The Review of Austrian Economics, 16:2/3, 133–152, 2003.

Order-Dependent Knowledge and the Economics of Science

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Abstract. Economists approaching the study of science typically assume the applicability of a market analogy, but then base their analysis on the presumption that science constitutes an area of pervasive market failure. Given the interactions that are actually observed to occur between scientists, we suspect that the failure is in the analogy, not in the putative market. In considering how one might better apply the economic way of thinking to the understanding of science as an activity, we suggest that it is necessary to specify exactly how scientific interaction *differs* from market interaction, and to be clear about how the behavior of interacting scientists might be modeled in terms of the general pursuit of self-interest in a noncatallactic context. Our model of science portrays an institutionalized mode of interaction between scientists involving the publication, use, and citation of scientific papers, and it is in the exploration of the individual incentives thrown up by this arrangement that the interesting empirical implications arise. We give a short exposition of the possible lines of investigation that could be followed based on this approach.

Key Words: economics of science, institutional analysis, order-dependent knowledge, non-market activity

JEL classification: A14, B52.

In applying economic analysis to the knowledge-generating activities of scientists, economists, naturally enough, have deployed concepts that have been honed in the analysis of markets. The early literature—flowing from Nelson (1959), Griliches (1960), and Arrow (1962)—portrays science as a market-like arrangement in which there is market failure due to a pervasive public goods problem, a failure only partially offset by the operation of the property-like convention of priority of publication. More recently, economists such as Dasgupta and David (1994) have begun to pay more attention to the particular institutional arrangements involved in science. Just as in the older literature, however, the discussion is in terms of the efficiency (relative, presumably, to some market-inspired ideal) of the arrangements for producing knowledge, and just as surely market failure is detected. The newer work deploys more sophisticated economic concepts—including incomplete and asymmetric information and network theory—but the broad conclusions and policy implications are similar.

We suggest that a different approach is in order, one that uses economic concepts to focus on the positive study of the knowledge-generating activities of science but eschews the use of "market efficiency" as a benchmark ideal. The "economic way of thinking", based on a focused attention to the incentives faced by generally self-interested agents interacting in a social situation and concentrating on how these incentives affect behavior at the margin, can be a powerful approach to understanding social structures of all kinds, and is not limited in its applicability to market structures. Its power does not necessarily require that we conceptualize all activity to which it is applied as a market of some sort, however flawed. To apply it to orders other than markets, however, requires being explicit about the particular sorts of (nonmarket) institutions through which agents interact in order to pin down the sorts of incentives that are at play. The economics of markets is a successful and still fruitful field because this marriage of the economic way of thinking and explicitness about relevant institutional arrangements has been carried through for market-based activity; and we think that the economics of science could be a similarly successful enterprise if that same economic way of thinking were combined with reasonably realistic explicitness about the relevant scientific institutions.

Orders: Science and Catallaxy

Science is a social activity in which knowledge about the world is produced, but it is not a *market* activity in the usual economic sense. In Hayekian terms, science is a spontaneous order, but it is not a catallaxy.[1] Our approach to the development of an economic theory of science is based on a Hayekian approach to social theory in which social orders are conceptualized as being built out of the activities of generally self-interested individuals who interact according to generally understood modes of interaction—routines—that we refer to as "institutions". Social orders are seen as structures in which this institutional interaction of agents produces knowledge—a perspective based on a view of orders as active, knowledge-generating structures that continuously classify phenomena in their environment.[2] Different orders, however, have different knowledge-generating capacities, as evidenced by the disparate outcomes generated by social systems functioning under different institutional arrangements and rules. This is because the mechanisms by which agents' plans get sorted out, integrated, and reconciled differ. The market order produces knowledge in the form of prices, quantities, and other characteristics of goods and it is in terms of these categories that catallactic theory is able to explain market adjustment processes. Science, on the other hand, produces "reliable and codifiable" knowledge about the world. The constellation of interactions that produce scientific knowledge are not describable as goods exchanges and so are not fully analyzable by catallactic theory.[3]

The problem, then, is to adequately characterize the nature of the interactions through which the activities of scientists produce scientific knowledge. Following Hull (1988), we focus on self-interest in terms of the desire for the accumulation of "reputation", and we postulate that the pursuit and acquisition of reputation is the central coordinating mechanism in interactions among scientists. We develop a simple institutional model to describe the functional role of reputation in producing scientific knowledge, and look for ways in which this model might carry testable implications. We suggest that, by focusing on the means by which scientific ideas are disseminated among scientists and evaluated by them, light could be shed on the factors that affect the content, stability, and evolution of scientific activity.

Social orders in general, and science in particular, are structures comprised of the connections of interacting individuals under certain kinds of conditions. The outcomes generated by these interactions may be intended, unintended, or combinations of the two. These orders exhibit characteristics (generally, collections of results identifiable as patterns) depending on the properties of the constituent elements, including the ways they interact, and other necessary contextual stipulations. In social theory informed by economics, these conditions typically refer to assumptions about agent rationality, specification of exchange procedures (e.g., auction vs. posted price, price vs. non-price; barter vs. monetary), and other rules (e.g., property rights, contracts) relevant to the framework within which individuals interact. Perhaps the most interesting such orders are those that are "spontaneous"—emergent structures that have no overall purposes, are "abstract", and are complexly organized.

Foremost among such spontaneous orders, at least in terms of their scientific study, is the market order or catallaxy. The domain of catallactics is the study of economic processes of exchange and production among individuals under scarcity as governed by rules for the exchange of private property.[4] Characteristic of this particular arrangement, and unique to it, is that the interactions of agents generate prices—and, with indirect exchange, money prices. The power and even elegance of this construct and its various manifestations from Smith onward has given economics considerable success in generating insights about the operation of a market economy. But economists have more recently sought to extend themselves into areas once previously isolated from economic analysis, including familial relationships, jurisprudence, and the political process.[5] In venues such as these, it is desirable and perhaps analytically necessary to specify individuals as acting "rationally", that is, at the most general level, acting purposively in formal accordance with their self-interest as each agent defines that. But a social theory resting on such a stage would rightly study an array of structures incorporating more than just the specific interactions encompassed by market theory and processes. While all social interactions may be studied on the basis of rational individual action, not all social orders manifest a catallactic process.[6]

This is not to suggest, however, that economic analysis in the sense of tools applied to the study of constrained choice is somehow unable to provide quite essential insights about human action under scarcity. And, indeed, this kind of "economic point of view" is usually integral to an understanding of such issues. But the common metaphor that science occurs in "the marketplace of ideas" is nonetheless unfortunate and doubly so. First, the metaphor presumes that scientific knowledge is an output, much like tractors, rather than an emergent attribute of the interactions going on within the order. Economic theory tells us how interactions between agents in a catallaxy, all having their own private knowledge, transform this knowledge into another kind—market prices. All spontaneous orders have the capacity to display emergent properties and this, too, applies to scientific activity and the creation of scientific knowledge. Scientific contributions are produced and consumed by agents, but scientific knowledge is generated as a byproduct of the interactions that attend this production and consumption. The characteristics of that knowledge are affected by the kinds of rules and institutional arrangements in place.[7]

Second, the metaphor presumes that the generation and transmission of scientific knowledge conforms to a catallactic process by which certain kinds of things we might call "science-goods" are exchanged between buyers and sellers. It is perhaps under this rubric that the metaphor seems to slip into presumptions that cannot be so easily reconciled to suit the particular object of study (scientific activity) with the preconditions required by economic theory. In a catallaxy economic activity occurs through market interactions where suitably defined property rights claims are routinely exchanged for other things, including money. In science, the production and distribution of knowledge occurs largely through nonmarket arrangements in which property rights in the conventional sense are applied to only particular kinds of interactions (such as copyright agreements and patents), and which are often very ill-defined or are simply assumed in principle not to apply at all. Even for those situations in which property rights could be specified and applied, standard public goods theory is quick to suggest that the "outputs" of science (i.e., scientific knowledge) are largely nonrivalrous and non-excludable in consumption, hence suggesting that any such outputs would have zero exchange value.[8] We think, however, that the conventional "public goods approach" to science falters in not considering the implications of alternative institutional arrangements. Although public goods are ordinarily defined in terms of certain characteristics that establish nonexcludability and nonrivalry in use, these criteria also depend on the existing institutional framework, including the ways property rights are defined and applied.[9]

In markets, as opposed to nonmarket activity, the coordinating arbitrage function of entrepreneurs relies on the emergence of a system of prices. The ensuing adjustment in prices, quantities, and characteristics of vendible things brings individual plans into closer consistency.[10] Such "market knowledge" is both used and generated anew through the interaction of individuals through market institutions. When property rights are well defined and enforced (and certain other conditions satisfied that allow the market process to operate reasonably smoothly), we have good reason to think that the resulting arrangement tends toward one where resources will gravitate toward their most highly valued uses. This should be viewed as a byproduct of the overall operation of the market order, an order that, as Hayek reminds us, could not have been designed by anyone but emerges as an undesigned outcome of the catallaxy. Importantly, the increase in wealth that this arrangement may be expected to generate requires only a minimal set of constraints on individual autonomy, thus corresponding to a high degree of personal liberty.

None of this is intended to deny the obvious fact that science is always associated with market activity of some sort. University science is done by academics who supply teaching services (and sometimes "expert" advice) as a supporting adjunct to their scientific activity, and the basic science performed in business firms and laboratories is supported, ultimately, by sale of product. Government involvement in science, either directly in state-run organizations or indirectly via government support of university research, is ultimately funded by taxation of market activity. Even scientists who work independently support themselves out of savings from market income. However, the necessity for underlying market support does not imply that the enterprise of generating scientific knowledge is itself a market activity. The market is necessary to support the existence of the participants, but the character of their interactions *as scientists* is another thing altogether. In this paper, we see the scientific order as one that in essential ways functions as a noncatallactic order under existing institutional arrangements.

Orders and Knowledge

It is important to distinguish scientific knowledge from other kinds of knowledge, such as prices, that social orders produce. As used here, we shall refer to scientific knowledge, perhaps somewhat arbitrarily, as knowledge that is "reliable" and "codifiable".[11] Thus, characterizing scientific knowledge as *reliable* is only meant to suggest that some set of procedures have been employed that serve as filters for discriminating among rival claims. This neither implies nor requires that any knowledge claim that has survived this process is necessarily reliable in all applications. What matters, instead, are the ways that some kind of critical process, however imperfectly, has assessed competing claims, promoting some and rejecting others.

Scientific knowledge is also *codifiable*, i.e., aside from whatever other characteristics it may have, such as "coherence", it is also explicit and transferable. In suggesting that scientific knowledge is explicit, we highlight the distinction between private or tacitly held knowledge and knowledge that has been articulated verbally or in writing. This is not meant to suggest that there is something suspect about private knowledge or market knowledge (in the sense of prices, etc.), but rather that this vast reservoir of knowledge exists in a form which has not required it to be examined, tested, criticized, and assessed by others. Scientific knowledge attains that mantle by having, of necessity, exposed itself in a public forum where it can be compared to other knowledge-claims and evaluated by other scientists.[12] To be regarded as scientific, knowledge must become "objectified" because without that, feedback loops for correcting scientific mistakes and error could not operate. Thus, the procedures employed by scientists in transforming ideas into scientific knowledge have the effect of weeding out relatively "blacker claims" in favor of better ones. As a consequence of this process, scientific knowledge can be collected and put into a form that may be accessed by others, both in the present and in the future. Because scientific knowledge is codifiable, the mechanisms that work to establish its reliability also render it into a form that makes it transferable. In this way, each scientist has the luxury of being able to draw on an immense reservoir of cumulative scientific knowledge. Indeed, scientists do not (and actually could not) undertake their research *de novo*, but must always take a certain amount of knowledge as (tentatively) established or corroborated by other scientists.

The mechanisms involved in the production and transmission of scientific knowledge are, as we have stressed, different from those that explain the functioning of the catallaxy. Due to the alleged nonrivalrous and nonexcludable aspects of scientific knowledge, we cannot expect these mechanisms to operate in ways that explain the coordination of plans in the market economy. And so it should not be surprising that other kinds of mechanisms have emerged in science to coordinate scientific activities and their outcomes. We identify "reputation" as an important mechanism in understanding how science, as a non-catallactic order, works. Although a large literature exists that studies "reputation" as an element of *market* interactions,[13] our interest is to examine the special role of reputation as a mechanism in the production and distribution of scientific knowledge.

Reputation serves in our treatment of science as a mechanism analogous to "profits" or "utility satisfaction" in the market setting. We think it sensible to assume that scientists value reputation and rationally engage in activities that generate reputation. Reputation is an indicator of individual success in science and will sometimes create opportunities for scientists to convert reputation into monetary rewards. But in the pursuit of reputation, scientists will also engage in activities that create scientific knowledge. It is not necessary here to assume that the search for knowledge is the essential factor motivating scientists, although that may, in fact, be important in terms of the personal values of individual scientists. Instead, we see the generation of scientific knowledge as a byproduct of scientific activity whose efficacy cannot be isolated from the institutional arrangements and mechanisms at work in science. Further, it is not necessary to attempt to measure levels of reputation, for it is sufficient for our purposes here simply to identify potentially measurable events (such as citations) that we assume to have the effect of increasing or decreasing reputation.

Our distinction between catallactic and scientific orders is both summarized and put into sharper focus in the following table:

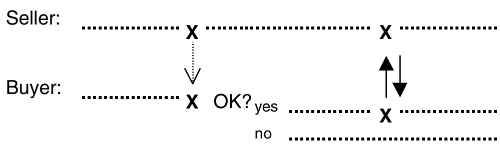
	Catallaxy	Science
Agents	Purposive; self-interested profit or utility seekers	Purposive; self-interested reputation seekers
Interaction and coordination mechanism	Entrepreneurial production and market exchange	Publication with citation (via journals, books, conferences)
Agent knowledge	Dispersed; includes private preferences, expectations, skills, and tacit knowledge as well as communicable personal knowledge	Dispersed; includes private learning and observational and analytical skills as well as personally generated scientific hypotheses
Knowledge byproduct (order-generated)	Market knowledge, i.e., classification of goods by prices, quantities, etc.	Reliable and codifiable (scientific) knowledge, i.e., classification of observable phenomena by scientific theories

Although it may seem natural (at least to academics) to distinguish sharply between academic and industrial science,[14] our approach would suggest that a more subtle distinction is needed. We model science in terms of an institutional arrangement involving publication and citation, and this does not depend on the nature of the organization for which the individual scientist works. If researchers in industry or the military publish publicly and can be cited, then they are engaged in science; if their work is not disseminated outside of their organization, then our analysis does not apply to them. Of course, the sources of income in academia and industry are different—academics are typically paid for teaching services whereas industrial scientists are paid a salary to work on projects of interest to their employer. But in both cases the scientist's accumulated reputation is typically a large factor in the level of income received.

Another pertinent distinction is between different fields of intellectual activity that involve a publication and citation mechanism. Different fields exhibit different standards in assessing a publication's positive "usefulness". In the natural sciences, the emphasis on empirical reliability distinguishes the knowledge produced there (which we characterize as "reliable and codifiable") from knowledge produced in philosophy or literary analysis. While our model could, in principle, be applied to all such fields, attention would certainly have to be paid to the nature of the norms of assessment and their effects on the incentives facing the individuals involved.

Modeling Interactions in Science

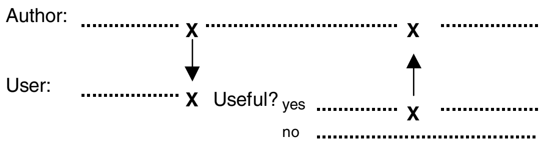
A convenient way of modeling the basic structure of social orders is to represent the relevant behavioral routines as parameterized communicating processes (ignoring, for the time being, the tendency for the routines themselves to evolve structurally as well as parametrically). The structure of the interaction can be illustrated schematically as in the following diagram, which shows the sort of communication between "seller" and "buyer" processes that takes place in a "customer market" (like a retail store):



The horizontal dotted lines represent possible execution paths; time flows from left to right. The Xs signify points of communication between the two processes. The dotted arrow represents a signal (the displayed price) from the seller to potential buyers which must be recognized and accepted before the interaction can proceed. The regular arrows denote transfers of goods, and in this case transfers take place between both parties, representing a spot exchange. Various parameters important to the consummation of the interaction, such as the buyer's reservation price, his ability to judge quality, and the seller's payment requirements, are not shown.

This is a model of one of the basic interaction patterns of a market order only, certainly not of the whole order. The market order is built up of an ongoing complex of multiple similar interactions involving many layers of capital and consumption goods and services. Emergent characteristics of the overall order can be investigated first by considering the various limitations the basic structure places on possible outcomes, and more comprehensively by simulation of the concurrent executions of the routines of a population of individuals.[15]

The basic interactions involved in any social order should be able to be specified and modeled in a similar way—one has only to be specific about the procedures followed and the nature of their communication. In the case of science, we propose that the significant interactions occur between an "author", who unilaterally communicates a "publication" containing "ideas" (not specified further) that others my find useful, and a "user" of the ideas. The (property-like) convention is that the user, if he finds the idea useful to his own work, "cites" the author. The structure of the interaction can be illustrated schematically as follows:



The author is motivated to publish by the potential reception of positive citations. This motivation could be driven by different factors for different authors.[16] For an author who is curious, the citations would be feedback as to the success of his curiosity. For the author who is career-oriented, the citations could lead to career enhancements. For the author desiring status, the citations build his reputation. But there is no more need here to delve into the details of the motivation than there is, in the economics of markets, to dissect "utility" or "profit", and so, for want of a better word, we characterize the pursuit of happiness in science as a pursuit of "reputation". We realize, of course, that the citation is not the only form of scientific feedback. There are prizes awarded at various levels, and there is that most valuable of rewards, eponymy. But the citation is still the major form of reputation-building response involving currently interacting scientists.

Note that there is no "exchange" here in the catallactic sense. No relevant exchange of property claims occurs either when a paper is published or cited. The author's physical publication[17] is a signal inviting response, which may or may not be recognized and acted on by other scientists. The interaction will only proceed to completion—the individual plans will mesh—only if the signal is received and the receiver, in a unilateral judgment, finds its contents acceptable. And in science, "acceptable" means useful in furthering one's own work. If actual use is made, the institution calls for the acknowledgement of credit in the form of a citation. The only property-like aspect of the arrangement is the convention that citation follows use, but citation is not a numeric measure and is not nearly as sensitive an indicator of value as are the money prices of catallactic exchange.

The parameters that characterize these routines need to be identified and described. Authors' publications will vary in cleverness (in the use of generally agreed concepts) and originality (in the espousal of radical concepts). Users will vary in their sensitivity to and tolerance of originality. Uses will vary, and so citations will vary in reputation-building effect. There will not only be use-citations, but a variety of forms, including negative ones. There will be impediments to the recognition of publication quality and potential usefulness, and there will be at least some basic criteria for a work even to be considered for possible use. In an environment in which most authors are acting as both contributors and users, there will be competition for access to better publication vehicles and also more direct confrontation involving both positive and negative citation. The exploration of these themes raises many questions which have empirical content, as we shall show.

We are not the first to note the fundamental importance of the publication and citation mechanism for the development of scientific knowledge. Most recently, and most explicitly, Hull (1988:306), in a detailed study of the practices of two groups of biologists, concludes that "although the desire for credit may not be as admirable as seeking knowledge for its own sake, it is nevertheless a powerful spur to action". And further (p. 309):

> "... the sort of credit that really matters is *use*. Individual scientists want credit for their contributions, as much credit as possible. Scientists ... readily acknowledge that they crave for recognition, not the recognition of the public at large or even of that amorphous hodgepodge termed the scientific community, but of the few scientists working in their area whom they genuinely, if sometimes grudgingly, respect."

In a 1988 essay in which he reflected on a lifetime of work in the sociology of science, Merton (1996:333) comes to a similar conclusion:

> "... the institutionalized practice of citation and references in the sphere of learning is not a trivial matter. While many a general reader ... may regard the lowly footnote or the remote endnote or the bibliographic parenthesis as a dispensable nuisance, it can be argued that these are in truth central to the incentive system and an underlying sense of distributive justice that do much to energize the advancement of knowledge."

It is also interesting in this context that, in one of his earliest studies (first published in 1938), Merton (1973:191–203) notes the coincidence of the rise of modern science in England during the second half of the 17th century and the formalized communication between scientists which took shape upon the founding of the Royal Society and the inauguration of its journal, the *Philosophical Transactions*. Prior to that, scientists such as Bacon had advocated that science be an endeavor pursued for the common good of society and not for personal credit, and indeed the French Academy was founded (at about the same time as the Royal Society) on just those principles. By the turn of the century, however, the French Academy had changed its rules so that members would receive explicit and individualized credit for scientific contributions, very much along the lines adopted by the Royal Society. The specific mechanism introduced by Henry Oldenburg, the secretary of the Royal Society, was access to publication for scientific papers, enabling the author to receive personal credit and identification of priority and thus eliminating a major reason why authors might withhold access to their work.[18] To this function was soon added the feature of referee-based certification, as Oldenburg turned to other society members for advice on acceptance of papers when the material was outside his competence to evaluate. Merton's statistics on scientific activity in England in the 1600s show that, as he put it (1973:191), "an unusually large number of seventeenth-century Englishmen turned to [work in original science and technology] during the fifth and sixth decades of the century", and that (p. 193) "there are three times as many discoveries in the second half [of the century] than in the first". Without attempting to claim unidirectional causation, it seems evident that the particular institutional arrangements for science that emerged during this time (and which have remained to this day in similar form) were of fundamental importance to the astonishing development of science as an intensely social activity. Science as we know it appears to be an interesting case of a spontaneous order brought about by the very intentional adoption of certain institutional practices (geared to solving particular, local problems) that turned out to be highly conducive to order formation.

It is, without doubt, a simplification to represent science purely in terms of the pursuit of reputation. But it is an approach analogous to the one economists make in representing firms as profit-seekers. Anyone who has ever been in business knows that the actual motivations of businessmen are much more complex than that, but it is, nonetheless, a suitable abstraction because, whether or not profit is the specific aim, the only firms that survive are those who do in fact, sooner or later, make profits. Similarly with scientists—the only ones who survive *as scientists* are those who, sooner or later, earn some reputation.[19]

Empirical Implications

Putting the model through its paces in order to show its usefulness in understanding the domain of science involves grafting some flesh onto the bare bones of the publication-use-citation interaction in the different contexts in which it can appear. The basic model provides an underlying structure within which a wide range of situations can be described, but in looking for observable implications we focus particularly on the sorts of incentives embodied in the mechanism that allow pattern prediction of likely trends. Several different lines of investigation immediately suggest themselves:

- *Issues related to the author's signal*. The signal that initiates coordination in this scheme is the act of publication. To be successful, the publication has to be noticed, since it is an unsolicited signal. And it has to be noticed by people likely to make use of it and therefore to cite it favorably. Many details of the structure of the publication process can be shown to be geared to enhancing the signaling effectiveness in relation to the characteristics of the audience. Most obviously, growth in the numbers of scientific publications being generated leads to a strong tendency toward journal specialization, as journals cannot continue to command the attention of the whole of an audience increasingly diverse with respect to their scientific interests. Division of journals is probably as fundamental in science as is the division of labor in market-oriented production. Similar factors impel journals to become increasingly selective with respect to the publication of articles received, and to provide an explicit certification function by virtue of such selectivity. These topics have received some attention in the sociological literature, especially the phenomenon of prestige hierarchies in journals—see, for example, Cole and Cole (1973) and Whitley (1984). But we think that the unifying idea of the need for signal strength could usefully integrate this area of research, and, especially if it were to be connected with observations as to the accompanying changes in the characteristics of the knowledge generated.

  A significant factor in determining signal strength has been observed to be the prior reputation of the author. As Merton (1973:443) puts it: "...eminent scientists get disproportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions". He has named the phenomenon "the Matthew effect", alluding to a famous biblical passage. This flair for description and naming probably overemphasizes the effect, however—a study that puts the issue more in perspective is that of Johnson (1997), who finds that prior reputation (measured as the number of citations per article in the past five years—self-citations not counted) has a "significant but small" effect in generating citations to later articles. Nonetheless, the model developed here would predict a positive effect of prior reputation at the margin simply on the assumption that author reputation is one selection factor considered by potential users of published work.

Considerations of signal strength also provide one basis for an explanation of the frequently observed tendencies toward fragmentation of scientific disciplines. Within a scientific group—a number of otherwise separate researchers all grappling with a particular problem or working within a common framework—the signals from researchers are likely to be stronger or more germane to those within the group than those outside it. Those outside the group may lack the interest, specialized knowledge, or simply the time to maintain close interactions with members of the group. Citations of group members may be dominated by other members of the group. The formation of such "research groups" provides important focus, expertise, and feedback effects that justify high degrees of specialization often necessary for the growth of knowledge. Groups of this kind constitute spheres or "islands" of interaction within science that promote chopping up questions into more manageable pieces. The relation between such groups and the wider scientific community may be entirely compatible and synergistic. But not all scientific groups perceive themselves in this way. Instead, the rationale of the group may reflect discord along any number of different fault lines. In economics, these may involve serious philosophical, methodological, analytical, or policy differences. Because the production of knowledge will in general have less currency outside the group, incentives exist for groups, especially when they coalesce around a "school of thought", to build their own instruments to increase the value of their contributions, institutionalize their commonality, and expand their influence (mainly through "recruitment"). These may take on more formal characteristics—new journals and conferences, for instance—once the group reaches a certain size or attains a distinctive identity. Such arrangements further "within group" citation, which can help foster the emergence of "stars" who would presumably have been less successful if they were competing for citations outside their group.

However, whereas the value of reputation is directly related to the size (or prominence) of a scientific group, a premium exists for those who can transcend their "home group" by appealing to larger and better established groups. This increases flows of knowledge among groups, modifying (perhaps only marginally) each group's own identity and providing the basis for further cross-group exchanges. Whether such tendencies in fact exist depends on several factors, including the initial degree of separation and the importance to each group of remaining true to itself on whatever "foundational" issues it deems essential. The strength of forces pushing toward group "mergers" and cross-fertilization must be compared to those forces promoting isolation. In economics, casual observation suggests that the reputational gains from merging may be substantial for the great majority of researchers. Nonetheless, economics demonstrates the capacity to support a fairly large number of subgroups, despite their relative inaccessibility to the high profile positions at important research universities and publication outlets. According to the analysis presented here, the survivability of such groups cannot be disassociated from the reputation enhancing opportunities participation in the group makes available to researchers and which would have otherwise been unavailable without the group. If we assume that the likelihood of error-correction increases with exposure before a wider audience, researchers whose work remains largely within the confines of their own group will benefit from that in-group engagement; but the value of their signals to the wider scientific community is not likely to be as high. In any case, our underlying model provides a vehicle for connecting various lines of inquiry in this area.

Finally, consideration of the signal strength effect could provide some useful insights about the possibilities for the development of “internet journals”. Attempts to replace the existing journal structure with more informal internet postings are not likely to succeed unless some method is found to maintain the signal strengths of individual publications. Although it is probably hazardous to speculate about exactly how this might be achieved from the safe vantage point of an armchair, one fairly obvious signal-enhancing feature that an internet journal could supply would be a much more sophisticated indexing feature so that potential users could conduct rather specific searches to find articles of interest.

- *Issues relating to competition between authors*. Scientists, whether individually or as members of a research team, seek to persuade others. Theories, explanations, and facts do not present themselves; rather, they form aspects of an argumentative context in which scientists compete to persuade other scientists, funding sources, and even the general public to see reality in a particular way. But the inherently rivalrous character of scientific activity is nothing other than the flip side of reputation. Success in one would be expected to be associated with success in the other through bidirectional feedback loops.

Two ways (among others) that competition is played out in science is through specialization and access to publication outlets, especially journals. Authors build reputation through the quality, frequency, and impact of publications and citation. In so far as specialization allows researchers to pursue their own interests or take advantage of specialized knowledge or skills, this will enhance the signal strength of publications. At the same time, however, specialized research is exclusionary in terms of the audience to which it appeals and, consequently, would tend to exert a diminished reputational impact beyond the immediate research community. But some of these costs to specialization can be reduced by adding some institutional complexity, such as the increasingly familiar “review essay”. Such innovations provide reputational benefits to both those specialists cited and to the authors of review essays. But nonspecialists may also benefit because knowledge is made available and put into a form that may be relevant for their own research. Menger’s (1871:217) insight about how “need for competition itself calls forth competition” is pertinent here, although the coordinating mechanism is not economic profit but reputation.

The payoff from specialization requires, however, the existence of publication outlets. Researchers, in effect, must compete for the available publication slots. If we assume that specialization is necessary for the growth of knowledge, an important part of that process will refer to the arrangements governing the number of journals (and presses) and their reputation-enhancing characteristics.[20] The rate of journal space accretion is constrained by significant start-up and operating costs. New journals also require an existing and identifiable body of scholarship to justify their appearance at all. Together, these considerations suggest that the introduction of new journals would be expected to lag new research specialties.[21] If so, this may retard the ability of new specialties to insinuate themselves into the institutionalized structure of science, at least without first demonstrating their value to the wider scientific community. Until then, such constraints provide gatekeeper functions that increase the reputational value of publication, given the available journal space. While all researchers want outlets for their work, they also must surely realize that their research has value in part because journal space precludes that option for others. This may account for attempts to replace the existing journal structure with more informal internet postings. However, as we have noted, these are not likely to succeed unless some method is found to maintain the signal strengths of individual publications, lest the arrangement slides into a case study of Gresham's Law.

- *Issues relating to citation measurement*. The recordings of citations and citation counts could be a source of empirical data, and citation studies have a long history in the sociological literature, dating at least from Garfield (1955) and Price (1963). Economists have also invoked citation counts as determinants of journal and school rankings as well as researcher salaries—see Liebowitz and Palmer (1984) and Diamond (1986), for example.[22] Studies by Quandt (1976) and Stigler and Friedland (1979) include rankings of individual economists based on citation counts. Biddle (1996) has used citation data to probe the question of what particular aspects Wesley Mitchell's work were most effective in establishing his reputation.

  Several workers in this area have commented that the practice of simply counting all citations as equivalent is unsatisfactory. Stigler and Friedland (1979:203) disaggregate their citation counts into "favorable", "unfavorable" and "neutral", and this is certainly consistent with the desire to relate citation quantities to reputation. But if we wish to be able to use citation data in analysis of contributions to scientific knowledge, then further disaggregation seems to be called for. There are positive "use cites", positive "review cites", positive "recognition cites", zero-effect "self cites", negative "argument cites", and negative "criticism cites". For building a reputation (and for contributing positively to current scientific knowledge), use cites are the most important (but probably with an effect that diminishes with quantity); for reducing a reputation, criticism cites are effective, and probably have a unit effect greater in absolute magnitude than use cites and increasing with quantity.

  In short, for citation data to be useful in providing tests for our institutional model, a simple compilation of data published in citation indices will not be adequate. For example, an interesting study might be to pick a pair of scientists with comparable total numbers of citations but significantly different standing in the scientific community (an informal assessment of reputation). The prediction would be that disaggregation of the cites would show either fewer use cites or more criticism cites for the lower-reputation person.

  Suitably disaggregated citation data could also be used in investigating trends in standards induced by external "nonscientific" influences. We have already noted that science, like government, is dependent for its survival as an order on the productive capabilities of an underlying market order. The methods by which scientists are supported—by employment for teaching purposes, provision of "expert opinion", and marketable product development, or by donation or government grant—will obviously have an effect on the productivity and direction of research. Competition for funding of various sorts or the emergence of new funding possibilities can also lead to "professionalization" of a discipline which should show up as a narrowing of the criteria for successful publication and a trend toward cumulative-style use-citation. Accompanying this might be fragmentation of the discipline as the "losers" in this competition dissociate from the "mainstream" winners. Comparisons might be made between citation spectra of sixteenth-century natural philosophy and modern physics, or between economics before and after the Second World War.

It is clear that there are practical difficulties here—assessing citations article by article is (we can report form experience) extremely tedious work. And there is also the issue of the appropriate weights to be assigned. This may be less of a problem than it seems at first blush, given the fact that we are largely interested in comparisons and trends in citation counts, not absolute values. But a more vexing issue is whether a criticism citation is actually negative in reputational terms. This may vary with the field. It could be the case (at least in some fields) that any recognition is better than none, but it may also be necessary to disaggregate negative citations into those associated with a demonstration of error and those associated with respectful disagreement.

- *Issues relating to the knowledge-generating effect.* The generation of scientific knowledge from this reputation-driven arrangement depends on reputation being primarily enhanced by useful contributions of scientific ideas—useful, that is, in the sense of actually being used by others (often competitors) in their own work. As we have noted above, this criterion conditions the characteristic attributes of the knowledge produced. Further, the proportion of use cites to total cites should be a measure that distinguished different scientific fields and the knowledge they produce with respect to the tend toward cumulativeness of that knowledge.

  Perhaps due to the nature of the subject matter, but more likely due the state of its development and the nature of the equipment or techniques considered necessary for its investigation, different scientific fields differ in their criteria for contribution recognition and therefore, we predict, in their ratio of use-citations to the other positive citation categories. And a field in which there is specific (and expensive) equipment involved or one in which very specific methods (often mathematical) have been developed would tend to have very specific recognition criteria for "normal science" publications, a relatively high proportion of use-citations, and a slowly cumulative type of knowledge-generation. Kuhnian paradigm shifts would be possible, but rare. Fields in which these criteria are not met would probably tend to have relatively more review, recognition, and argument-type citations and less cumulativeness in knowledge generation.[23]

  The hypothesis that direct *use* of a publication's scientific contribution is necessary for positive citations to be forthcoming needs to be amended for the case of review articles. Reviews, which can be regarded as intermediaries between publishers of scientific contributions and users of them, are a prominent feature of the scientific publication scene, increasingly so as the volume of published work in a field outstrips the ability of those in even closely related fields to stay abreast of it. Such articles in themselves are not direct contributions to scientific knowledge, yet scientists go to considerable trouble to write and publish them. Our model would predict that this would only be the case if the citation (and therefore reputational) returns from review publication were sufficient to bring forth the review effort, and this indeed seems to be the case. Not surprisingly, Laband (1990) has found that review articles are high on the list of cited articles.

- *Issues affecting the stability and evolution of the order*. When we look at science as a spontaneous order, it is necessary to analyze it in terms of the interactions from which it emerges and to avoid importing purposes to it apart from the separate aims of its constituent elements. Neither catallactic nor scientific orders have goals. Science, then, cannot be assumed to fulfill some larger purpose or, in particular, to pursue knowledge (notwithstanding the happy side-effect that the drive for reputation generates novel byproducts in the form of reliable and codifiable knowledge). Instead, we look to reputation as the relevant aim of individual scientists and thus infer that the stability of the order matters because the random awarding of reputation would unhitch the preferences of scientists from outcomes. For the order to function smoothly, procedures and incentives must come into play consistent with achieving an alignment between individuals' expectations and the outcomes it generates. This will likely involve arrangements to credential and monitor scientists by screening, certifying, and assessing their research, and to administer prevailing standards, such as citation conventions and intellectual honesty precepts, within the order.[24]

  But self-organizing and regulating arrangements within scientific communities may also extend beyond those boundaries to partly intersect with the market order. Such interactions are unavoidable because the pricing system of the catallaxy provides science with inputs it could otherwise not produce on its own. Supporting the organization of science, from labs to scientists, requires the productive capabilities of an underlying market order. The delivery of this support, whether by university donors or government grant, will have an effect on the productivity and direction of research.[25] The stability provided by the market to science exerts its fullest impact in areas of intersection and it is here that the (catallactic) value of scientists and their output can be most easily established. Stability would be expected to increase as the scope of these intersections broadened.

  The evolution of the scientific order will reflect changes in the interactions among scientists. Such changes will arise endogenously within science and exogenously from changes in the broader social framework, including the market order, legal system, and the public sector, within which science is nested. The (evolutionary) mechanisms at play here are obviously complex. While analyzing these critically important issues would take us far beyond the scope of the discussion in this paper, our emphasis on reputation may provide some leads here. The emergence of an increasingly complex scientific order, such as we observe in modern times, requires the extension and application of reputational mechanisms consistent with maintaining the viability of the overall order. Because reputation is associated with the generation of knowledge, ascertaining the value of that knowledge will figure prominently in the determination of reputation. This, of course, is often difficult in science because scientific knowledge may have no obvious value beyond that assigned to it by research communities and because it may itself be a byproduct of altogether different processes, such as technological advancement, as suggested by Kealey (1996) and Nightingale (1998). The ability of science to expand and deepen its reach will depend on its success in attracting resources, which in turn will be affected by demonstrating the value of its endeavors. In this regard, the extension and application of property rights, wherever possible, to science and its organizations will promote the marketization and commercialization of scientific activity and allow scientists to capture more readily the benefits of their endeavors beyond the accumulation of reputation.

The foregoing listing and discussion of research possibilities intentionally raises more questions than it provides answers. We envision a program of research built around the concept of science as a particular instance of a spontaneous social order, and centered on the identification of self-interest within the order in terms of the earning of reputation. The institutional model portrays a rather simple mode of interaction between scientists involving the publication, use, and citation of scientific papers, but it is in the exploration of the individual incentives thrown up by this arrangement that the interesting empirical implications arise. We have given a rather brief exposition of the possible lines of investigation that could be followed—but it is a detailed enough one, we think, to show that this line of research has definite promise.

Conclusion

We have proposed an institutional structure to describe scientific interactions and have proceeded to investigate how that structure, in conjunction with generally self-interested behavior on the part of the participants, constrains and directs the outcomes and trends that emerge from such interaction. We show that we have the basis for understanding, as well as pattern prediction, of a number of aspects of the scientific enterprise that might heretofore have seemed rather unconnected and requiring *ad hoc* explanations. The development of journals, their certifying function, the trend toward journal specialization, the differentiation of journals with respect to selectivity, the effects of different sorts of citation in enhancing and diminishing reputation, the way in which citation responses tend to correlate with the author's previous reputation, the trend toward fragmentation in some sciences, and the emergence of the review article are all quite understandable and predictable phenomena in the context of our institutional model.

Of particular interest is what the model implies about the type of knowledge generated as a byproduct of the sorts of interactions proposed. The structure constrains the knowledge produced to be useful to other scientists in their own pursuit of reputation, and therefore to be reliable and codifiable. What is considered useful will depend to a large extent on the current state of the particular standards and techniques, including the experimental equipment, that characterize the field. The different acceptability criteria of different scientific fields will have a decisive effect on the sort of knowledge generated and, in particular, the "cumulativeness" of that knowledge. And finally, the model provides a basis for evaluating potential outcomes from environmental and institutional changes, such as the attempts to establish internet-based journals and changes in funding possibilities.

The approach to the economics of science we propose views science as a social order, a Hayekian cosmos, one that has emerged out of the self-interested actions of people attempting to pursue happiness in identifiable ways. To understand that order and the sorts of knowledge it generates as a byproduct, we model the interactions between the participants as institutionalized behavior patterns and investigate the incentives facing the participants in such a context. We view this work as directed toward extending Hayek's fundamental insights into the nature of order and knowledge, and fully expect that the general methodological thrust it exemplifies can also be applied productively to other modes of social interaction in addition to markets and science.

Acknowledgments

Earlier versions of this paper were presented at the April 2000 AHTEA Conference in Paris, the NYU Austrian Economics Colloquium, and the Southern Economics Association Conference, and we thank the participants for useful feedback. We thank Mario Rizzo, Roger Koppl, Israel Kirzner, and Michele Quere for helpful comments and suggestions, and we are appreciative of the thoughtful criticisms of David Harper, David Levy, and the referees.

Notes

1. See Polanyi (1962). This is not to suggest that science does not have elements that fall within Hayek's (1973, ch. 2) conception of a taxis. Our definition of "catallaxy" here follows Mises (1966:232–234) as "a system in which there are money prices and economic calculation". Note Mises is referring to *actual* prices, not shadow ones.
2. See Butos & McQuade (2002) and Hayek (1952a).
3. The catallactic approach has merit in the area Wible (1998) calls "secondary science" (the interaction of science with the market economy), as opposed to "primary science" (the nonmarket activities in which scientific knowledge is generated). Standard work in the economics of science sees no difficulty, however, in applying itself to both realms—see Stephan (1996).
4. See Mises (1966), Hayek (1973), and Buchanan (1979).
5. See, for example, Becker (1981).
6. See Kirzner (1999).
7. This is not to infer that such knowledge exists as a "social stock of knowledge" disembodied from those same agents. Our position is meant to reflect methodological individualism, as elaborated, e.g., by Hayek (1952b). Whatever commonalities our approach has with the "sociology of knowledge", those commonalities are quite incidental to our objectives, which center on understanding the scientific order.
8. The public goods argument also suggests that the economic value of such goods will mainly derive from their not being made available to the market at all—another twist on the standard claim that public goods will be underproduced. Because it is more difficult to capture the full market value of public goods, producers could extract value by internalizing the benefits.
9. For example, a water cooler in one's office is a private good, but it can be *made* into a public good if a rule requires it to be placed in the lobby where its use cannot be restricted. The recent emergence of publicly traded bio-tech firms highlights the role of property rights in promoting the private goods character of certain endeavors in science that formerly were assumed to necessarily involve the production of public goods. That these productive activities are sensitive to property rights is clearly seen in the free-fall in share prices of bio-tech firms when President Clinton and Prime Minister Blair intimated that these firms should make their "output" freely available to all, a suggestion, in effect, to externalize an internality.
10. See Mises (1966) and Kirzner (1973, 1979, 1999) on *catallactic* entrepreneurial activity. It is also possible to identify other aspects of entrepreneurship that highlight its non-equilibrating economic functions along the lines described by Schumpeter and those activities, such as associated with "political entrepreneurs", which affect the institutional framework. The concept of "market equilibrium" is, moreover, a distinctively catallactic concept.
11. In particular, we wish to avoid in this paper epistemological or methodological subtleties (and pitfalls) concerning what sorts of knowledge-claims should count as genuine knowledge. For the purposes of this paper, what

people take to be true and useful (i.e., "reliable") and codifiable is sufficient for describing what knowledge is. But more detailed considerations of this kind would take us far beyond the desired scope of the paper. Bartley (1990:147) criticizes the "sociology of knowledge" as being "interested in the acceptance, not in the *content*, of ideas" and would probably criticize our definition of scientific knowledge on similar grounds. Although our emphasis of "reliable" might speak to the content of ideas, our reason for using it has little to do with Bartley's point and everything to do with highlighting the role of various filtering (or coordinating) mechanisms for knowledge we believe to be important in science. It is not necessary for our purposes to advance a particular view of what science "actually is" or how it should be conducted. Even if scientific activity were defined by its practitioners in terms of a set of exogenous methodological rules or to their commitment to seek truth, we see it as being more fruitful to simply take the stance of observers of a particular Hayekian order, the salient feature of which involves reputation-seeking agents who produce reliable and codifiable knowledge as a side effect of their interactions. And we are not suggesting that what counts as science or how it is practiced is uniform across different domains of inquiry (or even within particular domains).

12. Also, see Ziman (1978). We wish, however, to distance our discussion from Ziman's claim that "*the goal of science is a consensus of rational opinion over the widest possible field*" (p. 3, his italics). We do not attach any goal to the scientific order apart from that attributed to individuals.
13. See, for example, Klein (1997).
14. See Dasgupta and David (1994). As we use the term, scientific activity would, as an empirical matter, principally refer to so-called "academic science." The property rights established by patents are that part of the activity of the firm or institution employing the scientists which is directed at securing marketable factors of production, i.e., is located in what Wible (1998) calls "secondary science".
15. See McQuade (2000) for a detailed description of such computer simulations.
16. See Tullock (1966) for a discussion of possible bases for scientific motivation.
17. We consider a "publication" to be a disclosure in any public forum, whether it be a journal, a book, or even a working paper on a web site.
18. Oldenburg had tried other devices for ensuring credit, including the sealing of submitted manuscripts and the encoding of significant results as anagrams. Both of these were completely superseded by the devices of publication identified by author and certification by acceptance for publication. For fuller accounts, see Merton (1973:191–203, 460–496) and Hull (1988:323–324).
19. Lightfield (1971) found a strong correlation between how early in his career a scientist is cited and how scientifically productive he turns out to be. Only 2% of those who were not cited within five years of their degree had scientifically productive careers.
20. Our discussion will highlight the quantity of journal slots. The importance of journals and other publication outlets in terms of quality is evident enough. Researchers are ordinarily aware of the reputational differences among journals in their discipline and such differences seem to play an increasing role in evaluating productivity for promotion and tenure in academia.
21. This does not imply that the rates of increase in journal space and author numbers necessarily diverge. Also, a significant portion of journal revenues are third party (university) payments; this distances the value of journals from their ultimate users thereby tending to increase the number of journals that are financially viable.
22. A short (and incomplete) survey of the use of citation counts in economics articles is given by Johnson (1997).
23. Fuchs (1992) discusses such phenomena and characterizes them in terms of two different characteristics of a scientific field: the extent of "mutual dependence" of workers in the field on each other for recognition and rewards, and "task uncertainty" in the sense of the amount of disagreement in the field with respect to the proper ways of doing research. He notes (p. 85) that highly standardized research practices will emerge in the context of a combination of high mutual dependence and low task uncertainty, and cites evidence to that effect developed by Collins and Restivo (1983), who compare the different emphases on formal symbolism and logical rigor between Greek and modern mathematics. This elaboration of the conditions of "use" and its connection to the characteristics of the field are quite compatible with our model and certainly suggest some interesting research into the origins of the differences between different scientific fields.
24. See Hull (1988).
25. See Kealey (1996).

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