

Polycentric structure and informal norms: competition and coordination within the scientific community

Vlad Tarko*

Economics Department, George Mason University, Fairfax, VA, USA

(Received 21 October 2014; final version received 4 November 2014)

The success of the scientific community challenges in many ways our theories of social cooperation and public goods production. It is a very large-scale, decentralized, international organization lacking any central management or a formalized legislative or rule-enforcement body. Even the entry/exclusion rules are lax and unclear. By many standards it should not work. But, instead, it is one of the most successful human endeavors of all time. This paper provides an updated institutionalist theory of how this community works, with an extended discussion of its informal norms, prestige mechanisms, decentralized resource allocation, and interactions with states and civil society. Second, the paper discusses the ways in which the scientific community can fail at its truth-seeking task as a result of distortions created by outside political pressure and interactions with self-interested funding sources arguing that, as long as the polycentric structure is kept in place and the informal norms are preserved, the distortions are likely to be minor.

Keywords: economics of science; polycentricity; informal norms; signaling; prestige

Introduction

Two decades ago Douglass North (1994) has pointed out that “[w]hile there is a substantial literature on the origins and development of science, very little of it deals with the links between institutional structure, belief systems, and the incentives and disincentives to acquire pure knowledge” (p. 364). There were partial exceptions to this lack of concern such as the institutional approach of Polanyi (1962) and Tullock (1966), and Levy’s (1988) analysis of the “market for fame and fortune,” as well as the papers collected in Mirowski and Sent (2002), which focus on “(1) science conceived as a production process; (2) science conceived as a problem of information processing; (3) science conceived as an economic network of limited agents” (Geuna 2003). More recently, there has also been some increased interest in analyzing scientific entrepreneurship and the reward structure in science (Dasgupta and David 1994; Stephan 1996, 2012; Stephan and Levin 1996; Stephan and Everhart 1998; Butos and Boettke 2002; Leonard 2002; Stern 2004), and the dynamics of scientific prestige including the possibility of distorted research agendas (Wible 1998; Adams, Clemmons, and Stephan 2005; Bergstrom 2007; Boettke, Coyne, and Leeson 2014).

The purpose of this paper is to bring Polanyi–Tullock’s institutional perspective up to date by relying on Bloomington school’s institutional analysis and especially on the theory of polycentricity (V. Ostrom 1999; E. Ostrom 2005, chapter 9; Aligica and

*Email: vtarko@gmu.edu

Boettke 2009; Aligica and Tarko 2012). The institutional perspective is necessary for understanding how such a large-scale transnational and decentralized organization dedicated to a vague common goal (truth-seeking) can possibly work, especially considering that truth is generally considered to be a public good (Arrow 1962; Johnson 1972; Dasgupta and David 1987, 1994).

Interestingly, for such a large community, the norms and rules of the scientific community are mostly cultural and informal. It is commonly claimed that communities larger than about a hundred people cannot possibly work effectively without formal rules and some form of central management and monopoly on the enforcement of those rules. For example, Dixit (2003) claims that “[h]onesty is self-enforcing only between pairs of sufficiently close neighbors. Global honesty prevails only in a sufficiently small world. The extent of self-enforcing honesty is likely to decrease when the world expands beyond this size.” These claims are a result of a theoretical model. But if Dixit’s model would have been empirically valid, and not merely internally consistent, it would be a demonstration that science was literally impossible.¹

To put things in perspective, consider a number of scientific communities by field. The economics community currently has almost 34,000 registered members at IDEAS Research Papers in Economics, and there are currently over 209,000 registered authors at the Social Science Research Network. The American Chemical Society has over 164,000 members working both in the academia and in the private sector (almost 25,000 of them are professors). According to the American Institute of Physics, only in the USA, there are over 10,000 physics and astronomers working in the academia. The Society of Biology has about 80,000 members, and the 23 specialized associations that are part of the Federation of American Societies for Experimental Biology span from a few hundred members (e.g. the American Peptide Society) to almost 15,000 members (The Endocrine Society). Yet, despite their size and their complex nested organizational structure, scientific communities have no constitution laying down the rules, e.g. of how peer-review should be made or about the “scientific method” that should be used, and no monopolistic governing body enforcing such rules and deciding who is or isn’t part of the scientific community. Experiments with new rules, such as open-access publishing and preprint archives without peer-review, and new associations created without a need for permission from higher bodies are a natural and significant part of the system. Moreover, although we can separate such scientific disciplines, there is enough overlap between them that we can still talk about science as forming a single, heterogeneous community (Tooby and Cosmides 1992). Indeed, some of the most important discoveries over the past century have happened precisely at the interface between disciplines, e.g. quantum mechanics at the interface between chemistry and physics, genetics at the interface between chemistry and biology, neuroscience at the interface between biology and psychology, or behavioral economics at the interface between economics and psychology.

Despite its size, the scientific community is relatively successful in catching and punishing opportunistic behavior that threatens the production of its public good. Moreover, it has no geographic barriers, and yet it manages to delineate membership in a decentralized and largely informal manner (single organizations and associations may have formalized internal rules but not the community as a whole). The scientific community is arguably one of the most successful human organizations ever created, both with respect to its declared main purpose (truth-seeking) and with respect to secondary goals such as obtaining large government subsidies (while maintaining independence and freedom from interference) and obtaining preferential treatment in public schools or in courts of law (despite often being highly disruptive to common belief systems). It is

remarkable that the internal structure of this organization is so different from that of other large-scale organizations such as states or multinational corporations. Interestingly, the associations that look most similar to the scientific community in terms of their internal mode of organization are religious communities, which may perhaps be due to some historical path dependency (Smolin 2006).

The scientific community highlights the fact that polycentric governance can and do, at least sometimes, scale up, even in the absence of formalized and centralized control. As Polanyi (1951, 1962) originally has argued, science is so successful precisely *because* of its decentralized and quasi-anarchic organization. Consider for example its ability to go against some of our most cherished political ideals, such as the idea of democratic governance in which rules are made in accordance with majority will and opinion. As forcefully argued by some critics of science (Feyerabend 1978, 1993), science is virtually unique in successfully overriding the democratic ideal and shaping public policy (from school curricula to monetary policy) in the direction of truth – as established by the scientific community – *instead* of majority opinion. This would not have probably been possible had the scientific community not had such a decentralized and nonhierarchical internal structure. Its public credibility and authority in defining “truth” for everybody else stems from the fact that many *separated* but informed individuals come in support of the same position. Without the decentralized structure, the uniformity of opinion could have been believed to be the result of the hierarchy.

In what follows, the paper first describes the institutions of the scientific community, applying the polycentricity model to science in order to understand how it is possible for such a large-scale cooperative enterprise to work, and, then, discusses some of the possible distortions of science as resulting from departures from the polycentric organization.

The institutions of the scientific community

Polanyi (1962) points out that “scientists, freely making their own choice of problems and pursuing them in the light of their own personal judgment, are in fact co-operating as members of a closely knit organization,” and “the principle of their co-ordination ... consists in the adjustment of the efforts of each to the hitherto achieved results of the others” (p. 57). This has led Tullock (1966) to ask “why the individual scientist, who feels quite free and unconstrained, is nevertheless led to investigate problems of interest to others, and how, without any conscious intention, he exerts influence on the research done by other scientists” (p. 7). Moreover, “[h]ow does it happen that we can depend upon scientists not only to refrain from faking research results, but to exercise the most extreme precautions to insure accuracy?” (p. 5). Tullock’s basic answer is that “[t]here exists a community of scientists, and this community is a functioning social mechanism which co-ordinates the activity of its members” (p. 5). In order to explain the success of science we thus have to understand this “social mechanism.” According to Polanyi (1962), the critical problem that needs to be solved by the organization of the scientific community is this:

Scientific publications are continuously beset by cranks, frauds and bunglers whose contributions must be rejected if journals are not to be swamped by them. This censorship will not only eliminate obvious absurdities but must often refuse publication merely because the conclusions of a paper appear to be unsound in the light of current scientific knowledge. ... [U]northodox work of high originality and merit may be discouraged or altogether suppressed for a time. But these risks have to be taken. Only the discipline imposed by an effective scientific opinion can prevent the adulteration of science by cranks and dabblers.

The real question in explaining scientific progress then concerns the optimality of the rules and norms, “scientific standards” as Polanyi calls them, based on which the community accepts or rejects new theoretical proposals and new members, given the size of the community and the available technologies for sharing information. Whether or not the scientific community succeeds in its purpose of creating accurate theories of the world depends on its institutional arrangement rather than on a particular method (Kicher 1993). Moreover, it is the nature of those rules and norms that distinguishes the scientific community from religious organizations such as the Catholic Church as well as from the philosophical community (Kendall 1960; Smolin 2006).

Polanyi (1951, 1962) created and used the institutional perspective on science for the purpose of drawing more general conclusions about the proper way in which a social system should be organized (for example, he engaged in the socialist calculation debate). The key concept was “polycentricity” and science was Polanyi’s example of a very successful polycentric system. This idea proved very fruitful. To name just two of the most prominent examples, it was used by Fuller (1978) to analyze the proper limits of adjudication by courts (see also King 2006) and by Vincent and Elinor Ostrom to analyze the conditions under which governance systems are robust to shocks and are able to deal efficiently with public goods’ and common-pool resources’ problems (V. Ostrom 1999; E. Ostrom 2005, chapter 9; McGinnis and E. Ostrom 2011). Paradoxically, the idea has had fewer echoes in the philosophy of science and the economics of science literatures. This paper returns to Polanyi’s concern with understanding the scientific community with the added advantage of now being able to rely on a much better developed concept of polycentricity, as it has emerged from the new institutionalist literature.

The logical structure of the concept of polycentricity, in light of the different cases in which the concept has been successfully used, has been recently mapped out by Aligica and Tarko (2012) providing a succinct analytical guide for understanding any polycentric system (see also Aligica and Tarko 2013; Aligica 2014). Figure 1 gives the “logical structure” of polycentricity.

A polycentric system is a *multiplicity of decision centers* acting independently but under the constraints of an *overarching set of norms and rules* which create the conditions for an *emergent order* to occur via a bottom-up competitive process. The key idea is that the overarching set of rules constrains the competitive behavior in the direction of a beneficial emergent outcome. What makes the scientific community so interesting for both the institutional economist and the industrial organization economist is the fact that this overarching set of rules is a set of *informal* rules emerging and evolving endogenously in a decentralized fashion. Thus, science is a quasi-anarchic enterprise not only in the sense that there is no monopoly of rules enforcement, but also in the sense that the overarching set of rules evolves without any one center having ultimate decision power. In the case of the scientific community, the Aligica and Tarko (2012) structure of polycentricity translates into the institutional features highlighted in Figure 1, with the body of scientific knowledge being the emergent outcome. This account of polycentricity tells us on which aspects of the institutional structure (which, at first glance, may seem confusingly complex) we should focus our attention. Let us give more details about each element highlighted in Figure 1.

The polycentric nature of the scientific community is evident even within a specific domain. There are multiple research centers each with its own somewhat different research agenda and preferred methods of investigation. Journals and publishing houses also often lean in one direction or another either explicitly (in their stated mission) or

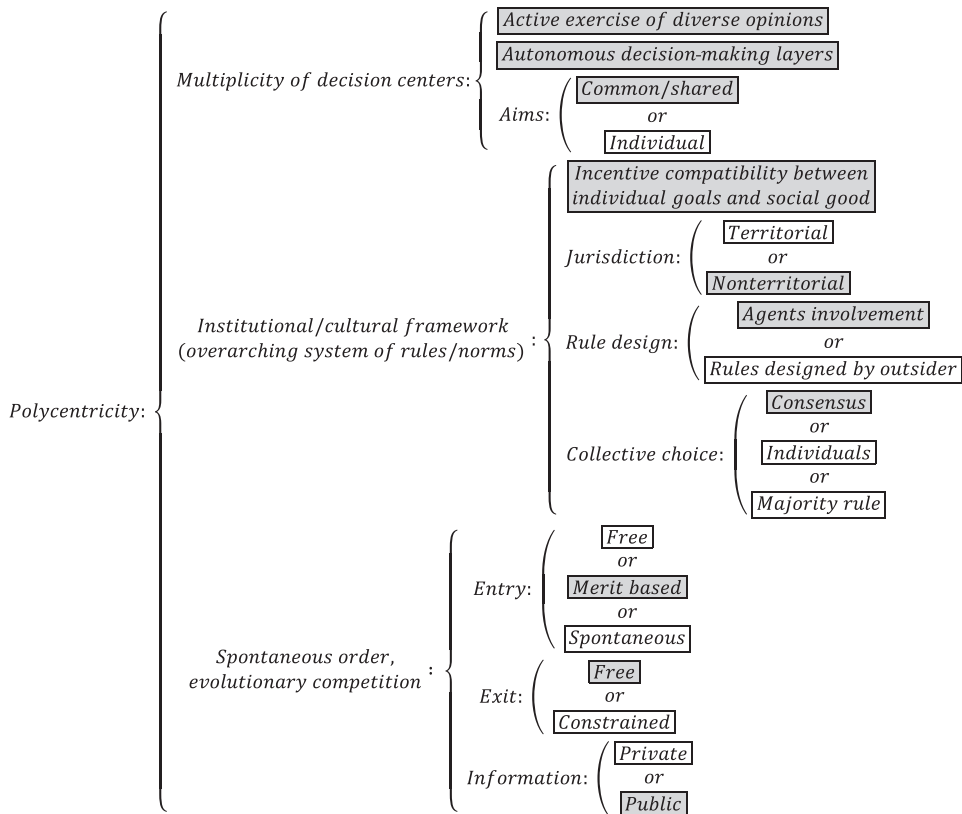


Figure 1. The map of possible polycentric systems, with features of the scientific community highlighted (based on Aligica and Tarko 2012, 2013).

informally (due to the personal idiosyncrasies of their editors). Science is thus essentially anarchic in the sense that there are no official leaders, no universal research method, and the entire process works on the basis of a complex and ever-changing *prestige network*. The impact of scientific publications, i.e. their popularity and usefulness within the scientific community, and implicitly the impact of the journals and publishing houses that publish them and of the academic institutions that create the research, is what generates the evolution in time of this prestige network.

The idea that science does *not* have a unique method is at the core of the controversy surrounding Feyerabend's claim that science is an anarchic community (1993). Feyerabend made the counterfactual argument that, in case of physics, a strict adherence to any of the "scientific methods," proposed by various philosophers of science, would have severely hampered progress and would have prevented actual discoveries – i.e. actual scientific practice is much looser than philosophers of science have often liked to admit. To put it differently, the critical bottleneck separating science from pseudoscience is not at the level of research *practice* (how research is done), but at the level of *acceptance* of the research by other scientists. Kicher (1993) makes a similar institutional argument. Peart and Levy note that having a strict scientific method can be seen as a useful device for reducing expert bias and wishful thinking – it amends the incentive to choose the method that leads to one's preferred result. However, "[t]he

problem with such a suggestion is that it presumes the community can identify and agree upon the optimum estimator or procedure. Experience has long demonstrated the contrary” (Peart and Levy, [forthcoming](#), chapter 7).

Looking at the institutions of science, rather than its presumed method of inquiry, is revealing. The scientific community does many things contrary to common intuitions and theories about the conditions for cooperation in large-scale communities. Perhaps most strikingly, the entry/exclusion rules are lax and unclear. Even authors like Elinor Ostrom (1990), who have significantly expanded our understanding of how communities self-organize, have emphasized entry/exclusion rules as essential for success. While the entry costs for individuals are relatively high involving several years of one’s time and effort – which corresponds to what Aligica and Tarko (2012) call “merit-based entry,” there are no clear entry rules at the level of new research centers. For example, when Freud and psychoanalysis was rejected by mainstream psychology, he and his collaborators have simply organized an alternative society, while still claiming to belong to the scientific community (Watson 1996, chapter 36). There was no formal institutional mechanism by which such a claim could be rejected. On the contrary, at least for a while, the new society has proven highly successful by the benchmark of the informal bottom-up acceptance mechanisms of publications, citations, and wider adoption.

These lax entry rules go hand in hand with the nonhierarchical and non-territorial organization of science. Interestingly, scientists tend to be much more mobile across borders than the general population, both now and in the past (Stephan 2012, chapter 8). As Tullock has noted (1966, 5–6):

This community is a most peculiar one, with its members living in different countries and speaking different languages. Further, it is not even geographically organized. A French scientist studying a certain virus may find that the other scientists whose work is most important to him live in Japan, Italy, Russia, the United States, and Argentina. In a real sense they are his neighbors in the scientific community, but the professor of astronomy who lives next door to him is almost a foreigner in terms of their scientific relationship. Membership in this community is completely voluntary, and the scientists do not think of themselves as controlled by the community or as participating in the control of other scientists. As Lord Brain says, “apart from contributing to... [the body of knowledge], they have no collective consciousness, interest, or aim.” Nevertheless, their search for knowledge is far from random.

The system has not only relatively free entry but also free exit, in the sense that scientists (even very famous ones) who are no longer willing to accept the “scientific consensus” become naturally isolated and ignored – the citation mechanism for granting prestige works both as an inclusive mechanism and as an excluding mechanism. As Kendall (1960, 979) put it, “[t]he ultimate fate of the entrant who disagrees with the orthodoxy but cannot persuade the community to accept his point of view is, quite simply, isolation within or banishment from the community.” Boettke (2012, chapter 17) notes the same about the workings of the economics community but claims that the economic orthodoxy is too strict and, also, that mainstream economists don’t have enough scientific reasons for enforcing the current particular orthodoxy. According to the institutionalist perspective, in order to see whether this criticism is valid, one would need to look more closely at the specifics of the norms of successful scientific communities (such as those of physics, chemistry, or biology) and see whether, or to what extent, the economics community complies with them.

To give a few examples, Einstein's case is quite spectacular (Smolin 2006). Although he was one of the creators of quantum mechanics and relativity theory, he ended up refusing to accept the probabilistic interpretation of quantum mechanics. This has led his research on "the grand unified theory" in a direction that has been, and still is, generally considered useless and misguided. His isolation in this regard was so complete that even the other physicists at Princeton's Institute for Advanced Study ignored him. He was basically excluded (or self-excluded) from the theoretical physics community. He was an outsider who managed, first, to get accepted to widespread acclaim, and, later, to get isolated almost as fast as he got accepted. Strikingly similar examples from economics are Hayek and Mises. They began as highly respected members of the economics community, suffered a period of complete neglect and isolation – as a result of their refusal (justified or not) to go along with the mainstream –, and, at least Hayek, later regained some mainstream fame and respect when real-world events partially vindicated his views.

The reason why the scientific community works, i.e. the way in which it puts to good use its anarchic nature of many research groups that "actively exercise a diverse set of opinions," is that (1) scientists share a common/shared goal (i.e. truth), (2) decisions about what counts as "truth" are taken by consensus (detailed in the next section), and, (3) importantly, there exists an alignment between the norms of the scientific community (described explicitly below) and the incentives of individual actors, in the sense that these rules are generally considered useful for promoting truth-seeking and discovering errors.

There are no rules about changing the rules, but the connection between rules and their consequences is relatively transparent and easy to understand. Scientists subjected to those rules and norms are involved in their design – there is no outside "legislative body" designing the rules. Science is self-governing. These rules and norms are also not entirely fixed – they have evolved as the community dramatically expanded in numbers (e.g. the adoption of peer-review publishing was such an adaptation), as the technology for sharing information improved, and as the sources of funding have changed (e.g. the adoption of the rule that conflicts of interests are to be disclosed). The information relevant for decision-making (i.e. the scientific literature) is public and, at least within the community, the costs of access are small.

The argument is thus that the functioning of the scientific community and the progress of science is the result of this community having a particular culture with a particular set of values: "*Science has succeeded because scientists comprise a community that is defined and maintained by adherence to a shared ethic*" (Smolin 2006, 301, emphasis in the original). This culture may be promoted *within* certain formal organizations, but ultimately these organizations themselves are a product of this scientific culture. So, what is the exact nature of this scientific culture? Kendall and Smolin provide a useful guide.

According to Smolin (2006), the norms of science are as follows: all information is public; arguments about truth matter, while persons or statuses do not; reaching consensus within the community is an important goal; when consensus is not available, skepticism is valued and promoted; theories are often questioned just for the sake of questioning them (there is a certain separation between experimenters and theoreticians and an experimenter doesn't need to provide any prior reason for questioning a theory, even a very well-established one). Smolin argues that although these values are somewhat vague, they still offer a sufficient guideline to understand the forces behind the organization of the scientific community. Moreover, he argues that *failures of the scientific community are caused by departures from these norms*.

Authors like Polanyi, Kendall, and Smolin emphasize that the gradual progress of science is a result of a combination between its conservative nature (i.e. having a large prior favoring of the existing theory against new proposals), the value placed on reaching consensus, and its skepticism related to the ability of any existing theory to fit *all* the facts. As Richard Feynman put it, science is a form of “organized scepticism in the reliability of expert opinion,” but within the context of a search for consensus (quoted by Smolin 2006, 307). Unlike philosophical skepticism, scientific skepticism has its limits: “[i]f an issue can be decided by people of good faith, applying rational argument to publicly available evidence, then it must be regarded as so decided” (Smolin 2006, 301). This is why, Smolin argues, philosophy doesn’t witness the same kind of progress that science does – this has to do with its subject matter only partially, and more with the underlining the shared ethic of the community: in philosophy nothing is ever considered as “settled” and consensus is not as highly valued as in science. Kendall (1960) calls this consensus the “orthodoxy,” and notes that “there is a strong presumption that prior investigators have not labored entirely in vain, and that the community is the custodian of – let us not sidestep the *mot juste* – an *orthodoxy*, no part of which it is going to set lightly to one side.” This orthodoxy is crucial for progress as

[it] must be understood as concerning first and foremost the frame of reference within which the exchange of ideas and opinions is to go forward. That frame of reference is, to be sure, subject to change, but this is a matter of meeting the arguments that led originally to its adoption, and meeting them in recognition that the ultimate decision, as to whether or not to change it, lies with the community.

Polanyi (1962) refers to this as the “dynamic orthodoxy.”

By contrast, if “rational argument from the publicly available evidence does not succeed in bringing people of good faith to agreement on an issue, society must allow and even encourage people to draw diverse conclusions” (Smolin 2006, 301). While the emphasis on consensus distinguishes science from philosophy, it is this second norm that distinguishes science from religion, which tries too hard to enforce consensus even when “people of good faith” still disagree. It is worth quoting Smolin more extensively on this matter:

[I]t is not sufficient to characterize science as an ethical community, because some ethical communities exist to preserve old knowledge rather than to discover new truths. Religious communities, in many cases, satisfy the criteria for being ethical communities. Indeed, science in its modern form evolved from monasteries and theological schools – ethical communities whose aim was the preservation of religious dogma. ... I would like to introduce a second notion, which I call an *imaginative community*. This is a community whose ethic and organization incorporates a belief in the inevitability of progress and an openness to the future. The openness leaves room, imaginatively and institutionally, for novelty and surprise. Not only is there a belief that the future will be better, there is an understanding that we cannot forecast how that better future will be reached. Neither a Marxist state nor a fundamentalist religious state is an imaginative community. They may look forward to a better future, but they believe they know exactly how that future will be reached. (Smolin 2006, 303, emphasis in the original)

As long as there are grounds for reasonable people to disagree, the polycentric nature of the scientific community is crucial for its success because it is this polycentric organization that secures the diversity of opinions. It is not enough to rely on *individual* scientists being creative and able to “think outside of the box.” It is essential for them to

have *institutional environments* where they can pursue their viewpoints. Given the social nature of creativity – the fact that groups of people tend to generate more knowledge than separated individuals – the diversity of institutional environments is important. This is often how controversies are kept alive, and, as long as “people of good faith” still disagree:

[C]ontroversy is essential for the progress of science. My first principle says that when we are forced to reach a consensus by the evidence, we should do so. But my second principle says that until the evidence forces consensus, we should encourage a wide diversity of viewpoints. ... Science proceeds fastest when there are competing theories. The older, naive view is that theories are put forward one at a time and tested against the data. This fails to take into account the extent to which the theoretical ideas we have influence which experiments we do and how we interpret them. If only one theory is contemplated at a time, we are likely to get stuck in intellectual traps created by that theory. The only way out is if different theories compete to explain the same evidence. (Smolin 2006, 304, emphasis added)

Such observations made by science practitioners like Smolin are best understood within the institutional theory of polycentricity. It is this theory that provides the framework for understanding the social role played by the scientific norms identified by Smolin or Kendall. Tullock and Polanyi have identified the challenges faced by a large-scale informal community, but we can now better understand the role played by these norms in solving these challenges.

Scientific competition, coordination, and consensus building

The shared goal aspect of the scientific community creates a somewhat different internal dynamic as compared to that of other types of polycentric systems, such as a market, in which actors pursue distinct individual goals. The norms and desiderata of the scientific community create the *framework* within which scientific research happens. But, in order to fully understand how scientific progress actually occurs, one needs to further detail the *competition* and *consensus building* processes. Roughly speaking, the consensus sought by science

is not synonymous with unanimity – nor with having achieved a simple majority. Instead, consensus connotes broad agreement after a process of deliberation, during which time most members of a group coalesce around a particular idea or alternative. ... A consensus-driven process, in fact, often represents an alternative to voting. ... Science, at least ideally, is exactly this sort of deliberative process. Articles are published and conferences held. Hypotheses are tested, findings are argued over; some survive the scrutiny better than others. (Silver 2012, 383, emphasis in the original)

Scientific competition can be understood as a form of Hayekian rivalrous competition (Hayek 1946 [1980], 1968 [2002]; Kirzner 1985, 1997; O’Driscoll and Rizzo 1985, chapter 6): an out-of-equilibrium situation in which the agents involved try to discover new opportunities for profit, which have not yet been noticed by others. In the case of science, the “profit” is mainly in terms of increased prestige (although money is not entirely irrelevant; Levy 1988).

The opportunities are the new ideas that can be pursued, either new theoretical developments or new empirical strategies. Polanyi (1962) noted that “the decisions of a scientist choosing a problem and pursuing it to the exclusion of other possible avenues of inquiry may be said to have an economic character,” in the sense that “his decisions are

designed to produce the highest possible result by the use of a limited stock of intellectual and material resources.” In this sense we can indeed say that scientists are entrepreneurial (Stephan and Levin 1996).

What makes science an unusual entrepreneurial endeavor is that the “clients” of the service that one is providing are the same as one’s competitors. This matter is linked to the consensus building process. On a market, such consensus is not necessary precisely because suppliers and buyers are separate, and thus, buyers can act independently on their preferences and sellers can independently satisfy this variety of demands. By contrast, in science (in a particular field) everyone is a producer and a consumer of the same product. This is an extreme example of *coproduction* (Parks et al. 1981; Aligica and Tarko 2013). The key insight of the theory of coproduction is that when producers have a vested interest in the product (because they are also consumers) they tend to create and enforce rules against shirking in a self-governing fashion. Unlike the case of team production (Alchian and Demsetz 1972), where an outside monitor, who is the residual claimant of the product, is needed, in case of coproduction the monitoring and incentivizing mechanisms tend to emerge endogenously. Because science presents us with a case of coproduction rather than team production, we can thus expect that such self-organizing governance mechanisms will occur endogenously. Let us briefly note some of the mechanisms that have been created in this regard.

Prestige

The recent economics of science literature has generally emphasized that in science, the problem of free-riding and shirking is addressed, at least to some extent, by the incentives created by prestige and by the “publish or perish” constraint.

Consider first the role and the limits of prestige. Scientists face a free-riding problem on other scientists’ attempts to find the truth because prestige (and sometimes other rewards as well) are shared. One way to deal with the free-riding generated by such spillover effects is by restricting prestige sharing, by means of a “winner takes it all” approach (Stephan 1996). This is obvious, especially with respect to prizes, when secondary authors (or sometimes even main authors like Rosalind Franklin in biology or Gordon Tullock in economics) get a lot less attention and less of a reputation boost than those who receive the prizes. The “winner takes it all” increases competition, but it also has the downside of increasing the risk of not getting much of a reputation boost from your own work, thus creating an incentive for shirking. A system of optimal prestige sharing is thus difficult to create and enforce.

The difficulty of finding the optimum balance between competition and prestige sharing is partially addressed by the “publish or perish” system, which can be seen to approximate a piece-rate compensation prestige system (Miller 1992, chapter 5). This has the advantage of being an on-going system providing a means of assessment at all times. The disadvantage is that it biases the system toward papers and away from book writing, leaving book writing mainly to tenured professors. Some have noted (e.g. Boettke 2012) that a subject may be complicated enough that, at least for our current level of knowledge, it requires more extensive works. From this model, we see that the problem is not cultural, but institutional. As the scientific community has grown in size, a move toward papers is necessary in order to reduce free-riding by means of a more continuous monitoring process. Hence, the move toward papers is a natural consequence of the growth in size.

Signaling and the eccentricity of scientists

A further insight about the ways in which scientific communities prevent free-riding and stimulate the productivity of their members can be obtained by looking at similarities in religious communities. Iannaccone (1992) notes that “a person’s religious satisfaction depends on both his or her own inputs and those of others,” hence highlighting the same element of coproduction that is at the center of science. This leads him to analyze essentially the same economic problem that is faced by science: “people with low levels of participation are tempted to free-ride off those with higher levels since, given the choice, people are better off in a group whose average level of participation is greater than their own,” and “even in a homogeneous group, opportunistic behaviour leads to an inefficient equilibrium with suboptimal participation, since individuals maximize personal welfare by ignoring the external benefits of their participation.”

Furthermore, Iannaccone notes that “[a]lthough it is theoretically possible for religious groups to overcome both problems through appropriate financing, such schemes are rarely practical,” because they would work “only if individual inputs can be accurately observed and appropriately rewarded.” As I have noted above, in case of science, such schemes, although exist, are also difficult to tweak very well. We should thus expect for additional mechanisms to be at play. How do religious communities deal with the fact that “the aspects of religious participation that confer external benefits (commitment, effort, enthusiasm, etc.) are intrinsically difficult to monitor”? Iannaccone’s explanation is that the community may rely on signaling asking members to perform some costly behaviors that hamper their participation in competing communities: “it may be possible to demand of members some salient, stigmatizing behaviour that inhibits participation or reduces productivity in alternative contexts.” This is because “[i]t is ... much easier to observe and penalize *mere involvement* in competing groups than it is to observe the *level* of involvement in those groups.”

This theory is in no way specific to religion. It is a much broader theory that applies to any situation in which (1) individual productivity is hard to observe and (2) production is done by means of coproduction (rather than team production with a third-party residual claimant). As such, it applies to science as well. My suggestion is that this theory explains several aspects of science’s organization. As mentioned earlier, scientists are much more geographically mobile than nonscientists. This works into cutting off scientists from other possible social connections. Moreover, many scientific centers (e.g. Santa Fe, Max Plank Institute, etc.) are in the middle of nowhere. Second, and more importantly, it explains the labor structure of most scientific activities.

Resource allocation: “scientific merit” and social entrepreneurship

A larger coordination problem exists regarding the share of the total budget (in terms of both money and time) to be allocated to each line of research. There are many possible distributions of the available budget. What determines which one actually occurs? Given the polycentric nature of the scientific community, there is no central decision-making body that decides how the allocation should be done. There are two main interrelated factors that determine the distribution: (1) the *success of research* along each of the available lines (what Polanyi 1962 calls “scientific merit”) and (2) the *social entrepreneurship* of various individuals and organizations managing to create focal points of research (Boettke and Coyne 2009).

The success of a particular line of research is generally very hard to predict in advance. It is precisely for this reason that polycentricity is important (Polanyi 1951, 1962); the

only available mechanism for scientific progress is entrepreneurship at the individual and research group levels. The individual agents assess the success of past research avenues and of the likely success of future research and invest money and time correspondingly. As Polanyi (1962) highlights, although we can point out to certain criteria for judging “scientific merit” of a particular line of research (criteria such as plausibility, accuracy, how consequential it is, how intrinsically interesting it is, and originality), these criteria have such a large subjective component that only a bottom-up process of aggregation can be trusted: “the pursuit of science by independent self-co-ordinated initiatives assures the most efficient possible organization of scientific progress” and “any authority which would undertake to direct the work of the scientist centrally would bring the progress of science virtually to a standstill.” As such, the budget distribution is an *emergent quantity* resulting from the actions of individual agents and research funders (the next section details the consequences of having only a limited number of funding sources).

Butos and Boettke (2002) note that, in markets, such bottom-up coordination and resource allocation is done by means of the price system and the profit-and-loss mechanism. But, where is the price system that guides resource allocation in science? And, in the absence of a coordinating “currency,” can we really trust that resources are allocated efficiently? The polycentricity theory perspective is that one does *not* necessarily need a metaphorical equivalent of prices for a productive emergent order to occur; all that is required is that the overarching system of rules and norms are as such that the private self-interest of individual agents is aligned with the social good, hence, setting up a productive “invisible hand” type process. In other words, we can see science as a very large-scale barter society. This works for two reasons. First, the variety of exchanged “goods” (i.e. of specific pieces of scientific knowledge) is limited enough that the double coincidence of wants never poses a big problem. Second, because scientific knowledge is kept public and production is done as coproduction, the exchange is not solely between separated individuals, but between each individual and the broader collective fund of existing knowledge. In our case, the norms of science align the individual scientist’s self-interest in terms of prestige and money with the common goal of the community (truth-seeking). As Smolin (2006, 307) put it: “At its finest, the scientific community takes advantage of our best impulses and desires while protecting us from our worst. The community works in part by harnessing the arrogance and ambition we each in some degree bring to the search.”

The social entrepreneurship aspect of facilitating large-scale coordination is also important. Apart from the role played by certain individuals, the most prominent manifestations of this type of entrepreneurship are the profile of different scientific journals, the creation of scientific associations, and the practice of prizes. A journal, especially a more specialized one, is an attempt at creating a focal point for a particular line of research. How successful such an attempt proves to be depends on the impact it manages to have thanks to publishing widely cited papers. For example, the emergence of public choice and of constitutional political economy as fields of research was accompanied and facilitated by the creation of those respective journals (e.g. see Tullock’s 1991, historical account). The creation of scientific associations, organizing conferences, and setting up mailing lists also coordinates research in more obvious ways. The meetings provide opportunities for scientists to gather and discuss outside the more formal conversation hosted by journal papers, while the mailing lists often publicize job opportunities and help the practitioners of a particular line of research to spread through academia (“colonize” the mainstream).

With respect to prizes, when a scientist receives one, this is not just an act of recognition, but it also serves as a coordinating device within the community – it establishes that something is part of the basic canon to which future research should primarily relate or draws attention to interesting new lines of work. Moreover, the recognition created by prizes lowers the costs for others in pursuing that line of research as relying on a recognized piece of research requires less justification than relying on less recognized research. The prizes also draw attention to certain lines of research *for the entire community to see*. For example, when Elinor Ostrom won the Nobel Prize in 2009 a substantial fraction of the economics community had not heard of her before (Levitt 2009), and thus, the prize also acted as a coordinating signal.

Distortions of science

The analysis so far ignores two important matters: (1) the impact science has upon society at large both in terms of enabling technological progress and of challenging wide spread beliefs (such as religious or political beliefs) and (2) the sources of funding for scientific research. These two factors affect both the institutional framework of science and the directions in which scientific entrepreneurship manifests itself.

With respect to the first aspect, it's safe to say that Galileo's conflict with the Catholic Church is part of the foundational myths of modern science. A more recent example is the distortion of biology in the Soviet Union and China due to political reasons generating massive losses in agriculture and contributing to the humanitarian disaster of the "Great Leap Forward" (Carroll 2006, chapter 9; Pollock 2006). The extreme nature of this example is useful for illustrating the problem of state interference with science in a very clear form keeping in mind that the problem also exists in milder forms in democratic societies (Mooney 2005; Berezow and Campbell 2012).

Noticing that, as they collectivized the agriculture, production was dropping, the Soviet leaders decided to solve the problem by technological means. Unfortunately, science in the Soviet Union was not free of political interference. Lysenko's discourse in 1935, cheered by Stalin himself, captured the atmosphere well: "Both within the scientific world and outside it, a class enemy is always an enemy, even if a scientist" (quoted by Carroll 2006, 223). Following a press campaign organized by *Pravda*, genetics ended up denounced as "bourgeois science," biology textbooks were changed, and numerous Soviet geneticists who refused to acknowledge the value of Lysenko's theory (such that a plant growing in the cold would have frost-resistance offspring) ended up in prisons. Nikolai Vavilov was one of them. He was the president of the Edinburgh International Genetics Congress in 1939 and was one of the most famed interwar geneticists. In his Galileo moment before the Communist Party Central Committee he told them:

Lysenko's position not only runs counter to the group of Soviet geneticists, it runs counter to all of modern biology. ... In the guise of advanced science, we are advised to turn back essentially to obsolete views out of the first half or the middle of the nineteenth century. ... What we are defending is the result of tremendous creative work, of precise experiments, of Soviet and foreign practice. (quoted by Carroll 2006, 224)

This speech was not a good idea. He was condemned for treason, sabotage, spying, and counter-revolutionary activities in 1940, and died in prison in 1943 at the age of 55.

Vavilov was basically defending the ethics of science – the reliance on consensus, the necessity of an aspiring entrant like Lysenko to work his way as part of the community,

and the international nature of the scientific community – while the Soviet distortion involved destroying the polycentric nature of the scientific community within the communist world by enforcing a top-down doctrine. It is this departure from the institutional framework of science, up until the 1970s, that explains the failure of biological science in USSR and China. We see this especially by comparing biology with physics (Pollock 2006, chapter 4). At first, physics was in a similar danger as biology, as quantum mechanics' acceptance of randomness as a fundamental aspect of reality was contrary to Marxist–Leninist views on “materialism.” However, Stalin was told that if he interferes with physics he will not get his atom bomb. This was more important to him than the efficiency of agriculture, so, unlike biology, physics was given a pass. The result was that, apart from Stalin getting his bomb, the physics community within USSR remained relatively free and connected to the scientific community at large (and made important contributions). The point is thus that Soviet physics prospered because the polycentric organization of the physics community and the norms of science were kept in place, while Soviet biology suffered because the community of biologists in the Soviet Union was subjected to centralized political control and the norms of science were overridden.

The second aspect of the problem is related to funding (Greenberg 2001). Some authors have written about the way in which funding can distort science as a consequence of funding sources *wanting certain conclusions* (e.g. Krinsky 2004; Mooney 2005). In my view, this is an issue of relatively minor importance as polycentricity and the prestige dynamics (Levy 1988) limit the impact that corrupted actors have on the system as a whole. The scandal and outrage associated with instances of corruption and falsehood in science showcase not only that these are exceptions to the rule, but also that the rule is taken very seriously. *The only factors that have an important distortionary impact on the content of science are those that significantly reduce polycentricity.*

This is why Smolin's (2006) discussion of funding issues is more interesting. He notes that a reduction of polycentricity occurred in high-energy physics due to the paucity of data sources: the high expense required for building particle accelerators (necessary for testing cutting-edge physical theories) has led to a significant reduction in institutional diversity, which, in turn, has led toward a more status-driven research. High-status physicists, who have already invested a lot of time and effort in a particular theory which has seemed very promising a few decades ago (but less so now), act as gatekeepers for new students limiting the theoretical diversity that is pursued and promoted. Although they are not in a situation in which “people in good faith” all agree, nonetheless, at least in Smolin's opinion, the community currently does a poor job at promoting a sufficient level of skepticism and a wide-enough diversity of viewpoints and, as a result, it has stagnated for more than three decades (which is an unusually long period of stagnation from the perspective of the history of physics).

It is also interesting to note that in macroeconomics, the distorting process is similar, but worse. On one hand, it is expensive to create macroeconomic datasets and, thus, the sources are relatively few – a similar situation to that in high-energy physics. According to Silver (2012, 185), “[t]he government produces data on literally 45,000 economic indicators each year,” while “[p]rivate data providers track as many as *four million* statistics,” which, however, are mostly kept as private information (and are thus not part of science per se). On the other hand, the sources of most of this public data are governments *who have a vested interest in distorting it*. This makes the situation worse because, in case of macroeconomics, the rationale for gathering the data is often not scientific in nature, but pragmatic – the data is supposed to be useful as a tool for social

and institutional design (Buchanan and Wagner 1978; Boettke 2012). For example, as *The Economist* (2011) has humorously noted, economists relying on the official statistics

ignore the biggest imbalance of all: the current-account surplus that planet Earth appears to run with extraterrestrials ... the world exported \$331 billion more than it imported in 2010, according to the IMF's World Economic Outlook ... the world ran a persistent current-account deficit for at least three decades until 2005. In 2001 the deficit was equivalent to 0.5% of global GDP, but by next year the IMF's forecasts imply that the surplus could hit a record 0.8% of GDP.

It is also important to bear in mind that different theoretical viewpoints alter the decisions about *what kinds of data* to gather in the first place.

Conclusions

Science works because it is a competitive polycentric ensemble of diverse research centers and scientific journals held together by an overarching shared ethic: the reliance on consensus reached in the past as a background for current research, the transparency and public nature of scientific information, the emphasis of the diversity of viewpoints when “people of good faith” disagree, treating the diversity of viewpoints as a problem to be solved in the light of the end goal of reaching consensus, and relying on prestige to motivate individual actors. Scientific progress is the consequence of the fact that consensus is valued, but no enforcement mechanisms of consensus are available due to the polycentric organization of the community. The polycentric nature of the system also prevents groupthink to persist or to be a significant problem for the community as a whole. Thus, consensus can be reached only by genuine scientific developments. The driver of the system is the competition between individual scientists, research centers and journals for gaining more prestige in the community. This competition for prestige creates the incentive to find weak spots in the existing consensus – i.e. to act entrepreneurial. This is why, following Feynman and Smolin, we can say that science is the organized skepticism in the reliability of expert opinion within the context of a search for consensus.

Note

1. Doubts regarding Dixit's sweeping claims have been raised before. For example, it seems that large-scale heterogeneous societies (of millions of people), held together mainly by informal mechanisms, have in fact existed (Leeson 2014).

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