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The 'Science-as-Market' Analogy: A Constitutional Economics Perspective

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The 'Science-as-Market' Analogy: A Constitutional Economics Perspective*

by

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1. Introduction

This paper takes a *constitutional economics* outlook at science as a social process, as "a process in which conflicts about truth are resolved" (Buchanan 2001c: 154).[1] The adopted perspective is '*economic*' in the sense that the agents in the science-arena, as in all other social arenas, are assumed to act on their self-interest, and it is '*constitutional*' in the sense that its focus is on the ways in which the 'rules of the game' affect the ways in which the social process of science operates.

With its *constitutional* outlook this paper takes its departure from the more broadly defined *economics of science*, a growing body of literature that is related to, and did in part evolve from, the traditional field of *economic methodology* (Wible 1998; Hands 2001: 353ff.; Mirowski 2004). A major contribution of the economics of science literature is to have narrowed the divide between, on the one side, a methodology or philosophy of science that inquires into the standards that define science as a truth-seeking enterprise and, on the other side, an empirical-sociological outlook at science that emphasizes the private ambitions that actually motivate the players in the science arena, and the social influences to which they are subject, ambitions and influences that may well corrupt their scientific objectivity.

My particular interest here will be in the use that is often made of the science-as-market analogy in support of the argument that, just as it helps in markets to align self-interest and the common good, the 'invisible hand' of competition can be trusted to do its beneficial work in science as well. In focusing on the role that constitutional constraints play in conditioning the workings of the 'invisible hand' in science, just as they do in markets, the present paper aims at further clarifying the relation between

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[1] The concern here is only with *empirical* science for which the way the world is does "meaningfully constrain what is believed to be" (Leonard 2002: 160). Accordingly, the notion of "truth" is meant to imply that the participants "in the process acknowledge the existence of a reality that is itself independent of any belief about it" (Buchanan 2001c: 154).

methodological rules and a social process that works with self-interested and potentially biased individual agents. A central argument will be that, in the case of science just as in the case of markets, the question of how science and markets work as social processes under *de facto* given conditions, including the prevailing institutional-constitutional conditions, must be distinguished from the question of what kind of institutional-constitutional conditions would be required in order for science and markets to serve their desired social function. In terms of this distinction, descriptive accounts of the social process of science focus on the first question, while methodological arguments on the norms of scientific inquiry are concerned with the second. In a sense similar to Adam Smith's notion of political economy as "the science of a legislator" (Smith 1981: 486), methodology, so interpreted, can be said to be concerned with identifying the kinds of constitutional constraints, i.e. the kind of rules or 'laws,' that scientific communities should adopt if the competitive efforts of their members are to advance the growth of knowledge.

2. The 'Invisible Hand' in Markets and in Science

The main thrust of the economics of science is to draw attention to the fact that a naturalistic outlook at the social process of science need not at all imply the kind of relativism that some authors have concluded from it, nor must it be in conflict with traditional methodological principles of objective science. By applying the Smithean invisible-hand logic of explicitly distinguishing between the *motivation* of the individual agents participating in a social process and the *social function* that is served by the process, the economics of science shows that, just as in markets self-interested economic agents are led to advance the common good, the social enterprise of science may lead self-interested and socially biased researchers to serve the advancement of objective scientific knowledge.[2]

The economics of science can draw support for its claims from a number of studies that rely on invisible-hand type arguments in order to rebut the relativist conclusions that the sociology of scientific knowledge seemed to imply. Among the first to do so was Michael Polanyi who, in his 1962 paper on "The Republic of

[2] As W. Hands (2001: 355f.) notes, the "economic turn" in science theory serves "to reconcile the interest-ladenness of scientific activity with the cognitive virtues of science" by showing, in the spirit of the economic paradigm, "how individually self-interested agents can, within the context of certain institutional structures, bring about a result that is simultaneously (1) socially desirable and (2) not the intention of any individual agent or group of agents." – See also Zamora Bonilla (2002: 301).

Science," portrayed science as a field that exemplifies the principle of spontaneous coordination by mutual adjustment (Polanyi 1962: 55), a field in which the "self-coordination of independent initiatives leads to a joint result which is unpremeditated by any of those who bring it about" (ibid.), but is, instead, "guided as by 'an invisible hand'" (ibid.). Referring to Adam Smith's invocation of the 'invisible hand' Polanyi explicitly pointed out the similarities between the self-coordination in science and "the self-coordination achieved by producers and consumers operating in a market" (ibid.: 56).[3]

Following Polanyi's lead the French sociologist Pierre Bourdieu (1975: 37) has used the "academic market place" metaphor in order to characterize the workings of competitive constraints in science as a social process. "The collective order of science," Bourdieu (ibid.: 33f.) argues, "is built up in and through the competitive anarchy of self-interested actions, each agent finding himself dominated – as is the whole group – by the seemingly incoherent criss-crossing of individual strategies."[4]

In a major treatise on "Science as a Process" David L. Hull (1988: 3f.) has supported the view that the "objectivity that matters so much in science is not primarily a characteristic of individual scientists but of scientific communities." According to Hull, it is the way science its organized as a "system of mutual use and checking motivated by self-interest" (ibid.: 357) that brings about "the coincidence between the professional interests of individual scientists to gain credit and the institutional goal of science to increase our knowledge of the empirical world" (ibid.). In seeking, via their publications, recognition for their own views, scientists are induced to *cooperate* by recognizing the works of others that lend support to their own views[5] as well as to *compete* by critically examining the work of their competitors.[6] It is the cooperative-competitive interaction among self-interested,

[3] Polanyi (1962: 56): "I am suggesting, in fact, that the coordinating functions of the market are but a special case of coordination by mutual adjustment. In the case of science, adjustment takes place by taking note of the published results of other scientists; while in the case of the market, mutual adjustment is mediated by a system of prices broadcasting current exchange relations, which make supply meet demand."

[4] Bourdieu (1975: 32): "The scientific field always included a measure of social arbitrariness …; but this does not prevent the inherent logic of the field … from bringing about, under certain constraints, a *systematic diversion of ends* whereby the pursuit of private scientific interests … continuously operates to the advantage of the progress of science."

[5] Hull (1988: 319): "One cannot gain support from a particular work unless one cites it, and this citation automatically confers worth on the work cited and detracts from one's own originality."

[6] Hull (1988: 4): "Scientists rarely refute their own hypotheses. … Their fellow scientists will be happy to expose these hypotheses to severe testing." – Hull (1997: 121f.): "Scientists … do get credit for discovering mistakes in the work of others."

recognition-seeking scientists that, as Hull (ibid.: 354) notes, "has very much the appearance of providing 'hidden-hand' explanations of the success of science." Without explicitly invoking the invisible-hand metaphor but employing extensively the formal apparatus of economic modeling, Philip Kitcher (1993) has worked out a thorough critique of "the notion that one can infer directly from the existence of social pressures and nonepistemic motivations the conclusion that science does not advance" (1993: 388). The progressive characteristics of science, so Kitcher argues, are not dependent on the truth-love and objectivity of individual scientists but are due to its institutional structures that channels "our efforts towards community goals" (ibid.: 351).[7]

Authors who have recently taken up the market analogy and the invisible-hand outlook at science include Allan Walstad (2002) who portrays "Science as a Market Process" or Thomas C. Leonard (2002: 141) who makes a "case for seeing science as a kind of invisible-hand process."[8] Others have cautioned, though, that the science-as-market analogy may well be misleading, either because it tends to distract attention from characteristic differences between the ways in which competitive forces work in science by contrast to markets, or because it invites an inappropriate generalization of theoretical concepts of economics. The first objection has been voiced, for instance, by James M. Buchanan who argues that it is "improper, even metaphorically, to conceptualize scientists as 'trading'" (Buchanan 2001c: 158), or by Philip Mirowski (2004: 60) who notes that "the act of formal acknowledgment of published work does not look much like a price system." A more extensive argument along these lines has been elaborated by Max Albert (2004; 2006) who points out that, by contrast to markets, in science the driving force of competition is not the exchange between producers and customers but the mutual recognition and checking between producers (M. Albert 2004: 128).[9] The second objection has been raised, for instance, by James

[7] Hands (2001: 367) describes Kitcher's work as "the most influential and most self-conscious attempt by a philosopher of science to enlist economics in an effort to salvage scientific rationality and normative epistemology from the threat of relativism and social constructivism." – For a discussion on Kitcher's approach see also Mirowski (2004: 99ff.).

[8] Referring to earlier contributions that apply invisible-hand type reasoning to science Leonard (2002: 154f.) notes: "What unifies these various projects is the idea that science is successful not because real scientists are selfless truth seekers, but because science is socially organized in an epistemically beneficial way – showing how (and under what circumstances) epistemically impure scientists can produce epistemically good outcomes."

[9] M. Albert (2006: 25): "While scientific competition shows some similarities to competition in markets, there are two important differences. First, the basic transaction in science is not an exchange

R. Wible who questions the appropriateness of the science-as-market analogy on the ground that the economist's typical theoretical outlook at markets cannot be applied to "the market place of ideas" (Wible 1998: 141), because science is "an evolutionary process which cannot be conceived in any manner as a sophisticated extension of any of the equilibrium theories of economic competition" (ibid.: 227).[10]

The two kinds of objections underscore the need to qualify the science-as-market analogy, yet they are not meant to reject an invisible-hand approach to science. They either point to differences in the ways in which the 'invisible hand' works in science compared to markets, namely through competition for recognition in the scientific community rather than via competition for paying customers.[11] Or they point to limitations of equilibrium constructs as models for invisible-hand processes, limitations that may affect their applicability not only to science but to markets as well. When Wible argues that "as an evolutionary social process" science cannot be adequately interpreted in terms of the "presumption of equilibrium" (ibid.: 230),[12] this argument may well be generalized to markets as evolutionary social processes. In fact, one may argue, as Wolfgang Kerber (2006a: 458f.; 2006b: 3ff.) does, that what justifies drawing an analogy between market and science is exactly that both can be viewed as evolutionary processes of knowledge creation.

Drawing on Friedrich A. Hayek's (1978) concept of competition as a discovery procedure and on Karl R. Popper's (1963) notion of the growth of scientific knowledge by conjectures and refutations Kerber argues that competition in markets can be interpreted in analogy to science as a process in which alternative conjectures – in this case, conjectures about consumer preferences and effective ways of meeting them – are put forward and subjected to systematic testing – in this case, the ultimate test of consumer choice. As Kerber (2006a: 458) puts it, "market competition primarily should be seen as a process of parallel experimenting, in which firms

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(no-exchange condition). … Second, production decisions are not governed by the evaluations of final consumers (producer sovereignty)."

[10] Wible (1998: 230): "Scientific competition cannot be described with economic theories of competitive equilibrium. Science is an evolutionary process fraught with flaws, imperfections, and other problems due to the public good nature of scientific theories. Science as a process is not an equilibrium process."

[11] Stephan (1996: 1206) notes that in science a reward structure based on priority in the discovery of new knowledge "offers non-market based incentives for producing the public good 'knowledge'."

[12] Separating the invisible-hand explanations from the equilibrium presumption Hull (1997: S118f.) notes: "If invisible-hand explanations are limited only to those systems that are moving toward or are at equilibrium, then they certainly do not apply very well to the course of science. One of the most important features of science is that it changes and, it is hoped, will continue to change. …The history of science does not look much like a movement toward equilibrium."

compete with different hypotheses (conjectures) about good solutions for the problems of the demand side and can learn from each other through imitation. … In that respect, competition can be seen as a 'test of hypotheses' in which knowledge is generated and spread by imitation."[13] That science and markets can both be looked at as evolutionary, knowledge-creating processes is likewise suggested when, on the one hand, Polanyi (1962) talks about the "Republic of Science" as "a Society of Explorers," "a society that strives towards an unknown future," and when, on the other hand, Hayek (1948b: 101) describes the market process as "a voyage of exploration into the unknown, an attempt to discover new ways of doing things better than they have been done before."[14]

3. The 'Invisible Hand' Within Constitutional Constraints

The thrust of the science-as-market analogy is that in both realms aggregate social outcomes result from processes that can be explained in evolutionary or invisible-hand terms, i.e. as outcomes that are not brought about by design but emerge as unintended byproducts from spontaneous interactions among individuals who pursue their own aims. Invisible-hand explanations are of particular interest – and have been typically employed – in those instances in which the emergent unintended outcomes are in some sense 'beneficial.' Yet, so Buchanan (2001b: 99) reminds us, invisible-hand explanations are per se normatively neutral, they "may be as applicable to 'orders' that are clearly recognized to be undesirable as to those that are recognized to be desirable." Obviously, what kinds of outcomes invisible-hand processes can be expected to produce depends on the specific ways in which the 'invisible hand' works.

The burden of invisible-hand explanations is, as Ullmann-Margalit (1978: 267f.) has noted, in specifying "the process, or mechanism, that aggregates the dispersed individual actions into the patterned outcome: it is the degree to which this mechanism is explicit … that determines the success and interest of the invisible-hand

[13] Kerber (2006a: 458): "The unpredictability of innovations (due to the creativity of human agents), the experimental character of competition, and the role of consumers in markets as selectors suggest the interpretation of competition processes as evolutionary processes of variation and selection."

[14] Lavoie (1985: 26) points to the correspondence between Polanyi's arguments on the 'republic of science' and Hayek's arguments on 'competition as discovery procedure' when he notes: "Economic rivalry among competitors in the market generates knowledge that no rival on his own could have possessed in the absence of that rivalry. This, as … Michael Polanyi has shown, is but a special case of the way that … progress is attained within the 'Republic of Science' … . They are what Hayek calls *discovery processes*."

explanation." Without specifying the working mechanisms of an evolutionary, invisible-hand process one cannot say anything specific about the nature of the outcomes that result. To be sure, one can predict that in all evolutionary processes selection will work in favor of 'the successful,' yet as long as the selection mechanism and the nature of the selection environment are not specified such prediction does not tell us more about what characterizes 'the successful,' other than that they survive. Whether what survives is 'good' in terms of some normative standard cannot be said. 'The successful' survives in the evolution of bank-fraud technology just as in the evolution of cost-saving technology in the automobile industry. Likewise, all evolutionary processes can be said to be knowledge-generating processes, as advocates of evolutionary epistemology like Donald T. Campbell and Popper emphasize.[15] Yet, whether the knowledge gained in the process serves, in terms of some normative standard, 'good' purposes cannot be said in the absence of knowledge about the selection mechanisms that are at work. The competition among rivaling mafia groups generates knowledge as does the competition among scientists.[16]

In an article entitled "What's Wrong with Invisible-Hand Explanations?" Hull (1997) has defended his invisible-hand account of *Science as a Process* against the charge that it implies an optimistic bias. Conceding that, whether invisible-hand processes produce 'good' outcomes – as in Adam Smith's example – or 'bad' consequences – as in the tragedy of the commons – obviously depends on the specific mechanisms that are at work (Hull 1997: 118), Hull argues that his ambition in *Science as a Process* was exactly to specify the mechanisms and to identify the conditions that, in the case of science, serve to align "individual 'selfish' goals and the greater good," conditions that, as he adds, "are frequently realized in science as it has been practiced for the past couple hundred years in the West" (ibid.: 120).

Like Hull, most other advocates of an invisible-hand outlook at science explicitly note, or take implicitly for granted, that the beneficial working of the

[15] According to Campbell (1974: 413) evolutionary epistemology holds "that evolution – even in its biological aspects – is a knowledge process, and that the natural-selection paradigm … can be generalized to other epistemic activities, such as learning, thought, and science." – Popper and Eccles (1983: 133): "On all three levels of adaptation (the genetic level, the behavioral level, the level of scientific theory formation) adaptive changes always start from some *given structure*. … More or less accidental mutations or variations come under the selection pressure of mutual competition, or under external pressure which eliminates the less successful variations."

[16] See Kerber (2006a: 457) for a more detailed discussion of how, in markets, "the institutional framework influences the dynamics and direction of the generation and spreading of new knowledge and therefore channels the evolutionary knowledge-generating competition process."

evolutionary process of science is contingent on the presence of conditions that serve to channel the agents' competitive efforts into socially productive directions.[17] They recognize, in particular, that in science, as in all *social* evolutionary processes, socially enforced rules constitute an essential part of the selection environment and that, accordingly, the ways in which the 'invisible-hand' works will depend on the nature of the framework of rules or the 'constitution' within which the process of science unfolds.[18] They recognize, in other words, that the 'invisible hand' in science – not different from the 'invisible hand' in markets – operates under constitutional constraints, that it is a 'constitutionally constrained invisible hand.' As Leonard (2002: 143) puts it: "Scientific rules, and the means of their enforcement, *constitute* the invisible-hand mechanism," they induce "scientific actors with worldly goals to make choices that (sometimes) lead to epistemologically good scientific outcomes."[19]

The game metaphor has been often applied to markets as well as to science in order to emphasize their spontaneous as well as rule-guided nature. Hayek is known for insisting that the market economy can best be understood as the "game of catallaxy," a game that, as he notes, "proceeds, like all games, according to rules guiding the actions of individual participants" (Hayek 1976: 71). An often quoted prominent example in the science literature is Popper's reference to "the game of empirical science" in his 1935 *Logik der Forschung* where he argues that science is in similar ways constituted by its rules as is a game like chess. Following Popper's lead Jesús Zamora Bonilla has recently elaborated in some detail "the notion that scientific research can be described as a game which is played according to some rules" (2008: 263), applying a constitutional economics and game theoretic perspective to "methodological norms … as rules defining a competitive game" (2002: 300).

Market and science are games of competition. In both realms, the engine that drives the evolutionary invisible-hand process is the competing ambitions of reward-seeking agents. And in both realms competition is not unbounded but is *constitutionally constrained*, subject to rules that can be more or less suitable in channeling the participants' ambitions in socially productive directions. In the same way in which the economic constitution can be studied in its effects on the market

[17] Leonard (2002: 142, 153): "Strictly speaking, invisible-hand processes can also lead to collectively bad rather than good unintended consequences. … If it is to have any explanatory force, the term 'invisible hand' must be more than a label for a black-box process."
[18] Hull and Polanyi refer to such rules when they speak of the "conventions of science" (Hull 1997: 120) and the "professional standards of science" (Polanyi 1962: 57).
[19] Leonard (2002: 160): "Like markets, science is successful because it has robust institutions."

process, the rules of the game of science can be studied in their working properties, and just as in the case of markets in the case of science the question can be asked of which rules promise to be most suitable in reconciling the participants' self-interest with the social function of the enterprise. Such exploration into the constitution of science is, according to Wade Hands (2001: 361), the subject of the "economics of scientific knowledge" which inquires into the question of whether the behavior of scientists and the institutions of science work together to "produce scientific products that are cognitively efficient or optimal (or if they are not optimal, how the institutions might be changed in order to improve epistemic efficiency)." And Kitcher sees the role of "social epistemology" in undertaking such inquiry, namely "to identify the properties of epistemically well-designed social systems, that is, to specify the conditions under which a group of individuals … succeed, through their interactions, in generating a progressive sequence of consensus practices" (Kitcher 1993: 303).[20]

Authors like Ian Jarvie (2001) and Hans Albert (2004: 30f.; 2006: 121f.) credit Popper with having initiated a constitutional turn in the philosophy of science by drawing attention to the role that methodological rules play as constitutional constraints in science as a social institution.[21] In contrast to a sociology of knowledge that sees the objectivity of science in jeopardy because of socially biased interests and perceptions of scientists, Popper has argued that "scientific objectivity" does not depend on the individual scientist's impartiality or objectivity but is a "product of the social or public character of the scientific method" (Popper 1971: 220), resulting from the "*friendly-hostile co-operation of many scientists*" (ibid.: 217).[22] It rests on "the public character of science and of its institutions which imposes a mental discipline upon the individual scientist" (Popper 1994: 155f.).

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[20] Hands (2001: 367): "Economists are interested in finding out the arrangement of our social institutions that is most conducive to economic efficiency; Kitcher's normative philosophical project is to find out the arrangement of our cognitive institutions that is most conducive to epistemic efficiency (that best encourages the formation of reliable beliefs)." – Bartley (1990: 93) assigns a similar role to epistemology when he notes: "Epistemology … must seek to identify what contributes to the growth of knowledge, and what stands in its way. It must seek to understand the principles underlying the competition of ideas in the marketplace of ideas, and to identify ideas and institutions that do, and that could, contribute to such a competitive market, and those that hinder it."

[21] Jarvie (2001: 35) speaks of "a 'social turn' in Popper's thought" and notes "that insoluble difficulties in the 'pure' logic of science led Popper to propose, in 1935, that we treat science as a social institution incorporating a set of rules, or methodology, designed to advance us towards agreed-upon aims." According to Jarvie (ibid.: 36) Popper's *Logik der Forschung* "contains profound and original ideas on the social character and constitution of science" (ibid.: 36).

[22] In a similar spirit Hayek (1948a: 15) notes: "Human Reason, with a capital R, does not exist in the singular, as given or available to any particular person, … but must be conceived as an interpersonal process in which everyone's contribution is tested and corrected by others."

In terms of the "institutional … analysis of the conditions of progress" that Popper (ibid.: 154) advocates, methodological rules can be understood as conventions or 'rules of the game of science' that serve to advance the growth of knowledge. In this sense Popper's falsificationist methodology is not meant as a descriptive account of the history of science or an empirical statement about the motivation that drives individual scientists. It is, instead, about constitutional choice. It is meant as an answer to the question of what rules of scientific practice should be adopted if one wants the process of science to best serve its function of improving our knowledge about the world.[23] With his emphasis on falsifiability Popper recommends a scientific practice that seeks to advance the growth of knowledge through the elimination of error, a recommendation that naturally follows from his evolutionary outlook at knowledge processes. The engine that, from such outlook, drives the growth of knowledge is a competition among alternative conjectures that facilitates the elimination of hypotheses that conflict with evidence. And the social strategy that makes this engine work effectively is adopting rules of scientific practice that encourage such competition (Popper 1994: 154f.).[24] As Jarvie (2001: 29) puts it, the "social content of Popper's philosophy of science was institutionalization of the decision to maximize falsifiability," an institutionalization in the form of methodological rules that "create incentives to discover and to expose error" (ibid.: 41).[25]

Commenting on Popper's institutional approach to methodology H. Albert has stressed the instrumental or 'technological' nature of methodological rules. According to Albert, with its constitutional interpretation Popper has pointed out that methodology can be viewed as *social technology*.[26] It is concerned with the social-technological problem of identifying suitable rules for science as a social enterprise

[23] Jarvie (2001: 13): "Popper offered neither sociological nor historical generalizations about science. He offered methodological rules as legislated norms, not empirical ones." – The validity of Popper's methodology as a constitutional recommendation for how the 'cumulativeness' of science can be promoted is not affected by Thomas Kuhn's (1970: 96) doubts about whether the history of science confirms "the ideal that our image of its cumulativeness has suggested."

[24] Jarvie (2001: 43f.): "Popper suggests adopting a supreme or meta-methodological social rule not to avoid falsification; in order to monitor this rule, co-inquirers must be free to check one another's claims."

[25] Jarvie (ibid.: 52): "Falsifiability is not self-justifying like a tautological truth. It has to be adopted, a decision that is taken in order to foster certain aims."

[26] H. Albert (2004: 31): "Meines Erachtens lassen sich Poppers Vorschläge am besten technologisch interpretieren, also als Versuche einer an der Zielsetzung des Erkenntnisfortschritts orientierten Sozialtechnologie."

that seeks to advance the growth of knowledge.[27] Such concern with the constitution of science is, as H. Albert (2005: 173; 2006: 124ff.) argues, but one instance of the more general social-technological inquiry into how social arrangements can be framed by constitutions that guide the participating agents' interaction in ways that serve intended purposes. In this sense, methodology can be viewed as a counterpart to the "science of political economy" in the spirit of Adam Smith, i.e. as a science concerned with "designing appropriate legal and constitutional constraints" (Buchanan 2001a: 52).

Like social-technological recommendations in general, the methodological recommendations can be critically examined in terms of their instrumental suitability in serving the aims that they are supposed to promote. As statements about what the rules of science *should* be they address a normative problem, but they are 'normative' only in the sense of *hypothetical* rather than categorical imperatives. In the same way in which social-technological recommendations in general inform their addresses about 'social tools' that can help them to better achieve certain aims, methodological recommendations tell the addressees to whom they are directed what rules they should adopt if they wish to advance the knowledge-generating capacity of science.

Hypothetical imperatives are statements about matters of fact. As empirical conjectures about how a supposed goal can be achieved they are refuted if the 'tool' that they recommend is in fact not suitable for the stated purpose. And they are ineffective if their addressees do not wish to pursue the goal the 'tool' is recommended for. It is in reference to their conjectural nature that Jarvie (2001: 44) comments on Popper's methodological proposals: "Both the rationale of the methodological choices and whether the choices will in fact foster the desired aims are matters on which there can be reasoned dispute."

4. The Constitution of Science as a Social Contract

Constitutional recommendations must be addressed to someone. In order to be of relevance the goals that they suppose must be those the addressees can be expected to share. And in order for such recommendations to be valid the rules that they propose

[27] In his 1942 article "The Normative Structure of Science" R.K. Merton interprets the institutions of science in a similar social-technological sense: "The institutional goal of science is the extension of certified knowledge. … The institutional imperatives (mores) derive from the goal and the methods. The entire structure of technical and moral norms implements the final objective." (Quoted from Walstad 2002: 31).

must be in fact capable of advancing the supposed goal. In other words, in order to have an impact the authors of constitutional proposals must provide arguments to their addressees for why they should consider the supposed aim desirable, and why they should consider the proposed rules to be suitable instruments for promoting that aim.

A constitutional economics that inquires into how "the legal framework, the 'laws and institutions,' of the marketplace (can) be designed so as to further the 'general interest'" (Buchanan 2001a: 52) advances constitutional proposals as conjectures about what rules the members of a relevant constituency might voluntarily agree on to their mutual advantage. Adopting a contractarian perspective, it analyzes constitutional choices in terms of a social contract among those whose behavior is to be governed by the adopted rules, and whose voluntary agreement is the *ultimate* test for whether or not the proposed rules are to their mutual benefit.[28] The principal task of such contractarian-constitutional inquiry is to identify common interests that the members of the relevant constituency can be expected to share and to identify rules that are suitable for advancing these common interests.

In their capacity as constitutional recommendations, methodological proposals must also be addressed to someone, and in order for them to have an impact their addressees must be convinced that the recommended rules are capable of fostering a goal that they share. The natural addressees of proposals for the rules of science are the members of the scientific community who are invited to agree on the recommended rules as the constitution of the enterprise they are engaged in. In Jarvie's (2001: 47) reading Popper's methodological proposals can be interpreted in this sense, namely as suggesting "that science is to be seen as an interested group that shares an aim and legislates conventions for itself in order the better to pursue that aim."[29] And even a contractarian interpretation is invited when, according to Jarvie (ibid.: 44), Popper's arguments imply "that we treat the refutability criterion as a proposal for a suitable agreement or convention," an agreement by which the

[28] Respecting the addressees as the ultimate judges on what serves their interests does not require one to take their proximate preference for rules as the ultimate word. Because of their limited insights into the actual working properties of alternative rules the addressees' *proximate* preference may be in favor of rules that they would not wish to adopt if they knew what they will produce. By informing them about the working properties of rules the constitutional advisor may cause them to revise their proximate preference.

[29] Jarvie (2001: 68): "For the methodological rules Popper proposed … are directed to science conceived of as a general and abstract public. They enjoin: here, if you want to respect and advance these aims, is a set of proposed procedures. … Popper's arguments and rules will then seem directed to men and women of good will who want to advance the project of science."

scientific citizenry self-legislates its constitution.[30] Interpreted in this sense, Popper's proposal presupposes that the members of the scientific citizenry share a common interest in the advancement of knowledge and that what needs to be argued for is the suitability of the recommended convention in furthering that aim.

Adopting a constitutional economics perspective Zamora Bonilla and José L. Ferreira (Ferreira and Zamora Bonilla 2006) explicitly advocate a contractarian approach to methodology.[31] The authors share with invisible-hand accounts of science the assumption "that scientists can be depicted as self-interested, strategically behaving agents" (Ferreira and Zamora Bonilla 2006: 191), they emphasize however that, by contrast to approaches "that describe scientific order as the emergent outcome of some market-like mechanism" (ibid.: 194), their contractarian approach looks at methodological rules as constitutional constraints "which researchers would prefer to impose on themselves" (ibid.: 192).[32] The focus of Ferreira's and Zamora Bonilla's contractarian outlook is on the *constitutional preferences* that "researchers interested in their own reputation, and perhaps in other epistemic goals" (ibid.: 194) can be expected to share, and they seek to show "that recognition-seeking scientists will have an interest in establishing methodological norms which tend to select theories of high epistemic value" (ibid.: 191).

While Ferreira and Zamora Bonilla emphasize the difference between his contractarian and invisible-hand accounts of 'scientific order,' on closer examination the contrast between the two does not appear quite as definite as they suggest. As has been noted before, most of the advocates of invisible-hand accounts of the process of science acknowledge, implicitly if not explicitly, that the beneficial working of the 'invisible hand' is contingent on the presence of rules of the game that guide the competing scientists' ambitions into productive directions. Once the role of the rules

[30] Jarvie (2001: 218) speaks of "the scientific citizenry's submission to methodological rules." Because they are "self-legislated conventions" (ibid.: 47), methodological rules are, as Jarvie (ibid.: 52) notes, "more like the laws of the land and less like the (more stable and finite) set of rules that constitute chess."

[31] A similar outlook has been proposed by Zamora-Bonilla in an earlier paper in which he supports the view that "the methodological rules of a scientific discipline can be understood as the result of an agreement between the members of that discipline" (Zamora Bonilla 2002: 319). - See also Zamora Bonilla (2008).

[32] In his earlier paper Zamora Bonilla (2002: 309) had argued that, "according to the contractarian approach, the 'constitution' of a scientific discipline should be understood as an *exchange of constraints* on the acceptance of scientific statements," and that methodological rules can be viewed as such constitutional constraints on which "rational recognition-seeking scientists can reach a collective agreement …, especially if the choice is made 'under a veil of ignorance', i.e., before knowing what theory will be proposed by each scientist" (ibid.: 300).

of the game is acknowledged, however, we must obviously distinguish between two versions of invisible-hand accounts, one more modest, the other more ambitious. The modest version claims that, *given adequate rules of the game of science*, the invisible hand of competition among recognition-seeking scientists can be shown to produce epistemically efficient outcomes. It remains silent, though, with regard to the question of how the rules of the game come about. By contrast, the more ambitious version makes the additional claim that the rules that make for an efficient game of science can themselves be explained in invisible-hand terms.

Since they remain silent about how the rules of science are established, modest invisible-hand accounts can be perfectly compatible with a rule-focused contractarian account. If there is a conflict, it can only exist between the latter and invisible-hand accounts of the more ambitious variety. Yet, even such conflict need not necessarily exist. In fact, whether it exists or not depends on the specific explanatory claims that are associated with a contractarian approach, claims that, similar to the two versions of invisible-hand accounts, may come in a weak and a strong version. In its weak version a contractarian approach merely claims to show that scientists have a *common constitutional interest* in rules that make for an efficient game of science, and that they could be expected to agree on such rules if they were to collectively choose a constitution for their common enterprise. It does not claim that an explicit agreement is needed for such rules to come into effect, or that the rules of the game of science are actually based on a social contract. What distinguishes the strong version from a weak contractarian approach is that it invokes such explicit agreement in its account of the constitution of science.

While the strong contractarian and the ambitious invisible-hand version are clearly competing explanatory accounts, a weak contractarian approach may well be compatible with an ambitious invisible-hand account of 'scientific order,' a possibility that Ferreira and Zamora Bonilla do not explicitly recognize even though most of their arguments appear to be more in line with the weak than with the strong contractarian version.[33] To show, as they do, that scientists can be assumed to have good reasons for sharing a common constitutional interest in epistemically efficient rules does

[33] Not more than the weak version appears to be implied when Zamora Bonilla (2008: 265) notes about his contractarian-constitutionalist approach: "From a game-theoretic perspective, two different but interrelated sets of questions emerge once we interpret scientific norms as the rules of a game: First, what will scientists' behavior be *once* certain norms have been established? And second, what norms would they *prefer to have* if they were given a choice?"

obviously not imply that such rules must come into effect by deliberate agreement. The very fact that they are in scientists' common constitutional interest may just as well be said to facilitate their spontaneous emergence. This is what M. Albert (2004) seems to have in mind when he supposes that the rules that govern competition in science are themselves generated in the process of competition,[34] and that methodological rules that represent suitable tools for the advancement of knowledge have not only a good chance of prevailing but are also incentive-compatible in the sense that scientists will by and large have an interest in complying with them (ibid.: 133). The reason is, so Albert argues, that scientists in order to play the game of science need to coordinate on some rules and that methodological rules that provide an effective technology for arriving at valid theories represent a natural focal point for the coordination problem that the scientific community has to solve (ibid.: 148).[35] Even though Albert does not refer to such contractarian interpretation, rules that can be assumed to be in scientists' common constitutional interest would certainly qualify as focal points in a process of spontaneous coordination.[36]

The notion that epistemically efficient rules represent a focal point for the coordination problem that scientists face appears to be also implied when Hull (1988: 320) argues: "Once one identifies the operative norms of research science, the explanation for the high frequency with which individuals adhere to these norms becomes obvious. It is in their own self-interest to do so." Such emphasis on the self-enforcing nature of the rules of science is perfectly compatible with a contractarian approach that points to scientists' common constitutional interest in such rules.[37]

5. Competition for Resources and Multi-Level Selection

In markets and in science the invisible hand of competition and the rules of the game must work together to produce efficient outcomes. How well markets and science work as social systems, depends on the quality of the rules, or the constitution, on

[34] M. Albert (2004: 130): "Wissenschaft als Institution ist ein Wettbewerb, der auf bestimmten Regeln beruht, die sich in diesem Wettbewerb selbst herausgebildet haben."
[35] M. Albert (2004: 143): "Methodologische Regeln, die als Technologien zur Gewinnung wahrer Theorien gedeutet warden können, bieten sich als Fokalpunkt des Koordinationsspiels an."
[36] Kitcher (1993: 305) compares the coordination problem scientists face with the problem faced by "a philosopher-monarch, interested in organizing the scientific work force so as to promote the collective achievement of significant truths." Since, as Kitcher (ibid.) adds, "science, of course, has no such benevolent dictator," the required coordination must be brought by the 'invisible hand' or by an explicit social contract.
[37] Hull (1988: 394): "Scientists need very little encouragement to adhere to the institutional norms of science. … Scientists adhere to the norms of science because it is in their self-interest to do so."

which they are based. And the quality of their constitutions is measured in terms of their capacity to advance the respective aims that markets and science are supposed to serve. The clue to the beneficial working of the competitive processes in both realms is that they are governed by rules that align the individual participants' self-interested ambitions with the social purpose of the enterprise.[38]

Since Adam Smith's critique of the 'mercantilist system' and his argument for the "simple system of natural liberty" it has been a widely shared understanding among economists that the proper measuring rod for the performance of markets is how well they serve the interests of consumers, a notion that has been condensed in the concept of *consumer sovereignty*.[39] To Adam Smith it was self-evident that consumer sovereignty is the adequate ideal for an economic constitution, because, so he argued, we produce in order to consume and should, therefore, assign priority to consumer interests over producer interests in choosing the rules of the economic game (Smith 1981: 660). He presumed that – translated in the language of constitutional economics – it is in the common constitutional interest of all participants to have the economic game they play based on rules that give priority to their interests as consumers, rather than to the interests in protectionist privileges that they may harbor in their capacity as producers.

If science as a social system is supposed to advance the growth of knowledge, the quality of the rules of the game of science is to be measured in terms of their capacity to further this aim. Most of the authors whose comments on the issue have been reviewed above not only take for granted that to improve our knowledge of the world is, indeed, the intended function of empirical science as a social enterprise, they also appear to presume, implicitly if not explicitly, that the participants in the game of science share a common constitutional interest in epistemically efficient rules of the game. Furthermore, these authors seem to be also in essential agreement that such rules have a good chance of prevailing in the competitive process and to be spontaneously enforced to a sufficient degree by the mutual monitoring among recognition-seeking scientists. Hull (1997: 122) describes science as "the only self-policing system that actually polices itself to any significant extent."

[38] As Hull (1997: 121) notes: "Although appeals to duty certainly have some effect, it always helps if individuals do not have to sacrifice their individual goals for the good of the group. Social systems work much better when virtue and individual benefit go hand in hand."
[39] For a more detailed discussion see Vanberg (2005: 35ff.).

From the self-policing nature of science and the fact that, as Polanyi (1962: 60) puts it, "scientists exercise their authority over each other," M. Albert (2006: 25) concludes that science is characterized by a kind of *"producer sovereignty*," by contrast to the *consumer sovereignty* that reigns in markets. In science, so Albert argues, it is the producers themselves who by their mutual monitoring pass the relevant judgment on each others' work, while in markets the choices of consumers are the ultimate verdict on success and failure (consumer sovereignty).[40] While such 'producer sovereignty' surely exists, it can only work, of course, within the limits imposed by the need of scientists to "seek resources from society at large to support their research" (Walstad 2002: 38). Science as a social system does not generate endogenously the resources it needs for its functioning. Scientists need support from resource-providers. It is this need that, as Walstad (ibid.) puts it, "connects the purposes of scientists with those of people in the wider community," and, thereby, imposes constraints on the ways in which the self-coordination among producers in Polanyi's *republic of science* may operate.[41]

That in science the competition for recognition among peers is tied up with the competition for resources is an obvious fact. In order to be able to participate in the competition for recognition scientists need to secure resources, and this introduces another level of competition that has its own rules. Accordingly, the question arises of how the two kinds of competition are related to each other, in particular in regard to their respective impact on the epistemic efficiency of the scientific process.[42] Even if the fact that "people in society at large value the technological advances that science makes possible" (Walstad 2002: 38) may seem to support a harmonious relation between the two because of the "correlation between the epistemic and the pragmatic

[40] See also M. Albert (2004: 143f.). In a similar sense Bourdieu (1975: 23) points out that "in a highly autonomous scientific field, a particular producer cannot expect recognition of the value of his products … from anyone except other producers" and that, in this regard, "the scientific field functions in exactly the same way as a highly autonomous artistic field." – As Hull (1988: 306) notes: "Scientists must seek first and foremost to have their work accepted by their peers, not by government officials, science reporters, or the general public."

[41] Referring to the selection forces that result from the need to secure resources G.E. Allen (1991: 699) has commented on Hull's invisible-hand account: "Hull admits to being a partial realist – there is some sort of real world out there, after all against which competing scientists test their theories. But often more important, at least in the short run, are social and political factors such as the ability of individuals or groups to corner research funds, attract graduate students, and gain control of key journals or professional societies."

[42] Stephan (1996: 1231): "Given the role that resources play in scientific discovery, it is important to understand more fully how scientific outcomes relate to the way governments and philanthropic organizations provide resources."

values of theories" (Ferreira and Zamora Bonilla 2006: 205),[43] such harmonious relation is by no means guaranteed. Whether the rules according to which scientists compete for recognition, and the rules that govern the competition for resources, are well aligned, and whether they support or inhibit each other in promoting the growth of knowledge, is an empirical matter.[44]

Comparable to the case of markets, in science forces of competitive selection work at several levels, and the working properties of the entire system will depend on the degree to which the rules of competition at the different levels are in harmony with each other and with the purpose of the overall enterprise. In the case of markets, the ways in which rewards are assigned within a company to the various participants in the corporate enterprise will have an obvious effect on how well the firm performs relative to its competitors. What a firm needs to do in order to perform well in its environment will, in turn, depend on the rules that political authorities at various levels – local, regional, national, and supra-national – define for the economic game, and on the selection forces the firm faces in a global market environment. Likewise, in the case of science, the ways in which research institutes, universities and professional associations assign positions, promotions, rewards and honors to their members will have an obvious effect on the ways in which scientists compete with each other. And what kind of internal reward structure will help to make research institutes, universities and professional associations, in turn, successful in the environment in which they have to compete for resources will depend, among other things, on the nature of the legal-institutional framework within which they operate, including the rules that define the terms of competition for government funds, for research grants and for other forms of financial support.

In evolutionary biology the concept of 'vicarious selection' has been introduced to describe a harmonious relation of the selection principles that operate at

[43] Zamora Bonilla (2008: 271): "However, as long as the results of a discipline have some practical consequences, on which scientists payoffs depend, it is sensible to assume that a discipline whose rules of inference and observation lead *systematically* to mistaken practical conclusions will cease to get the resources it needs. So, the members of a scientific discipline will have an interest, if only for this reason, in collectively adopting a system of rules which is efficient in the production of (approximately) true statements."

[44] Jarvie (2001: 66) notes in reference to Popper's methodological proposal: "Election to professorships, elections in national academies and in learned societies, the selection of journal editors, were all conducted by sets of rules that bore little or no relation to Popper's methodological and procedural rules. It is not at all clear that the weight of these offices and their electoral rules was always brought to bear to move in the direction Popper intended, namely, critical open-mindedness (especially if Kuhn is to be believed)."

different levels of a multi-level evolutionary system. Generalizing the concept, the selection principle that operates within a subunit of a more inclusive system – such as, e.g., a firm within a national market, a university within a national university system, a national economy within a global market, a national academic system in a world-wide scientific environment – can be said to be 'vicarious' if it selects in favor of characteristics that enhance the capacity of the unit to successfully compete in its own selection environment. The reward structure within a firm is, in this sense, 'vicarious' if it guides the self-interest ambitions of its members in directions that strengthen the firm's ability to compete with its rivals. Analogously, a university operates as a 'vicarious selector' if its internal rules of competition are in line with the competitive constraints it faces in its environment. And the same logic applies to the relation between the rules of competition that exist in any system and the selection principles that operate in its more inclusive environment, be it the relation between a university's internal rules of operation and the legal-institutional environment it faces at the national level, or be it the relation between a national economic constitution and the selection forces that work in global markets.

Since sub-systems, whether in markets or in science, that operate on 'non-vicarious' or 'dysfunctional' selection principles are at a disadvantage in competition with systems that operate as vicarious selectors, there will be a tendency, at least in the long run, for sub-systems to adopt internal rules of competition that are in line with the competitive constraints that they face in their more inclusive selection environment. This does not rule out the possibility, though, that systems – be it firms, universities, national economies, academic professions, etc. – that operate internally on rules of competition that are not in line with those that prevail in the more inclusive selection environment may not survive for more or less extended periods. Accordingly, in science sub-systems may be able to sustain themselves over considerable time-periods that operate on selection principles that do not advance the growth of knowledge even if in the more inclusive system of science epistemically efficient rules of competition are in force.

Some authors have suggested that contemporary mainstream economics – rather than working as a 'vicarious selector' – may be just one such case of a sub-system in which selection principles prevail that are dysfunctional in the sense of encouraging the production of a kind of 'scientific output' that is not only of little use and interest to the wider community, but also contributes little to the growth of

knowledge of economics as an *empirical science*. Echoing John Cassidy's sobering report on "The Decline of Economics" that appeared in a 1996 issue of the magazine *The New Yorker*, Bruno Frey (2003: 213) has observed that "non-economists are using the results produced in modern economics and its publication system less and less, because they judge them to be far from relevant."[45] Thomas Mayer (1993: 10) arrives at the conclusion that economics suffers from an excessive 'producer sovereignty,' and that it is because of the very lack of feedback from 'consumers' that the 'invisible hand' fails to guide the research efforts of economists into epistemically productive directions.[46] Agreeing with Philip Mirowski's (1989: 381) diagnosis that "the pressure to usurp the legitimacy of science has always weighed down economic research," Mayer argues that their desire to "claim a kinship to the hard sciences" (ibid.: 15) and to "appear like physicists" (ibid.: 16) has tempted economists to overly focus on one of the tools of physics, mathematics, and to allow "formal criteria … to invade what should be the domain of empirical science economics" (ibid.: 66).[47] According to authors like Cassidy, Frey, Mirowski and Mayer it is the desire to raise their status by turning their discipline into a branch of applied mathematics that has lead economists to adopt selection principles that are counterproductive, both with regard to the outside reputation of their profession and with regard to its contribution to the growth of knowledge.

6. Conclusion: The Local Vulnerability and the Global Robustness of Markets and Science

If markets and science can be looked at as multi-level evolutionary systems in the sense explained in the previous section, an important distinction can be drawn

[45] Frey (2003: 218): "Economics loses its importance for advising governments and becomes increasingly less attractive as a field for students." – Frey specifically targets the refereeing system as an inefficient selection principle for journal publications and criticizes its harmful indirect effects, because "rankings of individuals, departments and universities in modern economics are based to a large extent on publications in refereed journals" (ibid.: 211). – That the refereeing in general has its grave deficiencies is suspected by Henneberg (1997: 1) who notes: "It is argued here that peer review as now undertaken by most scientific journals stifles scientific communication, slows the advancement of knowledge and encourages dishonest behavior among referees."

[46] Mayer (1993: 10): "The reason for this market failure is that the market is dominated by the producers, predominantly academic economists, so that the usual market discipline does not exist. Unlike academic researchers in fields such as medicine or law who work for a large market of practitioners, academic economists, by and large, write for each other. Hence, their tastes, not the consumers', determine what is produced."

[47] As Mayer (1993: 20) summarizes his argument: "I have treated the methodology of economics as a problem in public choice theory, and discussed how self-interest is likely to bias the methodological choices that economists make, particularly in the direction of over-using mathematics and formalism."

between *proximate selectors* that producers in markets and scientists face in their immediate environment and the more remote and *ultimate selectors* that, globally and in the longer run, determine what counts as success and what as failure in markets and in science. How well markets work in the service of consumer preferences, and how well science works as a knowledge-generating enterprise will surely depend on the extent to which the internal rules of operation and the external constitutional environment of business enterprises and scientific organizations are supportive of these goals. There are good reasons, though, to suppose that, in markets, consumer preferences and, in science, the 'truth' – or more modestly phrased: the compatibility with the facts – work as effective ultimate selectors, even if the competitive constraints that producers and scientists face in their proximate environments may be at odds with an economic constitution that promotes consumer sovereignty or an epistemically efficient constitution of science.

Producers in markets may seek to restrain competition and to exploit consumers by cartel agreements, and governments may grant protectionist privileges to particular industries at the expense of consumers, but man's ineradicable interest in finding access to the most attractive sources of supply will be a powerful force that incessantly works to circumvent, to erode and to overcome the barriers that are put in their way. The principles of selection that prevail in research institutes and universities, the constraints that national rules and regulations define for scientific work, and conventions that come to prevail in professions such as economics may be dysfunctional, but man's ineradicable interest in knowing how the world around him works will be an incessant force that tends to select in favor of more informative theories, and stubborn reality will be an inescapable ultimate selector between conjectures that are compatible with the facts and those that are not.[48] Globally and in the long run the capacity of markets to serve consumer interests and the capacity of science to advance the growth of knowledge appear to be quite robust, even though the constitutional requirements for their flourishing may be unevenly met in different locations and at different times.

It is this combination of local vulnerability and global robustness that Hans Albert (2004: 38; 2006: 126f.) points out as common feature of markets and of

[48] Leonard (2002: 146): "The criteria of validity of claims to scientific knowledge are not matters of national taste and culture. Sooner or later, competing claims to validity are settled by the universalistic facts of nature which are consonant with one and not with another theory."

science when he notes that a government may well undermine the stability of a national financial system and cause a national crisis by misguided regulations, but that this does not prevent international financial markets from functioning, and that, likewise, a government may well ruin the scientific institutions of a country by unsuitable legislation, but that this does not prevent the international system of science from functioning. As a global system science can be trusted to advance the growth of knowledge, deficiencies in local constitutions of science notwithstanding. As Jarvie (2002: 230) puts it: “The authority of science, unlike the authority of other institutions, is not bounded by national jurisdiction. What is truth on this side of the Pyrenees remains truth on the other side.”

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