Private Vices, Scientific Virtues:
Using Economics to Think about Science

THOMAS C. LEONARD

Department of Economics
Fisher Hall
Princeton University
Princeton, NJ 08544
United States

tleonard@princeton.edu
phone: (609) 258-4036
fax: (609) 258-5561

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Abstract

This paper makes a case for using economics to study science and its product, scientific knowledge. Traditional theories of science – due mainly to epistemology – imply that science is successful *because* scientists are selfless truth seekers, and because they rigidly adhere to a method. Post-modern theories of science – due mainly to sociology and literary theory – argue that science cannot be successful, *because* scientists are neither disinterested nor selfless, and because methodological rules derive from a faulty epistemology.

An economic theory of science argues that, contra the traditional view, successful science doesn't require the restrictive premises of Traditional theory of science, and that, as a result, contra the Post-modern view, successful science is not ruled out when those premises are. An economic theory of science accommodates both a realistic conception of scientific motivation and procedure, *and* the possibility of genuine scientific success. In so doing, it offers an intellectual means to address a central question in the theory of science: how do self-interested scientists, who have wordly goals, come to produce the collectively beneficial outcome of reliable scientific knowledge.

Keywords

Economics of science, philosophy of science, bounded rationality, institutions, scientific knowledge, economic methodology

1. Introduction

This paper takes up a conundrum that immediately presents itself to the economist considering science. Economics offers a famously unflattering theory of human action. It says that people are self interested, calculating, and strategic. But when considering their own motives, and those of other scientists, economists generally resort to a gentler conception imported from Traditional philosophy of science, one which portrays scientists as selfless, disinterested, rule-abiding truth seekers. Since scientists are people, too, one or the other view must be wrong, and consistency requires choosing. (Colander 1991, Leonard 1998).

Economics has not much attended to science, itself a puzzle, so the inconsistency of treatment has mostly gone unremarked.[1] Perhaps vanity is at work; *homo economicus* is, after all, not the most flattering self-portrait. More plausibly, we suggest, the contradiction between our conception of ordinary agents and our conception of scientists, has its roots in a central problem in the theory of science more generally.

The problem in theory of science is this: if we conceive of scientific motivation and procedure in economic terms, how does science ever achieve the collectively beneficial outcome of reliable knowledge production. That is, if scientists' motives are epistemically impure, or if they don't always do what method prescribes, or if method is itself fallible, how does science come to produce reliable knowledge?

Traditional (a.k.a "positivist") theories of science argue that it cannot. Science works, on the Traditional account, *because* agents are selfless truth seekers who possess and observe a method which guarantees the production of scientific knowledge. Post-modern theories of science, which are otherwise opposed to the Traditional view, agree. Science, say Post-modern theorists, cannot produce scientific knowledge, *because* scientists are neither disinterested nor selfish, and because method derives from a faulty epistemology.

Ordinarily bitter rivals, Traditional and Post-modern theories of science here make common cause: they both conclude that science cannot succeed without selfless, rule-following truth-seekers. They differ only on the question of whether scientists (and their procedures) can reasonably be so described. Given a realistic depiction of scientific motivation and procedure, neither Traditional nor Post-modern theories of science, therefore, can explain scientific success.

An economic view of science aims to provide that explanation. Unlike Post-modern and

Traditional theories, economics insists upon a realistic conception of scientific behavior, *and* upon the possibility of good collective outcomes. In offering an invisible-hand explanation of scientific success, the economics of science helps to tackle a central problem in the theory of science.

The paper proceeds as follows. In Section 2, we motivate our inquiry by surveying the field, especially the views of scientific motivation and procedure for the two theoretical approaches we label Traditional and Post-modern. In Section 3, we examine and reject the Post-modern conclusion that scientific knowledge is not possible. Having preserved the possibility of scientific success, there remains the task of explaining *how* scientific success can occur with self-interested scientists. This task is begun in Section 4, where some concluding remarks are also given.

2. Theories of science and of scientific knowledge

As long as there has been science, there has been theorizing about science, especially about its chief product, scientific knowledge. Because students of science are theorizing about theorizing, their business is sometimes called meta-theory or meta-science. The meta-theorist thinks about science and scientific knowledge in the same spirit that the scientist thinks about his own subject. Though the questions will often be different, thinking about science and scientific knowledge need be no different, in principle, than thinking about evolutionary biology or quantum mechanics or price theory. The economist Andrew Schotter puts it this way: 'We can aspire to alter the world, or to alter the way we see the world. One approach is to theorize directly about the real world, whereas the other is theorize about the theory existing to describe the real world . . . .' (1981: 144).

The theory of science was long seen as the rightful domain of epistemology, or philosophy of knowledge. Traditional theory of science, an amalgam of positivism and analytical philosophy, makes epistemology its home port. Many still use the term "philosophy of science" as a generic description of any theorizing applied to science, especially to scientific knowledge. Epistemologists attended mainly to scientific knowledge, without worrying much about the social aspects of science. Sociologist Robert Merton (1973) exploited this lacuna, theorizing about the social aspects of science.

A pioneer, Merton maintained the traditional "fire-wall" between the study of science and the study of knowledge. The social context of science, Merton argued, can influence science, for example, by affecting the selection of research problems. (Merton 1957: 554, cited in Cole 1992: 4). But the actual cognitive content – scientific knowledge itself – is another matter altogether. Merton argued that:

> The criteria of validity of claims to scientific knowledge are not matters of national taste and culture. Sooner or later, competing claims to validity are settled by the universalistic facts of nature, which are consonant with one and not with another theory[2] (ibid).

This deference to the authority of philosophy did not last. As even a casual newspaper reader now knows, contemporary theory of scientific knowledge is a more plural enterprise. Historians, psychologists, sociologists, ethnographers, and theorists of rhetoric and literature have taken up not just science, but scientific knowledge itself.

With this transformation has come controversy, recently manifested in the so-called "science wars." The newer students of scientific knowledge are far more skeptical of science's accomplishments, and decidedly less reverential towards both its practices and its practitioners. Indeed, in some Post-modern corners, a debunking attitude towards scientific knowledge is axiomatic, built into the very enterprise.

We now turn to the underlying issues which have given rise to the recent upheavals in theory of science and of scientific knowledge.

2.a. Traditional theory of science (the received view)

It is impossible to characterize an entire intellectual tradition in a few paragraphs, but let us try to represent some fundamental ideas in Traditional theory of science. Traditional philosophy of science can be seen as having two key components: one, a theory of the nature of scientific knowledge, and, two, a theory of how that scientific knowledge is produced.

First, the Traditional view says that science is special, an epistemologically privileged realm that can be demarcated from other social arenas, such as politics or religion, and even from other knowledge producing enterprises, such as the humanities. Science can be demarcated because it alone produces *epistêmê*, that is, knowledge that is true in virtue of how the world actually is. *Epistêmê*, properly scientific knowledge, is to be distinguished from all other kinds of knowledge, which are deemed mere opinion, or *doxa.* (See the Appendix for more elaboration).

Second, science produces scientific knowledge owing to its producers' motives and their *modus operandi*. Individual scientists share the great collective goal of producing scientific knowledge; and they selflessly collaborate in order to achieve it. The scientist of the Traditional view is a selfless seeker of truth, with no other (operational) goals.

In addition, scientists produce scientific knowledge by following explicit rules for developing, testing and appraising rival theories. A complete set of these rules – a scientific method – provides a kind of infallible recipe for producing scientific knowledge.[3] In particular, there exists an objective body of brute facts about the world, identical and indubitable for all inquirers who bother to check, which can be used to unambiguously adjudicate among rival theories. Says Larry Laudan (1984) in this regard:

> At least since Bacon, philosophers have believed there to be an algorithm or set of algorithms which would permit any impartial observer to judge the degree to which a certain body of data rendered different explanations of those data true or false, probable or improbable . . . Science was regarded as consisting entirely in claims about matters of fact. Since scientific agreements were thought to be, at bottom, disagreements about matters of fact, and since disagreements of that sort were thought to be mechanically resolvable, philosophers had a ready sketch of an explanation for consensus formation in science (Laudan 1984: 5-6, cited in Cole 1992: 6).

In short, science works because scientists are selfless, because scientists have only about truth, and because scientists possess and observe an algorithmic method that ensures the production of scientific truth, all of which redounds to the benefit of society.

Put in this radically elliptical fashion, we have what is sometimes called the "received view" in the philosophy of science (Suppe 1977), variants of which continue to exist in economics and elsewhere in science. (Refer to the Appendix for more elaboration). As depicted, it is clear why one might like to wear the mantle of "scientist." It certainly sounds less dismal than "selfish, calculating maximizer."

2.b. Revisionist theory of science

But history has been unkind to the received view. Particularly unkind has been the field that spawned it, philosophy of science. Beginning in the mid-1950s, a "second generation" (Callebaut 1993) of philosophers of science — Karl Popper, Stephen Toulmin, Norwood Hanson, Willard Quine, Imre Lakatos, Paul Feyerabend, and Thomas Kuhn among them — assembled a compelling critique of the received view. We focus primarily on their critique of scientific method.

Revisionist skepticism follows from their criticism of the Traditional view of theory testing, in particular, the nature and function of facts. Facts, said Quine in a famous paper (1953), do not exist prior to or free of all theoretical interpretation; experience is not prior to all belief. Facts are, rather, unavoidably contaminated by the theories devised to explain them. Scientists have no choice but to look for data under the street lamps of theory, and different observers can therefore construe the data differently (Hanson 1958). Facts are *theory-laden*. And if facts are theory-laden, that is, if they are not identical and indubitable for all inquirers, then they cannot unambiguously serve as a neutral court in which to try rival theories.[4] (Kuhn 1992: 4-5).

A related revisionist critique, which goes by the name of the Quine-Duhem hypothesis, attacks the Traditional conception of theory testing on another front. Even if scientists can reach agreement on what the facts are, there is the problem of determining exactly what the facts say regarding the theory they are confronting. Testing failure cannot unambiguously reveal what has gone wrong.

Was it the theory, or the evidence? And, if it was the theory, which aspect – the main hypothesis (e.g., consumers maximize utility), an auxiliary hypotheses (e.g., subjective rates of time preference are nonnegative and constant), or a *ceteris paribus* condition? In Quine's elegant phrasing: "our statements about the external world face the tribunal of sense experience not individually, but only as a corporate body" (Quine 1953: 41). Thus, when tests are disconfirming, it the scientist, not the "facts" who must determine which part (or parts) of the "corporate body" has been refuted. 'The choice of where exactly to point the accusing finger of refutation,' says Martin Hollis, 'is ours, not nature's. . . .' (1994: 80).

A key implication of the Quine-Duhem problem is *the underdetermination of theory by data,* the idea that several theories can prove equally compatible with a given body of evidence. As a result, the data cannot always, by themselves, determine a uniquely best theory. Choice among theoretical rivals has an unavoidably non-evidentiary aspect that cannot be captured in an algorithm of theory choice.

The non-evidentiary aspects that bear on theory choice may be aesthetic – elegance, beauty and parsimony are non-empirical criteria that will be familiar to economists. Other reasons for choosing a particular theory when the data don't select are more suspect (if still familiar): conformity with political ideology, consistency with theoretical priors, likelihood of promoting wealth, esteem or fame. Whichever, the insight is that factors beyond evidence can influence not just research-project selection but scientific knowledge itself. Thus, it is through the revisionist breach opened by theory ladenness and theory underdetermination that social science, and with it Post-modern theory, has entered the theory of science.

2.c Postmodern theory of science

In more recent times, revisionist theory of science has given rise to a more radical variant, what we will call Postmodern theory of science. The Postmodern theorist – who generally resides in a Sociology or English Department – denies science any epistemic privilege. He reasons: *because* scientists are self-interested and have non-cognitive goals, and *because* method is epistemically flawed, scientific knowledge is not possible. If the Enlightenment spirit of Traditional theory of science implies that science can know everything, the Post-modern *ethos* is the polar opposite: science can know nothing.

The radical Post-modern conclusion derives from two sources: (1) an especially strong reading of the revisionist arguments on theory ladenness and theory underdetermination, and (2) a view that self-interested scientists preclude good scientific outcomes. There is interesting empirical work – enthnographic and historical – that lends credence to (1) and (2) by revealing actual science to be far messier and rule-flouting than Traditional theories can explain.[5] (See, for example, Latour and Woolgar [1979] 1986, Knorr-Cetina 1981, Pickering 1984). Let us take up (1), the Post-modern view of scientific method, first.

Post-modern theorists generally interpret theory-ladenness and theory-underdetermination in the strongest terms. On their reading, theories are not merely *under*determined by existing evidence, they are essentially *un*determined. As such, successful theories are selected wholly (or predominantly) based upon social factors, to the exclusion of empirical evidence. Says the sociologist Harry Collins, for example: "the natural world has a small or non-existent role in the construction of scientific knowledge" [6] (1981: 3).

Post-modern theory takes the revisionist idea that social factors may *influence* theoretical choices and renders it into the far stronger claim that social factors *determine* theoretical choices. If the received view said 'its all Nature', the Post-modern view completely inverts it, saying 'it's all Society'. In this sense, as sociologist Steve Woolgar puts it, "science itself is not

SCIENTIFIC accept insofar as it represents itself as such'"(Emphasis original, 1988: 107).[7]

To this view of method, the Post-modern theory of science adds a theory of scientific motivation. Scientists are not the selfless truth-seekers of the Traditional view. Real, flesh-and-blood scientists have motives other than acquiring knowledge; they seek professional esteem, prestige, wealth, even revenge upon enemies, no less than do other agents. Scientists are also fully capable of pursuing their individual interests. Though radical in the gentlemanly precincts of traditional theory of science, this conception of human action is less alarming to economists.

The Post-modern view of scientific motivation and practice, results in cognitive relativism, what Laudan calls cognitive egalitarianism, "he thesis that all beliefs are epistemically or evidentially equal in terms of their support" (Laudan 1984: 29-31, cited in Cole 1992: 42). For the Post-modern theorist, scientific knowledge is epistemically on all fours with the knowledge produced by social practices of any kind, such as literature, or mythology, or religion.

In the Post-modern account, the theoretical knowledge of a modern physicist is indistinguishable from that of the Azande poison oracle (Campbell 1988: 446). And by implication, Traditional methodological rules cannot function as guides to good scientific practice, since there are not better or worse practices. Methodological rules are therefore best seen as self-serving, *post hoc* attempts to legitimate currently dominant practices.

If greatly simplified, these synopses will serve as our working accounts of the Traditional and Post-modern theories of science. Now we briefly turn to the matter of how these ideas in the theory of science manifested in economics.

2.d Economic methodology: Theory of science in economics

Until the mid-1970s economic methodology, to say nothing of economics, was quite traditional in its view of science. It was not uncommon for introductory elementary textbooks to include a prefatory section on Traditional ("positivist") method (see, e.g., Richard Lipsey (1963), *Introduction to Positive Economics,* in note 3, supra). Economic methodology attended to revisionist theory of science (especially Popper, Lakatos and Kuhn) in the mid-1970s (see Latsis 1976), but truly Post-modern theories did not arrive until *circa* 1983, with the publication of McCloskey's paper, *The Rhetoric of Economics*. By 1983, revisionist theories of science had been underway for a generation, and the version that finally reached economics was radicalized by a long gestation in Post-modern corners of the academy, particularly the English and Sociology departments.

Much revisionist theory of science thus came to economics in Post-modern garb. "New Methodologists" such as Klamer 1983, McCloskey 1983 and Weintraub 1989 are less the descendants of revisionist philosophers like Popper, Quine and Kuhn, than of Wayne Booth, Stanley Fish and Richard Rorty, two literary theorists and a philosopher most influential in modern language departments. Another notable "new methodologist," Philip Mirowski (1994) likewise draws on sources more Postmodern than internal to revisionist philosophy of science.

It is, of course, an oversimplification to ignore programmatic differences among the scholars we lump together as "new methodologists."[8] But, for present purposes, recognize that the new methodologists, however otherwise diverse, are united in their endorsement of the central conclusion of Postmodern theorizing – they all agree that scientific knowledge is unobtainable, and that the historic methodological function of prescribing rules for good scientific practice is, thereby, obviated. The new methodologists are all on the Post-modern side of the epistemological divide, the very same divide that gives rise to the central problem in theory of science. (Gerrard 1990).

Even in economics, then, theorists are expected to accept what is a false dichotomy: the choice between a realistic conception of scientific motivation and the prospect for successful science. But, survey in hand, we are now in a position to argue against that false dichotomy, beginning with the Post-modern claim that science cannot succeed.

3. The Post-modern claim: scientific knowledge is impossible

The problem in science studies is this: how to reconcile self-interested agents with successful science. Much of the Post-modern critique is persuasive to an economist. Scientists are not the bloodless, selfless paragons of legend, they are, rather, just like everybody else. There is indeed historical evidence that the actual practice of science looks different from what Traditional philosophy says it should look like (Feyerabend 1975). In economics, certainly, it is clear that the Popperian norm – reject falsified theories – is hardly ever observed. And, finally, there are indeed important epistemological shortcomings in the Traditional theory of science, and these shortcomings undermine the Traditional ideal of a infallible, universal method.

But these sensible critiques are not, by themselves, sufficient for the Post-modern conclusion that science can know nothing. Even if scientists' motives are epistemically impure, or if they rarely do what method prescribes, or if method is itself fallible, it does not *necessarily* follow that scientific knowledge cannot be produced. The reason is simple: science *does* produce scientific knowledge, provided one adopts a more modest, and more realistic conception of what scientific knowledge actually entails.

No review is required to see that some sciences have enjoyed spectacular success in predicting, explaining and understanding aspects of the physical world. Science, David Hull (1988) reminds us, sometimes does exactly what it claims to do. And unless that success is a miracle of happenstance, there is something important (and worth studying) in the way that successful sciences proceed.[9]

A solution to the problem involves several steps. The first step is to uncover a little-noticed flaw in the Post-modern reasoning, i.e. to show how its radical conclusion does not directly follow from its stated premises. The second step is to suggest how the Post-modern conclusion can be avoided. These arguments are presented in the remainder of Section 3.

3.a Antifoundationalism and the certainty criterion

The first step towards a solution of the problem is to indicate why the Post-modern conclusion is wrong. This demonstration involves showing that their conclusion requires a hidden premise, and that the hidden premise is faulty, an incongruous hangover from the received-view epistemology that Post-modern theories otherwise reject.

Recall that Traditional philosophy of science insists on certainty. *Epistêmê* must rest upon indubitable foundations. Revisionist philosophy of science quarreled with the Traditional theory's identification of scientific knowledge with certain knowledge. A set of revisionist critiques, which goes by the cumbersome name of antifoundationalism, argues that certainty is unobtainable is science, because indubitable foundations are not to be had. (See Quine 1953, Kuhn [1962] 1996, and Hanson 1958. See Appendix for more elaboration).

Not surprisingly, Post-modern theorists of science tend to be antifoundational. But antifoundationalism is not the sticking point of the science studies puzzle. Most contemporary theorists of science are antifoundational. Following Dewey, they generally agree that epistemology's 2500 year-old quest for certainty has proved a failure. Says David Hull, echoing many others:

> [T] content . . . of science can[not] be "justified" in the sense that generations of epistemologists have attempted to justify them. The reason that epistemologists have not been able to justify knowledge-claims in their sense of "justify" is that no such justification exists. They want the impossible. (1988: 12-13).

What distinguishes the Post-modern view of science is not antifoundationalism, properly understood, but its view of antifoundationalism's *consequences* for knowledge.

The Post-modern argument against scientific knowledge goes as follows:

> (1) properly scientific knowledge (*epistêmê*) must be justified and justification entails certainty (Traditional epistemology);
> (2) certainty is unobtainable (antifoundationalism);
> hence, (3) properly scientific knowledge is impossible (Post-modern conclusion).

The Post-modern conclusion – scientific knowledge is not possible – follows *only* if one endorses the hidden major premise, that scientific knowledge requires certainty. Antifoundationalism is not, by itself, sufficient. Post-modern theorists are therefore in the awkward position of conceiving of knowledge as *epistêmê*, a central element of the Traditional theory of science they otherwise reject.

Ironically, then, both Traditional and Post-modern theorists are united in the belief that scientific knowledge entails certain foundations, differing only on the question of whether certain foundations can be had. Traditionalists say yes, scientific knowledge is possible, because certain foundations can be had. Post-modern theorists say, no, scientific knowledge is not possible because certain foundations cannot be had.

Thomas Kuhn, a founding revisionist, was unsettled by Post-modern interpretations of his work, and pointed to this unlikely but crucial epistemic affinity between Traditional and Post-modern theories:

> [Post-modern theorists of science] are taking the traditional view of scientific knowledge too much for granted. They seem, that is, to feel that traditional philosophy of science was correct in its understanding of what *knowledge* must be . . . . If science doesn't produce knowledge in that sense, they conclude, then it cannot be producing knowledge at all. (Kuhn 1992: 9).

It is not merely a matter of strange bedfellows. For if the identification of knowledge with certain knowledge is wrong, then the Post-modern conclusion – "no knowledge is possible" – is also wrong. In other words, if one can conceive of scientific knowledge in more modest terms, then the Post-modern conclusion – "no knowledge is possible" – simply does not follow. What this alternative conception entails is scientific *knowledge without certainty*.

3.b Uncertainty and the economic dimension of knowledge

Once one abandons certainty, then scientific knowledge can be seen, like all human knowledge, as fallible and corrigible. (Popper 1959; Musgrave 1993: chapter 15). A more pragmatic conception of scientific knowledge treats it in terms of its **reliability** (Ziman 1978), the extent to which decision makers can rely upon it when making choices in an uncertain world.

What makes knowledge valuable to decision makers, scientific and otherwise, is its reliability. Newtonian mechanics is an exemplar of scientific knowledge, but it is not certain and is, in fact, wrong in places. Still, Newtonian mechanics is highly reliable in many settings, sufficing for valuable tasks like building long-span bridges or sending a machine to a particular spot in the solar system.

When a credible central bank adopts a tight-money policy, short term interest rates are likely to rise, and with them, the cost of financing housing and durable goods purchases. Macroeconomic knowledge such as this is less reliable than Newtonian mechanics, for one cannot readily forecast the exact timing nor the exact magnitudes of changes in spending. But one can be fairly sure of the *direction* of change, and this knowledge, however imprecise, can be valuable to decision makers.[10]

The recognition that knowledge can simultaneously be fallible and valuable, has a leveling effect which invites an economic approach. When one accepts that uncertain knowledge has value, the emphasis shifts from the epistemologist's all or none appraisal, to consideration of the decision maker's practical judgment under uncertainty, where there are incentives to improve the reliability of one's beliefs, and costs (in time, effort and risk) thereto.

To insist on certainty is therefore to adopt radical skepticism. It amounts to a refusal to accept any cognitive risk, i.e., the risk that one's working beliefs – the inputs to decision making – are incorrect. (Rescher 1989) An economic approach insists that, given uncertainty, agents must trade off cognitive risk (and the costs of time and expenditure) against the potential benefits of more reliable knowledge. The marginal increase in expected reliability may or may not justify the marginal cost of obtaining it.[11]

For a scientist, to insist on certainty is tantamount to a refusal to even enter the knowledge producing enterprise. (Rescher 1993: 86). This makes sense only for the radical skeptic, who assumes that the benefits of uncertain knowledge are zero, and that, therefore, no expenditure on human inquiry can ever be sensible. But requiring certainty is clearly untenable for the scientist, as it is for the everyday agent. Both accept the risk of cognitive error, along with other costs of inquiry, precisely because there are positive expected benefits to greater knowledge.

An economic approach to knowledge departs from Traditional and Post-modern theories, which identify knowledge with certain knowledge, by recognizing value in uncertain knowledge. An economic approach also recognizes that the benefits of knowledge are costly to produce, owing to scarce temporal and cognitive resources and to the risk that one's knowledge inputs are wrong. By implication, some knowledge will not be worth producing.[12]

3.c. Pragmatism does not dispense with epistemological concerns

On the pragmatic view of economics, scientists, like everyday agents, have worldly incentives to obtain reliable knowledge, not merely the putative desire to meet some abstract epistemological standard. The world punishes the scientist who employs unreliable knowledge, just as it does the agent who lends to the local bank at two percent, while revolving credit at eighteen percent. But the pragmatism of an economic approach to knowledge does not, as some Post-modern theorists would have it, relieve us of all epistemic concerns. On the contrary, given that some strategies for acquiring (or producing) knowledge are better than others, worldly incentives also give decision makers good reason to attend to epistemic matters. Uncertainty is not license to assume that all strategies for acquiring knowledge are equally good.

Epistemology recurs because everyday and scientific agents alike are drawn to processes, such as science, that produce especially reliable knowledge or that produce reliable knowledge at lower cost. Reliable knowledge tends to be empirically well founded, and "well foundedness" is, in turn, a staple of normative epistemological inquiry.[13] If agents want to understand, control and function in the material world, consideration of how well, and under what circumstances, beliefs actually comport with reality is unavoidable. And unless enterprises that historically produce reliable knowledge do so entirely by accident, their methods are worth studying.[14]

There are, to be sure, beliefs that make no claim to refer to the material world, and there are beliefs that cannot feasiblely be tested in any event. Reliability is not the point with aesthetic or spiritual knowledge, say. But to the extent that reliable knowledge *is* a prospect for a knowledge-producing enterprise, there are practical incentives for the scientist, like the everyday agent, to learn and to emulate the processes that successfully and efficiently produce reliable knowledge.

Note well that none of the foregoing requires "privileging" science *per se*. As Larry Laudan points out, what is called science in our culture is "epistemically heterogeneous." There are beliefs considered scientific that are not especially well founded, and there are "nonscientific" beliefs that are very well founded. (1995: 222). The carpenter's practical knowledge that miter joints are stronger than butt joints is not considered scientific, but it is well founded. (ibid: 220). There are "scientific" beliefs in Physics – such as those connected with "Superstring" theory – that are not, as yet, empirically well founded, and may well be untestable (Horgan 1996).

What makes a belief scientific, and what makes a belief well-founded are thus two different questions (op. cit.: 222), and the Traditional project of demarcating knowledge into "scientific" and "non-scientific" categories may not be especially fruitful. But foreswearing demarcation does not dispense with epistemology, it merely refocuses attention on familiar, pragmatic questions about warrantability. In particular, when can we regard our knowledge as well-founded (hence reliable), and why does the set of social practices traditionally called "science" tend to produce especially reliable knowledge?

Theorists of science are not obliged to do epistemology, but neither can they completely avoid its traditional normative concerns. It is logically possible that happenstance explains the relative success of science in producing reliable knowledge. But to simply assert this *a priori* is to uncritically adopt radical skepticism at the methodological level. Since radical skepticism is a strategy that Post-modern theorists of science likely (and sensibly) eschew in their own everyday activities, they should pause before attributing it to their fellow scientists.

4. Science as an invisible-hand process

We have argued against the Traditional and Post-modern identification of scientific knowledge with certain knowledge, and rejected the Post-modern conclusion that scientific knowledge is *a priori* unobtainable. But even if one accepts that the Post-modern conclusion is wrong, there remains the problem of scientific motivation alluded to at the outset.

Traditional theory of science says that successful science requires scientists who are collective minded and strictly truth-seeking. Post-modern theory of science says that science doesn't work, in part because scientists are neither. Both theories effectively presuppose that science cannot work absent collective-minded truth seeking. How is it, then, that more or less self-interested scientists, who also may have non-cognitive goals, come to produce the collectively beneficial outcome of reliable knowledge production?

A complete answer to this question goes well what can be presented here. (The reader is referred to Hull 1988, Kitcher 1993, and Leonard 1997). But any adequate answer must show that collective minded truth seeking is not a necessary condition for the production of reliable knowledge, that, in Philip Kitcher's terms, even "epistemically sullied" scientists can produce good scientific outcomes. This requirement, in turn, suggests another fundamental economic idea: unintended consequences, especially beneficial ones. We argue, in fact, that science can fruitfully be seen as an invisible-hand process. An invisible-hand process is characterized by the following conditions: (1) individual actions lead to unintended consequences; (2) the aggregate effects of individual actions result in a spontaneous order[15] that gives the appearance of design by a master planner, and (3) the order that results is deemed beneficial in ways that the individuals did not intend but nevertheless find desirable (Vaughan 1987).[16]

Some conceptions of invisible-hand processes omit element (3), that is, allow for bad rather than good unintended consequences. Bad unintended consequences are also an important economic idea. Poorly motivated policy interventions are one example. Common property resources (grazing land, riparian water, fisheries) are another. (When the resource is rivalrous in consumption and exclusion of non-payers is not possible or not cost-effective, self-interested individual choices create external costs that unintendedly lead to inferior collective outcomes, i.e., to overuse or even to destruction of the resource). Given our argument that science is *successful* in producing reliable knowledge, we adopt Karen Vaughan's definition, which applies to *beneficial* unintended consequences, i.e., the successful alignment of individual and collective interests.

Let us be clear. We do not attempt to argue that science everywhere and always produces reliable knowledge, or that science is optimally configured, or that the current institutional structure which gives rise to (some) reliable knowledge production is superior to all possible alternatives.[17] Our aim is more modest: we argue that the success of science is not incompatible with a more realistic (i.e., economic) conception of scientists' motives and procedure. We thus occupy the vast but underpopulated theoretical space between the Traditional view that science will always succeed (given collective minded truth seekers) and the Post-modern view that science can never succeed.

4.a The importance of institutions to invisible hand outcomes

Virtually alone among scholars, economists see spontaneity in social orders (such as markets), and the beneficial aspects thereof.[18] Other scholars find our invisible-hand arguments counterintuitive. Similar to Traditional theorists of science, they see agents as collective-minded creatures, which obviates any need to align private interests with the common good.[19] Or, like Post-modern theorists, they admit self-interest, but suppose that, absent the heavy (visible) hand of authority, pursuit of self interest cannot lead to order, never mind order in the common good.

We conjecture that invisible-hand theorizing is unpopular with non-economists in part because it is too often confused with a vulgar kind of laissez-faire. But the original and greatest invisible-hand theorists – Bernard de Mandeville, David Hume, Adam Smith and others we can (loosely) group into the Scottish Enlightenment – were not proponents of anything goes. They were, in fact, clear that government had a crucial (albeit limited) role to play in underwriting the possibility of invisible-hand outcomes. Smith, for one, conceived of his work as a "science of the legislator," that is, as advice to lawmakers on how to create an institutional structure – what today we might call constitutional rules – that could best foster invisible-hand outcomes. In modern language, the invisible-hand theorists recognized that functioning markets rely upon a substructure of rules that credibly establish and enforce the protection of persons, property, and contracts.[20]

The crucial point for our purposes is this: invisible hand processes do not work in any environment. Even competitive markets produce good collective outcomes *only with a robust institutional structure.* An institutional framework must be in place for the invisible hand to work its magic, and this will be as true in science as in ordinary markets. But, given the right institutional structure, science, like markets, can sometimes succeed, even with scientists who individually do not intend to promote its collective success.

By implication, then, a central task of an economics of science is to show that science possesses an institutional structure sufficient to permit invisible-hand outcomes, i.e. sufficient to produce reliable knowledge with self-interested scientists. (See Dasgupta and David 1994, Leonard 1997). To this end, we next identify several key scientific institutions, and suggest how they work to align individual and collective interests.

4.b Scientific institutions that create the right incentives

Let us proceed with institutions in science by posing and answering three basic questions concerning incentives: (1) why do scientists produce knowledge at all; (2) why do scientists openly publish their results; and (3) why do scientists tend to produce *reliable* knowledge?

The first question – why produce at all – has been emphasized by an older tradition in the economics of science (Nelson 1959, Arrow 1962). The "old" economics of science is more concerned with technology than with science *per se.* It begins with the idea that knowledge is a public good, i.e. non-rivalrous in consumption. Because the individual scientist may have a problem appropriating the returns to his discovery (once made public), a market failure results – the usual underprovision of a public good owing to the divergence between individual and social returns. (Dasgupta and David 1994: 490).

The traditional remedy for this "market failure" is state intervention.[21] The old literature thus did not much attend to the question of whether and to what extent science has already made provision for this kind of coordination problem. And, as such, it paid little heed to unique institutions of science – its rules, norms and conventions – taking them as given to the extent they were considered at all. (ibid: 492).

But long before the advent of government science, science evolved a set of institutions to meet some of its coordination problems. The institution that addresses the difficulty of excluding non-payers once knowledge is made public, is known as *credit.* Scientists who produce a useful idea receive payment in the form of credit, generally in the form of citation. In science, credit (and its opposite, discredit) are the coin of the realm. (See Latour and Woolgar 1986; Hands 1994).

If others use your output (and acknowledge that use), then you will be rewarded with credit. As Brian Loasby puts it: 'just as the market rewards knowledge which enables someone to offer goods and services which customers wish to acquire, so the reputational system rewards those who produce new ideas which others can put to use: and if the goods are or ideas are unwanted or defective, they will be ignored or criticized.' (1989: 39). A scientist's reputation can be thought of as the stock of previous credit and discredit. One can see credit as a goal unto itself or as a means to acquiring non-cognitive goods such as prizes, promotion, higher wages, election to learned societies and collegial esteem.[22] (See Merton 1973: 297-303; Stephan 1996).

The evidence that scientists care about credit is compelling. There is, first, some evidence that wages are positively correlated to citation. Examining economists' salaries, Hamermesh (1989) finds that higher pay is indeed robustly associated with greater citation (cited in Colander 1989). Second, there is the relative paucity of anonymously published work. A selfless truth-seeker might well publish anonymously, but anonymously published results are the rare exception the history of science. Third, consider the scientific institution of *priority*, the convention of awarding first discoverers all the credit. The selfless truth-seeker should be indifferent to priority in discovery. But even a cursory look at the history of science reveals the opposite; scientific history is rife with disputes over priority.

Robert Merton (1973) documents that the battles over priority are fierce, recurring, and involve some the greatest scientific names. Newton, Hooke, Liebniz, Huygens, Cavendish, Watt, Lavoisier, Farday, LaPlace, several Bernoullis, Legendre, and Gauss are some of the worthies that Merton identifies as having been involved in priority disputes. Were scientists without non-cognitive goals of the kind made possible by credit, then we would observe few disputes over priority. The recurrent struggles over priority that characterize much of scientific history are compelling counterfactual evidence.

The way in which credit is awarded in science is connected to our second question: why do scientists publish their results. There are two scientific institutions at work here – *priority* and *open publication*. In academic science, (1) credit is, as noted, generally awarded entirely to first discoverers (priority), and (2) credit is generally awarded only upon publication (the institution of open publication). It is typically difficult to obtain credit for results not published, or for results that have already been published by others.

Open publication and priority are norms intended to promote rapid scientific innovation and the wide dissemination of knowledge. Clearly, scientists will often have individual incentives to keep their results secret, or to delay publication, so the norms *qua* norms are not, by themselves, sufficient to induce cooperative behavior. The institutional "solution" has been to bundle credit with priority and open publication.

Generally, credit is awarded primarily to those who publish first. And, since credit is earned primarily when others use one's results, publication is seen as a necessary precondition for obtaining credit.[23] The tradeoff for the scientist who wants credit is clear: waiting too long to publish risks losing priority and therefore credit, while rushing into print risks errors which, if they ramify, will lead to discredit. (Hull 1988: 352). More pre-publication work increases reliability, but risks being "scooped."

An additional tradeoff arises when more than credit is at stake, as when there are potential financial gains to a discovery. The incentive to obtain credit by publishing can be overwhelmed by the prospect of pecuniary gain, i.e., complete public disclosure is not always in the individual scientists' interest.[24] Priority and open publication are efficacious institutions, but only to the extent that the incentive to obtain credit is sufficient to offset any commercial gains which will tend to demand secrecy.

Of course, rapid dissemination of ideas is not the only rationale for publication, so too is the policing function so characteristic of science – the certification of ideas as reliable. This takes us to our third query, why scientists produce reliable knowledge. There are two important scientific institutions here: *peer review* and *replication*.

Individual scientists, like all agents, accept much knowledge on faith. If every scientist were obliged to independently test the reliability of every bit of knowledge they used in their research, science would simply stop. Reliability is nonetheless vital, so science has evolved a social system of trust, which is built upon a process of verification via peer review and replication. Scientists have some assurance that their knowledge inputs are reliable, because they know that independent peer reviewers have directly assayed the ideas in question, and that subsequent "users" have indirectly done so.

Publication is generally necessary but not sufficient for obtaining credit. Credit accrues only when others *use* one's ideas, which, in turn, requires that they be reliable. Knowledge which proves unreliable will bring discredit, and will fall into disuse. In the empirical sciences, then, there are strong incentives to produce reliable knowledge. Research output is "quality checked" not only by peer reviewers, but also by subsequent users.

We don't wish to suggest that peer review and replication are without problems. Replication is not a simple matter practically or philosophically (Collins 1985). In many fields, direct *positive* replications (in the sense of reproducing a published result) are rarely publishable.[25] Some review processes are more demanding than others; the refereeing processes is itself vulnerable to corruption,[26] and it is probably the fate of most published papers never to be read, much less checked.[27]

Still, for knowledge that does survive beyond publication, replication is pervasive in the empirical sciences. Those who use other scientists' results as an input very much want those results to be reliable. Scientists are unlikely to knowingly sabotage their own work by choosing inputs that are faulty. As such, a kind of indirect replication is in force, because serious errors will ultimately manifest themselves, with adverse reputational consequences for the scientists who are the source of the error (especially if due to fraud or carelessness). For an idea that is actually used, survival is a tentative signal that one can rely upon it, without independently assaying it.

Direct replication, in contrast, is not accomplished in the course of doing one's work and is therefore costly in time and expense. Fortunately, competition provides some incentive to replicate directly. It is unrealistic to expect scientists to rigorously attempt to refute their own hypotheses. But, as Hull points out, their rivals will be happy to do so (1988: 4). Competition helps promote replication, because competitors have an incentive to refute results they find inimical to their own work.

There is, for example, a well-known recent literature in labor economics which finds that recent minimum wage increases do *not* result in adverse employment consequences for low-skilled workers (Card and Krueger 1995). Those who find this result congenial, such as political proponents of minimum wage increases, are unlikely to scrutinize Card and Krueger's methods too closely. But those who believe that a minimum wage does have adverse employment consequences, and have built scientific reputations on this view, are likely to examine Card and Krueger's controversial findings rather skeptically, and have, in fact, done so. (See, e.g., Neumark and Wascher 1995, Hamermesh 1995. For more on the controversy, see Leonard 1997, chapter seven).

There are two important aspects of competition in science. The first is analogous to ordinary markets. Competition in science works to eliminate error in a kind of self-correcting fashion. Errors are exposed *not* because scientists are wholly unbiased or disinterested, but precisely because their competitors have a "partisan" interest in exposing their errors. This function of competition is what Popper (1975: 93) referred to when he said: 'should individual scientists ever become 'objective and rational' in the sense of 'impartial and detached', then we should indeed find the revolutionary aspect of science barred by an impenetrable obstacle.' (Cited in Hull 1988: 359).

The second aspect of competition in science differs from competition among firms in ordinary markets, and works to keep scientific competition from becoming unhealthy. Scientists are both producers and consumers of knowledge. (In this sense, scientists use knowledge as a kind of capital good, an input in the production of new knowledge). As consumers of knowledge, they are dependent upon the very scientists who are also their rivals in the output market.

This is so for two reasons. First, scientists are likely to benefit from using their rivals' results. Rivals are, after all, working on similar problems and, given specialization, are the likeliest source for useful knowledge. Unlike ordinary firms who are rivals, scientists need their competitors's work (as an input) even as they compete with them in output markets. In this important sense, rival scientists are mutually dependent.

Second, not only is a scientist likely to need his rivals' work, he is also likely to need their support. A scientist wants other scientists to make use of his output. This is not merely pride, but the fact that, unique in science, credit is typically awarded only by fellow producers. Scientists need the output of others, for knowledge is the crucial input to knowledge production; and they also want their own output to be used, for credit cannot otherwise be obtained. Thus competition in science is tempered by a kind of mutual interest that has no direct analogue in ordinary markets.

Mutual interest goes to another incentive question: why scientists generally give credit where credit is due, that is, why scientists practice honest citation. Why cite properly, when it is costless to plagiarize and thereby receive credit for ideas one didn't actually produce? One might get caught, of course, with the attendant reputational disaster that likely will follow. But plagiarism, unlike unreliable work where errors eventually manifest in future research, is not subject to indirect replication. The determined plagiarist can go a surprisingly long time before detection. (See the case of Dr. Elias Alsabati in Kohn 1986).

Mutual interest among scientists, however, creates an incentive to cite others, in order to enlist their support, which is necessary for obtaining credit. We need our fellow scientists, in part for their output, and in part for their support, without which credit for our own output cannot be had. Honest citation is not only a Mertonian norm – the proper respect for intellectual property rights – nor is it motivated solely by fear of detection. Mutual dependence also makes it in the scientist's interest to cite properly.

This takes us to a final, important caveat. All of the foregoing presumes that reliable knowledge is attainable, which, in turn, presupposes an empirical enterprise capable, at least in the long run, of meaningfully discriminating among alternative ideas on the basis of reliability. If empirical testing is not possible or if it is not practiced, then reliability is not meaningful in our sense, and some of the scientific institutions we have presented as enabling invisible-hand outcomes will be ineffectual. If, for example, others cite work solely to obtain a reciprocating citation, mutual interest can degenerate into logrolling and thereby becomes a less effective means of promoting intellectual property rights.

But when ideas can meaningfully be checked against reality for reliability, there will be a selection process in science, as in markets, that rewards successful practices. In ordinary markets, "success" entails profitably satisfying the sovereign consumer, whatever his preferences. In science, the "consumer" of knowledge can also have idiosyncratic preferences, but will always require reliability. In the long run, scientists have powerful incentives to use reliable ideas and to eschew unreliable ones.[28]

When science succeeds it is because its institutions are robust – credit, priority, open publication, peer review, and replication. The reputational system induces scientists to openly and rapidly produce ideas by rewarding them with credit. Peer review and (direct and indirect) replication provide additional incentives to produce ideas that are reliable, i.e. ideas which other scientists can use. When scientists change their mind, it is not merely a matter of virtue, but because there are incentives to do the right thing. (Galison 1987). Even with "Post-modern" scientists, the right institutions can work to provide the collectively beneficial outcome of reliable scientific knowledge.

Recall that we do not claim that science always produces reliable knowledge, or that it is optimally configured, or that its current institutional structure is superior to all possible alternatives. Scientific rules, norms and conventions are imperfect, and they are not all selfenforcing. We know this because plagiarism, lazy practice, data massaging, and even outright fraud are part of the everyday stuff of science. (Kohn 1986, Broad and Wade 1982).

But this should not surprise us, for also it is true of other social locations, such as markets, which involve invisible-hand outcomes. It is well to remember, as David Hull notes, that 'the important feature of science is not that it *always* produces increased knowledge, but that *sometimes* it does.' (1988: 26). Like the .300 hitter in baseball, science is exceptional because it fails only most of the time. It is precisely because science fails only most of the time that its processes, especially its enabling institutions, merit our close scrutiny.

Appendix: The received view: more on the Traditional theory of science

Positivism, admixed with other constituents of philosophy of science influential until the mid-fifties, yielded an amalgam sometimes called **the received view** of science (Suppe 1977). Whatever label one employs, the received view comprises several ideas about science important to our discussion. Ian Hacking's (1981) list of what he takes to be the essential characteristics of the received view of science is a useful place to begin. The essential characteristics are:

1. **Scientific realism**: science is an attempt to find out about one real world; and there exists a unique best description of that world.

2. **Theory** and **observation** are separable.

3. There are empirical **foundations** to knowledge. Observation and experiment provide foundations for and **justify** theories.

4. The **context of discovery** is separable from the **context of justification**;

5. Science is **progressive**; new knowledge builds cumulatively on what is already known.

6. **Demarcation**: scientific beliefs are distinguishable from non-scientific beliefs.

7. **Unity of science:** there should be just one science about the one real world.

8. Theories have a **deductive structure.**

9. Scientific concepts are **precise**, and have fixed meanings. (1981: 1-5, with minor modifications).

A. Scientific realism

Of Hacking's nine tenets, which covers a vast terrain in philosophy of science, we focus on the first five. Take item one, scientific realism, first. Here there are several key ideas about the nature of reality (ontology) and our knowledge of it (epistemology). **Realism** is the doctrine that there really is an objectively existing world "out there." Says philosopher John Searle: 'I have defined realism as the view that the world exists independent of our representations of it.' (1995: 153). A realist believes, therefore, that the earth existed before there were humans to perceive it and to think about it. A realist perspective also implies that 'when we die, and all our representations die with us, most features of the world will remain totally unaffected; they will go on exactly as before" (ibid, p. 154). Thus defined, realism is an ontological position, not an epistemic one (ibid).[29]

Scientific realism adds an epistemic component. It claims not only the objective existence of an external world, but also that scientific claims about that world are true. (See Kitcher 1993: 169). Scientific realism conceives of scientific knowledge as representations of a singular reality, and sees representations (theories, beliefs) as corresponding to that one world — the better the correspondence, the closer to truth. Hence, for the scientific realist, theories are true or false in virtue of how the world really is, and, at the limit, there is a theory which best corresponds to reality. (On scientific realism, see Hacking 1983).

Scientific progress (item 5), then, consists of better and better representations of reality. New science builds on existing scientific knowledge and, in so doing, provides deeper or broader correspondence. A common analogy (popular in introductory economics textbooks) proposes seeing theory as a map, and reality as the terrain to be mapped. Maps can provide more detail for a given region (deeper correspondence) or can map a larger region, encompassing *terra incognita* (broader correspondence). New maps are better than old maps when they provide better correspondence. And the best "map," completely exhausts the theoretical referent, such that no other map can ever be better. (On the map analogy in economic theory see Goldfarb and Griffith 1991; on "exhaustion of the referent," see Leonard 1997: section 4.9).

The received view also puts special emphasis on empirical testing as the means of justifying knowledge claims (items 2, 3,4). Scientific theories should have testable implications which can be compared with the world. "Maps," after all, refer to a world that is "out there."

B. Certainty and foundations to knowledge

Item three is the most fundamental: it says that knowledge, if it aspires to scientific status (*epistêmê*), must be justified. (See Leonard 1997: appendix 2A). Justification consists in the provision of proof and other evidence. Unjustified claims are condemned to remain mere opinion (*doxa*). The kicker is this: philosophy of science has also argued that justification requires **certainty**.

The emphasis on certainty reflects the influence of Traditional epistemology. "Can we know anything with certainty?," goes the epistemologist's traditionally most urgent question.

The epistemologist Alan Musgrave has said: '[E]pistemology is actually a long and still unfinished war between dogmatism [yes, we can] and skepticism [no we can't].' (1993: 10).

The requirement of certainty, in turn, gives rise to foundationalism, the need for certain foundations if we are to have properly scientific knowledge. Suppose we claim that some proposition *C* is true because we have proven that it follows from proposition *B*, which we know to be true. The obvious question — "how do you know *B* is true" — is successfully answered only by reference to another proposition, "because it follows from *A*, which is true." An infinite regress is halted only if, "at bottom," there are propositions which are self-evidently true. Since most knowledge is inferential — it builds on other propositions we take to be true — the foundationalist argues that this inferential chain must terminate with knowledge that is beyond doubt. At bottom, then, properly scientific knowledge must rest on a foundation of certainty. (Hollis 1994: 67-71).

The idea that scientific knowledge entails certainty was widespread during and after the Enlightenment. 'Bacon, Locke, Leibniz, Descartes, Newton and Kant . . . may disagree about how precisely to certify the certainty of knowledge, but none quarrels with the claim that science and infallible knowledge are coterminous.' (Laudan 1995: 213). Isaac Newton could not say what gravity was, nor did he know what caused it. But, says Laudan, he nonetheless 'regarded his noncausal account as [scientific] because of the avowed certainty of its conclusions.' (ibid). The practice of science changed during the Enlightenment, but its philosophy continued to make certainty the badge of demarcation.

For an empirical philosophy of science, the indubitable foundations are data – objective facts obtained from observation and experiment. Facts are, at once, prior to theories, for theories are built to explain them, and they also (once a theory is built) can be used to test theories. A theory (or any belief) is thereby justified by its concordance with the facts, which are seen as objective.

What makes facts objective — so called "brute facts" — are two claims, one ontological and one epistemic. The ontological claim is realism again— there is only one world out there, hence aspects of that world are everywhere identical. The epistemic claim is that facts about the world are accessible to and indubitable for all inquirers. (Kuhn 1992: 4-5). Facts are given. Anyone can get out there and see for himself.

Hacking’s item two – theory and observation are separable – restates the idea that facts are objective in terms of theory testing. If facts are to serve as an independent arbiter among theories, then facts cannot themselves be influenced by or the products of the theories they are meant to test. Theory and observation must be separable, i.e. facts must be generated by a process that is independent of the theories they will be asked to confront. That facts are independent of theory is a crucial assumption an empirically-based theory of science.

Item four — the distinction between discovering theories and justifying then — likewise matters for the an empirical theory of science. The distinction, which is due to philosopher Hans Reichenbach, says that the creation of theories is fundamentally different from the process of justifying them. Inspiration, creativity, the social milieu, funding sources, and the scientist’s birth-order, are all examples of things which may influence a given theory’s development, or discovery. The factors that relate to theory creation are not of interest to the Traditional philosopher of science — they belong to the murky domains of psychology, sociology and history. Philosophers traditionally care only about what happens to a theory after it is born, in particular how it is justified. The received view asks questions such as: is the theory logically consistent, is it supported by observation, confirmed by experiment, compatible with other theories we take to be true, and so on. (Hacking 1983: 5-6).

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[1] Only a few scholars have explicitly used economic ideas as a template for thinking about science, and an even smaller minority are themselves economists. Some exceptions in economics are Tullock 1966, Colander 1989, 1991, McCloskey 1990, Mayer 1993, Feigenbaum and Levy 1993, Coase 1994, Dasgupta and David 1994, Wible 1995, Diamond 1996, Leonard 1997. Philosophers of science who make use of economic ideas are David Hull 1988, Michael Ghiselin 1989, Nicholas Rescher 1989, 1990, 1993, and Philip Kitcher 1993. William Bartley 1990 and Gerard Radnitzky 1986 are Popperian scholars who also use an explicitly economic framework. Michael Polanyi's 1962 view of science as self-correcting in the sense that competitive markets are, is an important early account. Antedating all of this work by a century is an obscure paper by the American polymath Charles Sanders Peirce, "The Economy of Research," first published in 1879. Peirce constructs a formal model of research project selection that maximizes the (expected) utility of a research project, subject to its expected costs. Peirce's remarkable paper is reprinted in Wible 1994.

[2] Cole reports that this quote was added as a footnote in the 1957 reprint of Merton's famous 1942 paper on scientific norms. Intriguingly, when the paper was added to the 1973 collection of Merton's papers, Merton altered the last sentence to read: 'Sooner or later, competing claims to validity are settled by universalistic criteria.' (Cole 1992: 4).

[3] Consider Richard Lipsey's (1963) textbook account of "postivist" method, as represented in Hollis 1994: (1) start with generalizations about human nature (premises); (2) logically deduce implications thereof (hypotheses); (3) test hypotheses against observations; (4) if hypotheses comport with the observations stand pat, if not either (4a) reject the hypothesis in favor of a better alternative, or (4b) modify the premises in light of the evidence, starting again at the top.

[4] As an illustration, consider an example from Alan Musgrave (1993). Say I my theory holds that oars are straight. When out rowing, I observe that a partially submerged oar appears bent. The data conflicts with the theory. Suppose I ascertain that the oar is not really bent, but only appears to be, because light propagates through water differently than through air, so that the bent oar is an illusion. Hence I reject the facts rather than the theory, and sure enough, once free of the water, the oar is straight. Note the ambiguities that this creates for the idea of empirical testing. Interpretation of facts opens the door to **different**, subjective views of the same data. I opted to interpret the facts *in light of another theory* (the propagation of light) **before** applying them to my straight-oar theory. The brute fact of a bent oar would otherwise been disconfirming -- a bent oar falsifies the straight-oar theory. But another observer without knowledge of propagation of light theory would not have read the data as I did. She might have applied a different theoretical interpretation of the observed bent oar, or might have seen the bent-oar as falsifying the straight-oar theory. The Quine-Duhem problem says: when evidence contradicts theory, should one reject the theory (and if so, which component) or reject the evidence? Should I reject my straight-oar theory (or some aspect thereof) or the bent-oar evidence?

[5] Post-modern (usually sociological) theorists embrace and extend the revisionist emphasis on *historicism* – the idea that history of science matters to theory of science. In contrast to the armchair approach of the received view, the second-generation theorists believed that history can, in effect, **test** theories of science. Larry Laudan says that Kuhn in particular advanced the concept that '[h]istory is philosophy teaching by examples" (Callebaut 1993: 11). If one believes that economists should, after Milton Friedman (1953), concern themselves solely with prediction, or, after the "early" Karl Popper (1959), ruthlessly falsify economic hypotheses, then it is surely relevant to see whether economists have, in fact, done so. As with any social theory, what agents actually do should influence what methodologists say they should do. Post-modern theorists practice ethnography – the study of scientists in their native habitats by participant observers (e.g., Knorr-Cetina 1981) – as well as more traditional methods, such as historical case studies and interviews (e.g. Pickering 1984).

[6] Sometimes it seems that Post-modern theorists take primacy of social factors over empirical evidence as a theoretical imperative more than as the conclusion of their research. Collins, for example, says that sociologists 'must treat the natural world as though it in no way constrains what is believed to be' (cited in Hull 1988: 4).

[7] The alert reader will have noticed that this sweeping epistemic claim indicts sociology along with the other sciences it studies. There is, as Larry Laudan observes, something contradictory in an enterprise that presents lots of evidence to show that evidence is irrelevant (Laudan cited in Hull 1988: 4). The self-defeating aspect of relativism is an important if ancient problem, one we do not pursue further here.

[8] See, for example, Mirowski 1988 (chapter 8), which contains a withering critique of McCloskey.

[9] Some thoughtful philosophers of science take seriously the idea that scientific success is pure happenstance. Mary Hesse, for example, says; "science might, after all, be a miracle" (1980: 154).

[10] *How much* reliability is required for scientific knowledge be socially useful is a different and, arguably, more difficult question. Keynes, for his part, dreamed of a day when economics was as useful as dentistry,

i.e. when our knowledge of economic processes would be sufficient for policy interventions that are as ameliorative as dentistry. We set aside the special difficulty that policy interventions can lead to changes in behavior which may undermine the theory upon which the original policy was based.

[11] Seen this way, the certainty requirement has the effect of minimizing Type II errors, accepting hypotheses one should reject, while maximizing Type I errors, rejecting hypotheses one should accept. (Hands 1992: 61)

[12] We don't wish to suggest that rationality with respect to knowledge production implies optimality. Scarcity (of time, and of material and cognitive resources) requires a trading off of costs and benefits, and the rational agent will do the best he can. But bounded rationality applies with special force in science, because its object is, in part, to produce novelty – ideas not yet conceived – and therefore the benefits of research are quite uncertain *ex ante*. (Loasby 1989: 197. See also Leonard (1998). Even retrospectively, the value of scientific knowledge can be difficult to measure. (Dasgupta and David 1994: 490).

[13] "Empirical" is meant in the broad sense of Hacking (1983), i.e., as comprising observation, experiment, prediction and technological application.

[14] "Methods" is plural by design, a recognition that the ways of producing reliable knowledge are heterogeneous. Ian Hacking (1983: 152) says: 'There is not just one way to build a house or even to grow tomatoes. We should not expect something as motley as the growth of knowledge to be strapped to one methodology.'

[15] The term "spontaneous order" is due to Friedrich Hayek, who meant it to be distinguished from a consciously planned or designed order. Hayek often cited Adam Ferguson in this respect, who characterized spontaneously evolved orders as those that 'are of human action, but not of human design.' (Hayek 1973: 20).

[16] Adam Smith used the term "invisible hand" but twice in all his published work, most famously (and closest to the sense used here) in the *Wealth of Nations*: '[E]very individual . . . neither intends to promote the public interest, nor knows how much he is promoting it . . . . [H]e intends only his own gain and it, and he is, in this as in many other cases, led by an invisible hand to promote an end which was no part of his intention. Nor is it always the worse for society that it was no part of it. By pursuing his own interest he frequently promotes that of society more effectually than when he really intends to promote it.' (1937 [1776], p. 423).

[17] This would be akin to arguing that all markets meet the necessary conditions for the First Fundamental Welfare Theorem to hold. Invisible hand processes need not entail Pareto-optimality. They merely characterize situations where decentralized individual choices, unintendedly lead to good collective outcomes.

[18] There are important exceptions. See, for example, Ullmann-Margalit (1978) and Nozick (1997: 191-97).

[19] Or, at least, they believe that agents can be so improved by the right kind of social engineering, what Isaiah Berlin referred to as straightening the "crooked timber of humanity."

[20] The invisible-hand theorists *do* represent a profound change in political philosophy, a shift from virtue to justice. No longer is government's purpose to morally instruct – to attempt to create a virtuous citizenry – it is, rather, to underwrite and enforce rules that are the essential preconditions to successful commercial society. Note well, however, that the invisible hand theorists were, in some sense, themselves motivated by virtue. They believed that, in a commercial society, their more modern conception of government would better serve the common good. The foregoing paragraph and this note are indebted to Jerry Muller's (1993)

superb discussion.

[21] Traditional interventionist prescription takes different forms: the state does the research (directly produces the knowledge); the state subsidizes research (claiming ownership of the output), or the state creates and protects intellectual property rights, which grants private temporary ownership rights to the scientist (generally patents), allowing him to appropriate the returns. Thus the "old" economics of science has, paradoxically, provided an intellectual rationale for the current system of government control and planning of science. (Dasgupta and David 1994: 496-97).

[22] Paul Samuelson, in an AEA Presidential Address, offered this view of credit as a motive in economics: 'Not for us is the limelight and the applause. But that doesn't mean the game is not worth the candle or that we do not in the end win the game. In the long run, the economic scholar works for the only coin worth having – our own applause.' (1962: 18).

[23] By "published," we mean "made public," or "openly disclosed." Working papers are, in this sense, published. Publication in refereed journals entails peer review, another institution of science we take up below. One can find examples of credit awarded to scientists who were second in a race, or who never bothered to formally publish their results, but these tend to be exceptions. Independent discoveries, such as the invention of the calculus by both Newton and Liebniz, are a different matter. (See Merton 1973: 343-70).

[24] This, of course, is the norm in industrial science, where good ideas with technological applications are made public generally only with patent protection, so that the producer of the intellectual property receives direct financial returns (in the form of temporary monopoly profits) rather than indirect payment in the form of credit. Dasgupta and David 1987 distinguish science from technology on this very basis – science is, they argue, concerned with adding to the stock of scientific knowledge, and thus has the practice of open publication; technology, in contrast, is concerned with maximizing rents from a given stock of knowledge, and thus has a practice of secrecy. We are using "science" in the former sense. There are other incentive effects which we don't take up here. For example, priority's winner-take-all structure creates incentives for rapid innovation, but it also can result in wasteful *ex post* gamesmanship (Merton 1973: 317), and in socially inefficient expenditures analogous to the patent-race problem. (Daspgupta and David 1994).

[25] Replication is rare in economics, and econometric results that are revisited are notoriously hard to reproduce. An organized attempt at replication, funded by the U.S. National Science Foundation, reviewed empirical papers submitted to the *Journal of Money, Credit and Banking* from 1980 to 1982. (DeWald *et al.* 1986). Of 154 original authors notified, only in 90 cases were authors willing and able to supply data and programs. The replicators reviewed 54 of these data sets, and found that only eight were sufficiently free of problems to permit an *attempt* at replication. And, only in two of the remaining eight papers were the results actually reproduced in full. The review team was thus able to fully reproduce econometric results only 3.7 percent (2/54) of the time. And, one should note, the replicators were attempting replication only in the narrow sense of reproducing results, using original data sets and statistical procedures. See also Feigenbaum and Levy (1993).

[26] Arthur Diamond (1986) has estimated the present value of an additional publication (in 1994 dollars) to a 35-year-old academic mathematician as about $6,750. (Cited in Stephan 1996: 1203). The editor of a journal which publishes 50 articles a year could develop a remunerative sideline, particularly where the marginal differences among published and unpublished papers are small. Less egregious and more common forms of corruption are nepotism and logrolling.

[27] Collins (1993) guesstimates that 90 percent of published scientific papers are never cited. Cole reports that

in a sample of the 1,187 papers published in the 1963 volume of *Physical Review*, one-half received zero or one citations in the subsequent three years, and that only 15 percent received six or more citations. (Cole 1992: 16).

[28] We don't claim that the reliability of a given idea is instantly known, or that scientists cannot be stubborn in ignoring ideas that they would prefer not to accept. For the scientist, as for all agents, there is sometimes an unhappy difference between what one wants to be true and what one takes to be true. Scientists are, like all human beings, adept in ensuring that the latter comports with the former, and some will never change their minds, even in the face of overwhelming evidence to the contrary.

[29] Things get a bit more complicated when theories refer to things that are unobservable, like quarks. The realist will insist that entities one cannot "see" are nonetheless real. Says Hacking: 'protons, photons, fields of force, and black holes are as real as toe-nails, turbines, eddies in a stream, and volcanoes.' (1983:21).