, ironically, the current lack of interest in Friedman's methodology may be due, in part, to Friedman himself.

In their appraisal of Friedman, Di Marchi and Hirsch derive five methodological rules from his work. These rules are the following:

1. Adopt an "outside" view of behavior.
2. Start with observation.

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3. Test implications continually.
4. Use the best knowledge available.
5. Do not look for answers "in principle" but address concrete problems.

These are not the methodological rules normally thought of as the rules of positive economics by the economics profession; these rules are not rules for testing to see whether a theory is true, but are instead rules for testing, using common sense, to see if the insights of economics theory are applicable to real-world problems. These rules are very similar to rules I have proposed for the art of economics (Colander, 1994) which is why I believe Friedman's work serves as an example of good applied-policy economics.<sup>3</sup> Thus, while Friedman's actual methodology is only of historical interest for the science of economics, it is of direct interest for the art of economics.

### **The Evolutionary Nature of Economic Methodology**

In any field there is an ongoing relationship between its applied policy branch and its theoretical development branch. As fields develop the appropriate methodology changes. Thus, just as Marshall argued that the appropriate natural science analogy for economics is evolutionary, so too is that the appropriate analogy for methodology.

A field of inquiry almost always begins with applied policy or art—researchers studying situations and finding reasonable rules of thumb for dealing with them. As the inquiry continues, certain similarities in answers to problems are found, and artistic generalizations made. Initially these artistic generalizations are heuristic, but as more people consider them, the insights become more and more formalized until someone "picks the oysters," making certain assumptions, showing the essence of the idea in an elegant formal model. In economics most of the oysters were picked in the period from 1930-1955, with the work of such economists as Samuelson, Lerner, and Hicks. They carried out the logic of the individual maximization model and strung the pearls of wisdom in general models. These general models convey the implications of economic theory far more efficiently than do artistic generalizations.

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<sup>3</sup> The only one of these that directly negates my rules for the art of economics is #1. I discuss that difference in an appendix.

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Once the general model has been developed, the nature of applied policy economics fundamentally changes. The oysters have been picked; now they need to be shucked, which means that the institutional issues and other assumptions that were put in the back of one's mind are added back in. In short, up until the 1950s, it may have made sense to combine applied policy and theoretical development; after the 1950s it did not. Once one understands the core theory, there is no need to redo the formal theorist's analysis in each area of application. Instead one can use the accepted theory, and deal with application of the theory—adding back the judgments, and the institutional detail necessary to come to a policy conclusion.

Friedman's methodological essay was written at the cusp of this transition; he picked some theoretical oysters—for example his work on the logical equivalency of an excise and income tax or his work with Savage on risk preference—but the majority of his work focused on the applied policy. Looking at the majority of his work, one sees it is highly policy-oriented. It often uses a heuristic methodology, integrating astute observation with a sense and a feel for the political institutions, a good sense of the implications of the theoretical economic model, and strong normative judgments about the relative importance of liberty for a good society. Judged relative to the methodology of the art of economics, Friedman is a brilliant practitioner.

### **Friedman, Marshall, and the Art of Economics**

In his writings, Friedman has made it very clear that he sees himself as a Marshallian; he is following in the Marshallian tradition. This is to be contrasted with the Walrasian approach which is a highly abstract mathematical approach to economics that uses a general equilibrium, rather than a partial equilibrium, framework. The Marshallian method involves the use of a less abstract mathematical approach that keeps assumptions in the back of one's mind and integrates real-world institutions into the analysis, whenever the analysis is applied. Most academic economists today consider themselves Walrasians; Friedman considers himself a Marshallian.

Exactly where Marshall stood on the art of economics is unclear. In early editions of *Principles*, he argued against a separate art of economics and focused on economics as a science. Marshall's concept of science, however, was not of a logical-deductive science as it is often thought of today. When Marshall and J.N. Keynes were writing, what J.N.

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Keynes called the art of economics was dominant in the German historical tradition. As discussed in Blaug (1980, p. 82), both Keynes and Marshall were attempting to reconcile the art of economics with the logical-deductive theoretical approach. In his integration Marshall gave short shrift to the "logical-deductive" approach; he combined it with the applied-policy economics. Keynes, on the other hand, left room for that "logical-deductive" branch of economics, and classified it as positive economics, leaving the art of economics as the applied policy branch.

By combining the two branches Marshall tried to forge a theory that could be everything to all people; that merged supply considerations with demand considerations, applied policy science with pure science, and formalism with non-formalism. Whereas Walras advocated a formalistic general equilibrium, Marshall, the mediator, advocated partial equilibrium. Marshall eschewed mathematics, but simultaneously structured his arguments in the *Principles* so that they could be deduced mathematically; those mathematics were placed in appendix if they were included at all. He incorporated enormous institutional and historical insights into his *Principles*, but he simultaneously removed "political" from the name of his discipline, calling it economics rather than political economy.

Marshall's argument against art and his focus on economics as science must be understood in the context of the times; when he was writing, economics as a separate discipline did not yet exist. The majority of economists he dealt with talked about policy, not theory. Only a small minority did theory or followed a logical-deductive approach. As he was writing *Principles*, he was also petitioning Cambridge to set up separate tripos in economics and was very much concerned that the objectivity of economics be maintained. (Marshall, 1902) Given his institutional needs, it is not surprising that he combined the art of economics and positive economics, and emphasized its scientific logical deductive nature.

Despite combining the art and positive branches of economics together, Marshall carefully separated out applied policy work from his analytic work. For example, when he discussed the art of economics in the fourth edition he wrote:

Of course an economist retains the liberty, common to all the world, of expressing his opinion that a certain course of action is the right one under given circumstances; and if the difficulties of the problem are chiefly economic, he may speak with a certain authority. But on the

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whole, though the matter is one on which opinions differ, it seems best that he should do so rather in his private capacity, than as claiming to speak with the authority of economic science. (Vol. 2, p. 154.)

Consistent with this view, Marshall was extremely hesitant to draw policy conclusions from economic theory. Policy issues--the art of economics--required normative and institutional judgments which had to be added back to the logical-deductive theoretical model. Policy conclusions did not follow from theory alone. What I am arguing is that Marshall's approach to real-world problems much more closely follows what I have suggested is the appropriate methodology of the art of economics than of what we currently think of the positive science of economics. For example, while he generally held to the quantity theory of money, he agreed that it would be swamped by other forces in individual cases. Alternatively, consider Marshall's analysis of the taxes that is discussed in Di Marchi and Hirsch. They point out that for Marshall the analysis of direct incentive effects was only a starting point of his analysis of taxes (Hirsch and Di Marchi p. 161). Another example they give is Marshall's consideration of the question of import duties. In that consideration Marshall states a variety of specific questions that need to be answered before one can come to a policy conclusion. They write:

Marshall operates not as a theorist who sets up his assumptions and then 'reasons out' (to some general conclusions for hypothetical categories of cases), *but as one who actually has to give advice, or to make the decision in favor of one tax over another, or for no tax at all* [emphasis supplied]. He cautions frequently against making direct application of the results of simple first-round impact analysis. A prefatory note in his Memorandum, for example, points out that 'the incidence of import duties is extremely complex' and he adds: 'the indirect are often much more important than the direct effects'. . . . Marshall also warns that although the exposition to follow is concerned chiefly with 'proximate causes and their effects' a student should actually be 'endeavouring to probe to the causes of causes'. . . . (p. 162).

Finally, Marshall's views changed over time. This is not surprising since Marshall and J.N. Keynes were close friends, and Marshall may have been influenced by Keynes's arguments for separating out an art of economics from positive economics. Between the third (1895) and fourth (1898) editions, a few years after Keynes' book came out,

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Marshall cut from his *Principles* a paragraph arguing against using the term "art of economics" (vol. 2, p. 154).

### **Art, Science, and the Teaching of Economics**

Both Friedman and Marshall were primarily teachers of economics. Indeed the primary reason why there is any interest in the positive/normative distinction at all is that it is about the only methodological distinction that economics students are taught. That, certainly, is the reason I am interested in it.

The teaching of economics poses additional constraints on the ideas one develops as an economist and, I think, failure to distinguish the pedagogical needs of teaching from the needs of positive economics and the needs of applied-policy economics has caused much confusion. Formal modeling of specific cases is often useful for teaching a general model. Formal modeling is also useful in teaching students to approach problems systematically, to separate out analysis from normative judgments. It follows that even if one believes that the art of economics is ultimately what most economists should be doing, and hence what should be taught, one does not necessarily go out and direct students to do applied policy work. One must first convey to students the general insights of the field upon which economists' general sensibility is based. Exercises applying the formal general model to a specific setting are a good way to accomplish this. Doing the particulars teaches one the specifics. They are wonderful teaching exercises, but they are not applied policy economics. Formal models of specific instances are to applied policy economics what exercises are to a sport: necessary to sharpen skills, but not the sport itself.

It follows that to teach students the art of economics, one must teach specific formal models. But if one is teaching these models as a foundation for applied-policy economics, the models must be taught as calisthenics--as an exercise of the students' minds to prepare them to start dealing with the more difficult issues of art.

The needs of models for pedagogical purposes are that they must be challenging, but not too challenging; the models currently taught in introductory and intermediate economics are approximately right for their intended purpose of exercising students' minds. Combining applied-policy economics with positive economics has, however, led to a tendency for these exercises of the mind to become more and more elaborate and to

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create students who are the Charles Atlases of such exercises, but have no training in going beyond such exercises and doing applied policy work. These exercises in logical deductive theory become the final products and much of what goes under the name "applied economics" is actually simply a logical deductive exercise recasting a general theory for a particular case.

In Marshall's time, the general sensibility of economics could be conveyed to students relatively simply; empirical methods had yet to be developed to the level where one could reasonably think of formally testing a model, and the generally-accepted models were not beyond the analytic capabilities of the students.

By the 1960s when Friedman's influence was at its peak, that was changing, and in the 1990s it has changed enormously. Today, it is essentially impossible for the majority of students to learn both the sensibility and institutional knowledge necessary to conduct the art of economics effectively, and the techniques necessary to conduct research in the positive science of economics. If they learn about history and institutions, they don't have time to learn how to conduct research at the highest level of theory; similarly if they learn the appropriate techniques for extending the theory, they don't have time to learn about the institutional and political sensibilities necessary to apply a theory. Division of labor is necessary--and the appropriate training of applied-policy economists significantly differs from the training of economic scientists, concerned about extending the model.

### **Conclusion**

I began the paper with the statement that Milton Friedman is a brilliant economist but that the majority assessment was that his work was ideologically flawed, and that the Marshallian economics he advocated had been superseded by Walrasian economics. These criticisms, I believe, stand, if Friedman is viewed as a positive scientist as the profession currently defines positive economics--as logical deductive exercises and empirical testing of those deductive results (testing which, as Rosenberg (1992) argues seldom comes). But that, I argue, is not how he should be viewed; he should, instead, be viewed as an economic artist--as an applied policy economist extraordinaire. As an economic artist, Friedman has almost no peer in economics, and his primary artistic flaw

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has been his failure to make clear the importance of the artistic component of his economic science.

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### APPENDIX: Friedman and Inside and Outside Views of Behavior

The one methodological rule which Hirsch and Di Marchi attribute to Friedman which does not fit my list of rules for the art of economics (Colander 1994) is the rule: "Adopt an "outside" view of behavior." Hirsch and Di Marchi interpret this rule as denying the ability to build realistic models and denying the applicability of introspection in considering issues. (pg. 162)

As I understand it, Hirsch and Di Marchi are suggesting that Friedman uses an anti-general theory rule--a rule which would deny the existence of any positive economics as I define it, leaving only art. Hirsch and Di Marchi recognize that this is a likely interpretation and specifically argue that this is the wrong interpretation. They suggest that the rule leaves a role for theory "as a creative process, introducing potentially useful interjections between broad sets of ideas and the data and the problems at hand." (pg. 155) This is reminiscent of Marshall's "engine of analysis" interpretation of the role of theory. But in Marshall there is a definite role for introspection. As Hammond (forthcoming) points out, Marshall maintained "one fundamental idea--the driving force to equilibrium of supply and demand--in considering issues. Marshall did not require the observer to take an outside view of behavior and used educated introspection throughout his analysis. Thus, "the outside view of behavior" rule is not consistent with Marshall's engine of analysis use of theory; it is an enormously strong rule which limits the use of certain types of common sense when approaching problems.

Moreover his is unclear to me how anyone can achieve an outside view of behavior. Friedman certainly did not. Consider Friedman's statement about the role of the market in his *Price Theory: A Provisional Text*.(1961) He writes:

In any short period of time when the amount of a product is relatively fixed, there must be some way of adjusting consumption to production. This rationing must be accomplished in one way or another. There may be rationing by favoritism, bribery, chance, or by prices. When people are allowed to bid freely for goods, even if the quantity of the goods available is completely fixed, prices will adjust themselves in such a fashion that the quantity people want to buy *at the market price* is equal to the quantity available.

Prices, therefore, do three things in solving the above five problems. They transmit information effectively and efficiently; they provide an incentive to users of resources to be guided by this

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information; and they provide an incentive to owners of resources to follow this information. (pg. 10)

This, he claims, is a descriptive statement, not a justification of markets. He continues:

In any normative judgment of the price system on the basis of the preceding description, several things must be kept in mind. First, this description implicitly supposes the existence of effective competition in translating consumer wishes into productive activity. It is assumed that people can affect their incomes only through use of their resources and not through interference with the price system. There is freedom to compete but not freedom to combine. Second, the controlling force is pecuniary demand; voting is in proportion to the number of dollars a person has. This is not obviously "just". The basic inequality, it should be noted, is an inequality in the ownership of resources. What the market does is primarily to determine the return per unit of resource, and there is no reason to believe that the market aggravates the inequality of the ownership of resources. Moreover, any given degree of inequality is a much more serious one in an economy which is governed largely by status or tradition than in a market economy where there is much chance for shifts in the ownership of resources. Historically, the fundamental inequality of economic status has been and is almost certainly greater in economies that do not rely on the free market than in those that do. (pg. 11)

The first part of the above statement is his summary of the information of positive economic theory. It does not prove that prices that actual markets reach transmit information effectively and efficiently. If it is description as he states it is, it is description that is based on his introspection. This is not surprising since description necessarily involves interpretation of observations and those interpretations are necessarily influenced by one's sense which is acquired from introspection. A complete outside view of human behavior is impossible.

Friedman is tentatively willing to take the above insight from economic theory as true. He is further willing to make the provisional judgment that further theorizing about the market has little to add to this important insight, so he moves on to applied-policy issues. Many economists do not accept that judgment; work in what is called the positive branch of economics is designed to get a better handle on how "prices transmit information effectively and efficiently."

The second part of the statement involves his subjective judgments about how politics, economics, and sociology interact. These judgments may be reasonable, but they

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are not derived from economic theory; they are derived from introspection—from a sense of the way the real world works, or what Hirsch and Di Marchi call an "insider" view of behavior. If that is correct then, in making these judgments, it seems that in this general statement about doing economics Friedman has violated the methodological prescription Hirsch and Di Marchi attribute to him.

My conclusion from the above discussion is that the outsider rule that Hirsch and Di Marchi attribute to Friedman is not quite descriptive of Friedman's methodology. His methodology is slightly more complicated. He is quite willing to accept the theoretical insights given in the page 10 quotation cited above. He is also willing to make certain judgments about political, social and institutional realities based on introspection. These judgments, combined with the insight of economic theory, lead to the conclusion that prices transmit information effectively and efficiently, and they do it better than any other means of rationing. With this conclusion, why do any further work on abstract economic theory? Only the applied-policy branch of economics remains relevant. And, given his judgments about politics and institutions, the applied policy goal of economics should be to convey that theory of how the market works to lay people who are not privy to that deep insight about the market.

Thus I would disagree with Hirsch and Di Marchi that Friedman's methodological thinking can be characterized as adopting an outside view of behavior; instead, I would argue it involves adopting one big inside view of behavior--that markets work--and thereafter, an outside view.

Many economists were unwilling to take that one big inside view of behavior--and, hence, could not follow Friedman's economic methodology. The alternative for them was presented as the logical deductive Walrasian approach. The choice they faced was (1) follow a Walrasian approach and question the effectiveness and efficiency of markets; or (2) follow a Marshallian/Friedman approach and do not question the effectiveness and efficiency of the market. Relying on an underlying positivist, scientific philosophy, most economists chose the Walrasian approach because it seemed the most honest scientific approach, the one most consistent with their interpretation of the theoretical work on markets--that given certain assumptions the market was efficient and effective, but given other assumptions, it might not be. This view allowed a role for positive logical deductive economics considering when markets were efficient and when not.

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Essentially, in this paper the approach I am proposing for most economists is a third approach: follow a Friedman policy-oriented method approach, but leave open the possibility of questioning the effectiveness and efficiency of the market. Doing so requires one to take an inside view of behavior, admitting that economic theory does not tell us that real world markets necessarily transmit information effectively and efficiently, but otherwise is quite consistent with Friedman's methodological approach.

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## **Is Milton Friedman an Artist or a Scientist?**

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(Revised version August, 1994)

## **Is Milton Friedman an Artist or a Scientist?**

### **Abstract**

Most economists will agree that Milton Friedman is a brilliant economist. Yet, the majority assessment is that his work is ideologically flawed, and that the Marshallian economics he advocates has been superseded by Walrasian economics. In this paper I argue that the reason for this negative assessment is that Friedman, like Alfred Marshall before him, tried to straddle a fence between policy and logical-deductive theory, combining the artistic science of the historical and institutional school with the logical-deductive science of economics under a single category which Friedman called positive economics. This combination worked for Marshall, but did not work for Friedman.

I argue that the profession's criticisms of Friedman stand, if he is viewed as a positive scientist as the profession currently defines positive economics--as logical deductive exercises. But that, I argue, is not how Friedman should be viewed; he should, instead, be viewed as an economic artist--as an applied policy economist extraordinaire--whose primary flaw has been his failure to make clear the importance of the artistic component of his economic science.