

# Gordon Tullock's *The Organization of Inquiry*: A critical appraisal

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**Abstract** The major themes of Gordon Tullock's 1996 book *The Organization of Inquiry* are identified. Tullock's treatment of the philosophy of science is criticized, as is his explanation for the backwardness of the social sciences relative to the natural sciences. The paper closes with a listing of some of Tullock's proposals for the reform of science.

**Keywords** Natural science · Social science · Knowledge · Scientific inquiry · Applied research · Pure research · Induced research · Philosophy of science · Theory appraisal · Spontaneous orders · Gordon Tullock · Karl Popper · F.A. Hayek

My goal in this paper is to provide a critical appraisal of Gordon Tullock's *The Organization of Inquiry* ([1966] 2005).<sup>1</sup> Unfortunately for me, though it is an enjoyable and stimulating book to read, it is also a difficult one to assess. Tullock's stated aim is simple enough: to examine science as a social system. He claims in his preface that his basic orientation will be economic rather than sociological, but in truth it combines elements of both.<sup>2</sup> It also

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<sup>1</sup>This paper was prepared for a conference held March 7–9, 2007 and sponsored by the Fund for the Study of Spontaneous Orders at the Atlas Economic Research Foundation, honoring Gordon Tullock's many contributions. I greatly benefited from comments by conference participants, and in particular those of Peter Boettke. Tullock's book was originally published in 1966 by Duke University Press, but all references here are to volume 3 of *The Selected Works of Gordon Tullock*, published by the Liberty Fund in 2005. (References that simply list page numbers are to this volume.) Liberty Fund is to be commended for bringing out a new printing, not least because the Duke University Press edition contained maddening end-of-line word-break hyphens that slanted upwards at a forty-five degree angle, a convention that was a great distraction for this reader!

<sup>2</sup>I say "sociological" because Tullock allows different "ideal types" of agents (those pursuing pure, applied, or induced research) who have different types of motives. For an example of what I would consider a more strictly economic approach to a related subject, see McKenzie (1979). Some philosophers of science have begun to employ economic models to analyse the social organization of science; for more on this literature, see Hands (2001, chapter 8).

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includes elements of the philosophy of science, especially in a chapter on the subject and methods of inquiry. Here Tullock draws mostly on the ideas of the philosopher Karl Popper, with whom he worked for about six months in the late 1950s and whom he thanks in his preface.<sup>3</sup> The book is partly descriptive and partly prescriptive, and these aspects are often mixed together in a given chapter. Though Tullock focuses primarily on the natural sciences, there is also a chapter explaining why the social sciences are so backward in comparison. It is a southern book: Tullock develops his arguments leisurely, with frequent diversions onto minor topics of particular interest. In supporting or illustrating his arguments, he often cites from an intimidatingly diverse set of articles and books, from philosophical tomes to general science sources to specific field journals. In sum, *The Organization of Inquiry* is an interdisciplinary work that amply displays the eclectic, idiosyncratic, and polymathic virtuosity for which Tullock is well known. These virtues make it, as I have said, an enjoyable read, but they impose costs on any reviewer whose reach is not so extensive as is Tullock's, or in short, they impose costs on any reviewer.

As such, my comments will only touch on a subset of the ideas presented in the book. After providing a brief synopsis of his major themes, I will first offer a criticism of Tullock's treatment of certain issues in the philosophy of science. Next, I will assess and provide a somewhat Hayekian response to his explanation of why the social sciences are backward relative to the natural sciences. I will close with a listing of some of the more intriguing proposals for reform that Tullock sprinkles throughout the book.

## 1 *The Organization of Inquiry: An overview*

Though he does not use the term, Tullock begins his book by describing science as a type of spontaneous order, comparable to the economic order described by Adam Smith in *The Wealth of Nations* (pp. 4–5). Scientific research is pursued by a multitude of widely dispersed individuals, each pursuing his or her own particular problem. The scientific community is widely dispersed; it is truly a world community. There is no central coordinating organization, yet each person's individual contribution feeds into the research of countless others. Most important, this unplanned process somehow regularly, even systematically, results in the discovery and accumulation of knowledge. These observations lead Tullock to his major objective: to explain why science “is such a successful social instrumentality—to explain 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” (pp. 6–7).

Tullock tackles the question by identifying the reasons why people engage in scientific inquiry. He says there are three. First, there is simple curiosity, the need to understand—this leads to pure research. Next, there is the desire to control—this leads to applied research. Finally, there is a third category, the wonderfully titled “induced curiosity,” which arises because many academics

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<sup>3</sup>“The genesis of this book was a period of about six months spent working with Karl Popper. At the time I had no intention of writing a book on science, and my studies were devoted to an entirely different problem; nevertheless Popper's approach necessarily rubbed off on me . . .” (p. xix). In the preface he also cites Michael Polanyi as an influence. In a phone conversation Tullock told me that while he was working with Popper he also checked over the English in Popper's manuscript for *The Logic of Scientific Discovery* (1959), which was a translation of Popper's earlier book, *Logik der Forschung*.

... do research and produce articles simply because this is the way they earn their living. They may actually have very little interest in the subject of their investigations and will abandon their researches without a single pang of regret if they are offered a better paying job doing something else (p. 24).

Tullock acknowledges that the categories are ideal types, that the motivation of most real-life researchers lies somewhere between the various extremes he posits.

The three motives that drive scientific inquiry also hold the secret to the success of science as a mechanism for the discovery of knowledge. Pure researchers by definition are driven by their curiosity to seek the truth. Applied researchers also have an incentive to seek the truth, because if the practical applications they seek fail to work, they reap no benefits. Furthermore, the interaction between pure and applied research also serves as a check on whether a putative addition to knowledge has actually been made: attempts to apply the findings of pure research may lead to their rejection, or modification, or even to the discovery of new relationships.

Incentive alignment is much less clear when it comes to research by scientists whose curiosity must be induced. However, even these plodding individuals are held to the pursuit of truth, because there are severe penalties for falsifying findings—their reputations, and thereby their ability to earn a living, could suffer irreparable harm should they stray too far from the straight and narrow.

As a result, with no central organization, scientific research in the natural sciences nonetheless generally leads to the expansion of knowledge. There are some exceptional circumstances when this is less likely to be true. For example, if some pure research has no applications whatsoever, the check from the applied side no longer operates, so specious results may hang around longer. Tullock also notes that when too many researchers crowd a field, there is an incentive to try to distinguish one's work by utilizing overly-sophisticated techniques or obfuscating jargon. Finally, in fields in which it is either necessary to uphold a particular point of view, or "if it is doubted whether anything approximating 'truth' is really existent in any field," then "the standards to which an induced researcher must conform may be deplorably low" (p. 48). Perhaps not surprisingly, Tullock suggests that this last problem is much in evidence in the social sciences.

The actual process by which theories or discoveries are tested is the subject of chapter 6, titled "Verification and Dissemination."<sup>4</sup> The first step is to test a hypothesis thoroughly, a process which involves thinking carefully about the hypothesis, gathering data relevant to its testing, modifying it in light of the data, and repeating these steps as many times as is necessary. Once a scientist is sufficiently confident that a hypothesis is correct, the process of dissemination begins. For applied research, the next step is to seek a patent. Once that property right has been obtained, the new innovation is presented in trade journals or even through advertising. Pure research is disseminated through presentations at conferences and attempted publication in academic journals. Journal editors evidently play a very important role in determining which pure research gets published, and Tullock spends quite a few pages examining the pluses and minuses of various ways of organizing journal editorship as a social institution. Once an article gets published, the verification process begins all over again, as other researchers try to replicate or challenge the original results.

<sup>4</sup>It is a bit surprising that Tullock uses the language of verification: given the Popperian influence, one would have thought he would have used the term falsification. However, in his discussion of the process, Tullock defines verification as "a handy term for the results of investigations which fail to falsify a given hypothesis" (p. 111), so he is true to the Popperian formulation. And verification is, of course, the more common term in use among scientists.

Tullock notes that debates among scientists about their respective theories can be as intense, emotional, and even ugly as are political battles, with advocates misrepresenting the findings of others, engaging in ad hominem attacks, becoming irrationally attached to their own theories, and so on. Furthermore, scientists often have motivations other than the three that he identifies driving their work. What keeps the scientific process working so well in the natural sciences are its system constraints, the multiple feedback mechanisms that act as checks on participant behavior. The interaction of applied and pure research, the ability of other scientists to detect fraud through the process of replication, and the severe sanctions that members of the community of science attach to fraudulent behavior, all serve to keep natural scientists focused on the discovery of knowledge. As we will see, for a variety of reasons these system constraints work less well in the social sciences.

In short, in his little book Tullock well describes the spontaneous order that exists in the natural and applied sciences, and shows how, and why, it works. Many people who invoke spontaneous orders do not show how and why they work, and this has led critics to be rightly suspicious of their often casual invocation, so Tullock's contribution is an important one. The burden of proof now shifts to the critic, who must show either that science is not so orderly as Tullock describes, or that if it is orderly, that Tullock has not correctly identified the reasons why.<sup>5</sup>

For Karl Popper, whom Tullock frequently invokes, criticism was not only the highest form of praise, it was also the key to the advancement of knowledge. As such I am delighted to offer two criticisms of Tullock's argument, one concerning his treatment of the philosophy of science, the other of his analysis of the problems of the social sciences.

## 2 Tullock on the philosophy of science

The most overtly philosophical chapter of the book is chapter 3, "The Subject and Methods of Inquiry".<sup>6</sup> It is also the chapter in which Karl Popper's influence is most evident. Though he does not use these terms, Tullock begins with a defense of a realist over an instrumentalist or conventionalist approach to the status of scientific hypotheses.<sup>7</sup> He notes that most scientists believe that they are seeking true laws about how the world works. This makes much more sense than do accounts that claim that scientific theories are either summaries of observations or mnemonic devices for capturing certain apparent empirical regularities. If scientists were seeking simple summaries rather than true relationships, he asks, why did Einstein's theories supercede the simpler Newtonian approach? If scientists sometimes mouth the instrumentalist or conventionalist lines about their own behavior, it is probably because they recognize that even well-established results may always someday be overturned. Good scientists develop a healthy skepticism about their theories; and given this skepticism

<sup>5</sup>One possible line of criticism is to note that not all areas of the natural sciences fit the model Tullock offers. Evolutionary theory, for example, seems to be a possible exception.

<sup>6</sup>Chapter 5, "The Problem of Induction," might seem like another candidate, but there Tullock simply uses Hume's problem of induction as a jumping off point for a discussion of the origin of new hypotheses in the minds of individual scientific investigators. His basic argument is that inductive reasoning and pattern detection play a role in the emergence of new ideas. At one point, in discussing how "clustering" of information shapes how scientists view a problem, he seems to describe himself: "This phenomenon, at the same time, explains why sometimes an outsider, or a man just learning a new field, will discover things which have escaped all the experts. . . . A new mind will, of necessity, have a fresh approach and is unlikely to develop exactly the same clustering of ideas as the older experts have" (pp. 103–104).

<sup>7</sup>Popper endorsed the former and criticized the latter two views.

“the development of a ‘theory of theories’ which simply denies the validity of theories as anything other than a simplified set of observations is not an unexpected result” (p. 37).

Tullock also endorses Popper’s fallibilism, though again the specific term is not used. Fallibilism asserts that though we seek the truth, we never can know that we have found it, even when we have. Now, given that “the progress of science consists of developing ever newer theories which approach ever closer to the truth” (p. 40), if one accepts fallibilism, one must figure out how to distinguish at least potentially true theories from false ones. For an applied scientist, this is less of a problem: the pre-eminent criterion here is workability, though it is a necessary rather than a sufficient one. Typically, though, what most scientists rely upon is the consensus of the informed. Tullock recognizes that this answer begs the question of how the informed come to that consensus. His next step is to list a few fundamental criteria of theory appraisal: preferred theories fit the world better, are more general, and are simpler. Tullock notes that an underlying metaphysical assumption for this choice of criteria is that the universe is logically ordered and comprehensible.

The final question Tullock addresses is how to distinguish science from other types of inquiry. He states up front that he would like a definition that would allow fields like physics and economics, which have “fairly elaborate theoretical structures,” to be included (p. 49). He dismisses the idea that it is the scientific method that distinguishes science from non-science, and also finds doubtful that a basic difference is that the scientist seeks “general” rather than “particular” truths. For Tullock, it is the organization of inquiry within the scientific community that is key: “it is not anything special about the individual scientist, or his work, which distinguishes him, but the special human environment in which he operates” (p. 51). This environment shapes the behavior of those who participate in it; those who conform are doing science.

I think that readers who know of developments in the philosophy of science and in related fields like the methodology of economics since the 1960s may have difficulties with at least some of Tullock’s arguments in this chapter. Part of the problem is simply that he was writing in the early 1960s. Logical empiricism, sometimes dubbed “the Received View,” was still the dominant approach at the time, and to some extent Tullock’s writings reflect that tradition, albeit as seen through the eyes of someone who had studied with Popper. Though Tullock mentions that he had read Thomas Kuhn’s ([1962] 1970) book, *The Structure of Scientific Revolutions*, it does not inform his work. The dissolution of “the Received View” as documented in Frederick Suppe’s Introduction to *The Structure of Scientific Theories* (1977) had not yet occurred, nor was it evident in 1966 that by 1977 it would make sense to title the opening section of the Afterword to Suppe’s volume “Swan Song for Positivism.” Kuhn’s work and that of the growth of knowledge philosophers transformed the philosophy of science, and further transformations later took place as philosophy took the naturalistic turn and as alternatives to the philosophy of science demanded room at the table. Were Tullock to do a second edition of his book, he would at a minimum need to respond to the many new developments in philosophy and related fields.<sup>8</sup>

But there is more. As noted in the introduction, one of the virtues of Tullock’s work is that he reads widely and integrates ideas from many diverse fields, which often leads him to look at problems in new and different ways. Sometimes, though, these new and different responses ignore or sideline problems that are viewed as fundamental by practitioners in the original field.

Let me be clear here—in voicing these criticisms I am not saying that everything that Tullock has written here has been either superseded or is not on point. I think he is right, for

<sup>8</sup>For more on these literatures, see Hands (2001).

example, that most working scientists would embrace some form of common-sense realism if asked about what they do, or for another example, that many philosophers endorse some variant of fallibilism.

But there are also identifiable problems. Rather than to continue with these generalities, I will provide some examples.

a. *The Progress of Science*: Tullock's account implicitly accepts the positivist vision of science as cumulatively progressive: through the toil of many independent researchers, scientific knowledge is slowly but surely built up over time. This is consistent with what Thomas Kuhn called normal science, in which scientists busy themselves solving puzzles within a given paradigm, and in which methods of theory assessment are widely understood and unquestioned. But Kuhn went on to argue that such puzzle-solving eventually leads to the discovery of anomalies, and that these ultimately lead to revolutionary science, in which a new paradigm emerges that overcomes the anomalies and vies to replace the old one. New paradigms provide different world-views, dictate different meanings for terms, and even affect the selection of data for testing, since facts themselves are theory-dependent. All this implies that competing paradigms are frequently incommensurable, with each side pointing to its own set of tests to support its theories. In opposition to the positivist assumption that science is cumulatively progressive, Kuhn resisted even using the word "progress" to describe the history of science, preferring instead the word "evolution" (Kuhn [1962] 1970, chapters 4–7, 13). None of this is dealt with in Tullock's account.

b. *Truth*: Tullock thinks that trying to define the word "truth" is a false problem: "I do not believe that anyone really has any difficulty understanding what this word means" (pp. 38–39). I think that he is right that most people simply accept some version of what philosophers term the correspondence theory of truth—a statement is true if it corresponds to the facts. An alternative is the coherence theory of truth—a statement which coheres with a widely accepted set of other statements is viewed as true. Tullock's suggestion that we look to the consensus of the informed when deciding which theories are true certainly seems to suggest a role for this second theory.<sup>9</sup> And his decision not to define "truth" leaves the question open.

c. *Theory-Appraisal*: Tullock suggests that the way that the scientific community comes to a consensus is by the application of criteria of theory appraisal. His account ignores a central problem within the philosophy of science, the fact that often it is not a single theory that is being assessed, but competing theories. He fails to ask, how does one appraise competing theories? By missing this issue, Tullock also fails to address what is probably the central finding of post-positivist philosophy of science, that there are no universal criteria of theory choice that can be invoked to accomplish this task. That was of course implied in Kuhn's account, where, for example, empirical evidence to judge competing paradigms are themselves theory-infected. Paul K. Feyerabend took an even more extreme position. For Feyerabend, there are no hard and fast rules that may be applied to choose among theories. Furthermore, theory-choice itself may be viewed as inimical to the progress of science, because given the theory-dependence of facts, the elimination of theories reduces empirical content! Even if one does not embrace Feyerabend's methodological anarchism, it is evident that there are many criteria of theory-choice (leaving aside empirical ones, some of the most

<sup>9</sup>For what it is worth, I think that Uskali Mäki's (1993) proposal that we accept a correspondence theory of truth, but a coherence theory of justification, solves this particular problem very nicely. Truth is defined in terms of correspondence with facts, but when we try to convince others of the truth of our theories (when we try to justify them), we use rhetoric and persuasion, we try to show how our theory fits in (coheres) with what our listeners already believe.

important include logical consistency, elegance, extensibility, generality, theoretical support, simplicity, realism, explanatory power, heuristic value, and fruitfulness), that the definition of each of these criteria is subject to debate, and that typically when two theories compete, one will meet certain criteria, and the other will meet others. Without a rank ordering of criteria of appraisal, what is one to do?<sup>10</sup>

It is hard to underestimate the importance of these findings for the development of the philosophy of science in the second half of the twentieth century. Most fundamentally, they have caused the movement away from a normative to a descriptive approach. And this has had an impact on the methodological writings of scientists in specific disciplines. Within economics, for example, the rhetoric of economics movement took as a starting premise that a rules-based methodology (Methodology with a big M) was misguided, that all scientific argumentation is simply a form of rhetoric (McCloskey 1985). The new climate is captured by Wade Hands, who titled his recent masterful survey of contemporary thought in science studies and its impact on economic methodology *Reflection without Rules*.

d. Demarcating Science from Non-Science: Tullock briefly discusses what philosophers call the demarcation problem, how to distinguish science from non-science. Like most people, Tullock feels that he knows science when he sees it. He wants to include physics and economics because they are fields whose theories have fairly elaborate theoretical structures. His basic criterion is membership in a scientific community. Others who have thought about the problem typically add that a science should make falsifiable predictions about the world, some of which are confirmed. Some might add that people outside of the specific community recognize it as such, and believe in its findings.

Though it might seem easy to come up with a list of such criteria, it turns out that the demarcation problem is another one of those thorny, seemingly unanswerable questions in the philosophy of science. Let's assume that we would not want to count astrology as a science, could we exclude it given the criteria enumerated above? Note that astrology has an intricate theoretical structure, one so impenetrable that one must hire an expert to construct one's chart. Note further that there is indeed a community of such experts, all of whom make predictions, some of which are confirmed. Indeed, some astrologists have better records at prediction than do, say, meteorologists, or economists. And when it comes to being "believed in" by the wider community, more people probably believe in astrology than do in economics—think of Ronald Reagan taking months to replace the chair of his Council of Economic Advisors, all while Nancy was arranging his schedule according to her understanding of planetary alignments.

e. Scientific Method and Criticism: Tullock rejects distinguishing science from non-science according to method. His argument, though, is strange, because it is based on the fact that "there is no evidence that the brains of scientists work differently from those of other men" (p. 50). This does not seem to me to be relevant: artists and scientists may have similar brain structures, but still follow different methods.

In any event, in claiming that it is not the scientific method that separates science from non-science, Tullock is deviating substantially from many twentieth century philosophers, including Popper.<sup>11</sup> For Popper, the scientific method consists in subjecting theories to se-

<sup>10</sup>For a more detailed presentation of the arguments, see Caldwell ([1982] 1994), especially chapters 12 and 13.

<sup>11</sup>I include here only those philosophers who believe that science should be distinguished from non-science. This would not include those who hold, for example, that there is no substantive difference between, say, doing a chemistry experiment and explicating a literary text, that all such fields involve the use of rhetoric and persuasion using norms accepted by the relevant community.



vere tests, and to avoiding ad hoc theory changes (he also called these immunizing strategies) meant simply to preserve a theory when it fails a test. Given his acknowledged debt to Popper, it is notable that Tullock did not invoke that philosopher's own demarcation criterion here.

It may be that Tullock's views are not, however, in reality so far from Popper's. If one focuses on Popper's writings on critical rationalism rather than on his invocations of falsifiability and testing, one can see many similarities in their positions.<sup>12</sup> For both men, willingness to criticize, to subject one's own views and those of others to repeated scrutiny, is a key component of the scientific attitude. And for each of them it is the scientific community that enforces the critical attitude. Popper got at this by emphasizing what he called the "inter-subjective" nature of testing. Tullock goes beyond this by identifying the feedback mechanisms in successful scientific enterprises that reinforce the critical attitude. His descriptive work thus nicely complements Popper's normative recommendations. Indeed, it puts some meat on the bones of Popper's proposals.

It is, of course, grossly unfair to criticize an author for failing to anticipate and respond to arguments that had not been made when he was writing. Furthermore, I am not making the claim that Tullock's position could not be defended, even in the light of more recent developments. My point is simply that given the magnitude of the changes that have taken place, this particular chapter of *The Organization of Inquiry* is dated. Whether that has any effect on the cogency of his overall thesis is certainly an important matter for further discussion and debate.

### 3 The backwardness of the social sciences

Tullock notes that explanations for why the social sciences are "deficient" when compared with the natural sciences generally fall into one of two categories: "those which allege that the subject itself is particularly difficult and those which point to various features of the social environment which makes research in the social sciences hard" (p. 135). He is skeptical about the first set of arguments, so begins by rebutting some of them.

Some of the arguments he rebuts strike me as straw men: I have never heard anyone assert, for example, that the problems that the physical sciences tackle are easy (pp. 135–136), and I doubt that anyone today believes that experimentation is impossible in the social sciences. For the more important claim that one cannot apply the methods of the natural sciences in the social sciences, Tullock's response is pretty weak: he states that the general method of the sciences, which is simply "to think hard about problems and collect data," is applicable to any science (p. 136). No one, of course, would question this, but they might well question whether the sort of falsificationist philosophy propounded by followers of Popper, or the method of positive economics put forward by Friedman, are appropriate ways to add meat to the bare bones advice of "thinking hard and collecting data." For those who think the social sciences are more difficult because they deal with human beings, which gives rise to special problems, Tullock simply answers that all fields have special problems that they have to solve (p. 137). This reply ignores a host of issues dear to the heart of hermeneuticists and others: matter does not think, but humans do; social scientists interpret the interpretations of other humans, while natural scientists do not; the sort of data that is of interest to social scientists may be different in kind from those of the natural sciences; and so on.

<sup>12</sup>See Caldwell (1991) for more on the various components of Popper's views, and their uses by economists.



Tullock next turns to what he considers to be the real reasons behind the backwardness of the social sciences, which in his view is due to differences in the social organization of natural versus social science. The first difference is the relative absence of applied research: because there is no way to patent applied research in the social sciences (He asks, for example, how does one patent a new sales technique?), little of it is done. But this means that, unlike the natural sciences, there are many fewer checks from the applied side on pure social science research (p. 149). Furthermore, the second motive for research, curiosity, is in the social sciences “likely to get distracted to essentially non-scientific ends.” This is because in the social sciences:

... there is a strong possibility of artistic distraction. Literature of all kinds is quite frequently based on careful observation of human beings. A large number of brilliant men led by their curiosity to study their fellow men have produced great literature instead of science (p. 151).

What is left is induced research, which is the type that is most likely to be affected by the prevailing climate of opinion. Unfortunately, this climate is often unfavorable for research in the social sciences. Unlike the natural sciences, which typically study uncontroversial (or when controversial, obscure) topics, the social sciences often study subjects about which the public and, more to the point, directors of funding agencies, have strong ideological or moral beliefs. If a social scientist comes up with a finding that is not in accordance with generally held views, he could at a minimum expect to face the moral disapprobation of his peers. In more extreme cases, he could lose his funding, fail to be published, and perhaps lose his job. As a result, induced researchers tend to avoid dangerous issues and to confirm the views of directors of funding agencies. Another difference is that in the natural sciences, a minority can convince a majority of the truth of its views, simply by demonstrating that a particular innovation works. To even try an innovation, a social scientist would in a democracy need to convince a majority to do so. In sum, the system constraints that lead natural scientists to seek the truth are less operative in the social sciences, and furthermore, there also exist in the social sciences (because of interests, ideology, and moral concerns) factors which may make it more difficult to seek the truth.

Some of what Tullock says here is clearly true: social scientists often do investigate controversial topics, and there can be real pressure applied on those whose views do not accord with those of polite society, as such figures as Charles Murray and Lawrence Summers have discovered. And there are admittedly fewer opportunities for social experiments comparable to trying out an innovation in the social sciences relative to the natural sciences.

That said, crucial experiments are often difficult in the natural sciences, too. But much more significant, I am unwilling to accept Tullock’s assertion that the curiosity motive is less strong in the social sciences because those with curiosity about the human condition tend to end up doing art or literature. Furthermore, if one thinks of social policy as the social science equivalent to applied research in the physical sciences, it seems clear that, even without a patent system, lots of applied research gets done, and that its results can feed back on pure research. The collapse of the east bloc, for example, had a pretty devastating effect on both pure and applied theories of planning.

There is an alternative to Tullock’s explanation for the relative backwardness of the social sciences, one that might draw on some insights provided by F.A. Hayek. Unlike Tullock, Hayek thought that the difficulties that exist arise from the subject matter of the social sciences, or more precisely, they arise whenever one studies complex, rather than simple, phenomena. For sciences that study complex phenomena, prediction is difficult. Often the best we can do is to provide pattern predictions, or explanations of the principle by which

some phenomenon of interest is produced. This implies that it is hard to get clean test results, and therefore it is hard to distinguish legitimate theories from their spurious rivals.

Tullock is right that interests, moral arguments, and ideology often infect social science pronouncements, but this is due to the fact that it is so much harder to establish on scientific grounds which theories are correct. Crucially, this is not the sort of limitation that will be erased as such sciences mature. Hayek put it this way:

But if it is true that in subjects of great complexity we must rely to a large extent on such mere explanations of the principle, we must not overlook some disadvantages connected with this technique. Because such theories are difficult to disprove, the elimination of inferior rival theories will be a slow affair, bound up closely with the argumentative skill and persuasiveness of those who employ them. There can be no crucial experiments which decide between them. There will be opportunities for grave abuses: possibilities for pretentious, over-elaborate theories which no simple test but only the good sense of those equally competent in the field can refute. There will be no safeguards even against sheer quackery. Constant awareness of these dangers is probably the only effective precaution. But it does not help to hold up against this the example of other sciences where the situation is different. It is not because of a failure to follow better counsel, but because of the refractory nature of certain subjects that these difficulties arise. There is no basis for the contention that they are due to the immaturity of the sciences concerned. It would be a complete misunderstanding of the argument of this essay to think that it deals with a provisional and transitory state of the progress of those sciences which they are bound to overcome sooner or later (Hayek [1955] 1967, p. 19).

I have done little more here than simply to note that Hayek's writings about complex phenomena provide an alternative to Tullock's account of why the social sciences are backwards.<sup>13</sup> Hayek's explanation is of the type that Tullock presumably rejects: one that asserts that the social sciences are more backward due to the difficulty of the subject matter it studies.<sup>14</sup> Assessing the relative merits of these and other explanations of the differences between the natural and social sciences is evidently a topic worthy of further study.

#### 4 Proposals for reform

*The Organization of Inquiry* is filled with proposals for reforming the practice of science. All aim at strengthening the feedback mechanisms that produce the "successful social instrumentality" of science, and each recommendation could stimulate an extended discussion. Rather than commenting on them (although I do have strong feelings about some of them!), I will conclude this paper by simply listing some of ones that I found most interesting.

a. Improving the Quality of Journal Editors—Tullock laments that too often when prominent academic scientists reach the non-creative stage of their careers, they go into "administrative work." Meanwhile, journal editors are often "respected but ordinary" workers in a

<sup>13</sup>For a more detailed account of Hayek's position, and the argument that it helps to explain the development of economics in the twentieth century, see the Epilogue to Caldwell (2004).

<sup>14</sup>Actually, Hayek thought that many natural sciences also study complex phenomena. Indeed, he advocated in his later work distinguishing sciences using the simple-complex dichotomy rather than the natural-social science dichotomy.

field (p. 116). Tullock thinks that foundations should put more money into elevating the pay of journal editors, which would attract more leading figures to do this essential work.

b. Improving the Refereeing Process—Tullock feels that a blind refereeing process reduces the incentives for both the general editor of a journal and for the referees to do a careful job. He recommends in its stead that general editors of journals be assisted by a board of editors each of whom is competent in some area of the field covered by the journal. Papers in a certain area would then be refereed by the appropriate board member. He notes that “the pressure on members of the board to review contributions carefully would be increased if the responsibility of each member for a given substantive category were specifically and publicly spelled out, and if the acceptance or rejection of any given article were clearly allocated to the specialist responsible rather than to the chief editor” (pp. 117–118).

c. Using Prizes to Induce Research—Tullock points out that, if some principal is interested in having a particular line of research pursued, typically he simply hires scientists to do so. This has the disadvantage that the principal’s “efficiency as a personnel manager and the various chance factors which always affect the hiring of individuals will be reflected in the results” (p. 29). Tullock recommends an alternative prize system, one that is only occasionally used (as in the granting of the Nobel Prize), which might have better results. He summarizes the advantages of a prize system succinctly: “Advertising a prize and letting anyone who wishes make investigations in that field will normally lead to a sort of self-selection by a very wide group of people, and only those who think themselves specially qualified will make the attempt” (ibid).

d. Choosing the Recipients of Grants—Researchers should be awarded grants based not on their ability to produce “a convincing brochure” (that is, a convincing grant proposal) but on the results of their past work. Indeed, Tullock would eliminate altogether the proposal phase:

Research workers who have had success in the past should simply be given funds to spend on what they wish, with the understanding that future funds will depend on the results they obtain. This procedure would largely eliminate the present waste of time on preparing projects and would permit scientists to concentrate on their real work (pp. 166–167).

Tullock recognizes that this system would make it much harder for a newcomer to get a grant, but he notes that is true under the present system as well. His solution is to have the prominent scientists who receive grants take on the additional role of recommending newcomers.<sup>15</sup> Again, they would continue to be asked only so long as the newcomers were themselves successful at achieving results.

e. Separating Teaching from Research—Tullock notes that many good university teachers do research only because they are required to do so, and similarly, many good researchers are mediocre teachers, but are forced to continue by their institutions. Greater specialization would improve both teaching and research without having to increase the resources going to the two activities (p. 171).

f. Abolishing Tenure—Tullock notes that the usual two rationales for the granting of tenure, that it protects minority opinions and that it saves universities money, are questionable. To the extent that many professors reduce their productivity once tenure has been

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<sup>15</sup>Both this proposal and the proposal to replace blind refereeing with refereeing by a single named member of an editorial board seem to me to place too much power in too few hands. However, Jack Sommer’s (n.d.) proposal for a combination prize-lottery system for choosing grant recipients would help to mitigate this problem.

granted, the saving of money that tenure allows may only be a short run phenomenon. As for protecting minority opinion, Tullock notes that tenure “applies to the wrong period of life” (p. 174). People have their most radical ideas when they are young. In most cases by the time tenure is granted, those who have gotten it have spent much of their adult lives getting good grades, pleasing superiors, and conforming to the party line. Furthermore, though tenure might keep a university president from having to fire a controversial faculty member, university administrators and faculty also recognize that state legislatures and individual and foundation donors still have the ability to reduce their allocations should academics get too far out of line. The major protection that tenure offers is to keep department heads from firing senior faculty members. Whether this is good or bad depends on individual circumstances, though given that many faculty reduce their productivity once tenure is granted, there would doubtless be many cases in which its abolishment would on net be positive.

g. Financing Information Retrieval—Tullock devotes an entire chapter to data collection, and in his chapter on the dissemination of research findings he highlights the important role of “information retrieval”—methods to make it easier for scientists to access new information. He talks about the problems of improving classification systems, of cross-indexing, of finding ways to allow information in a variety of fields that are unrelated to be combined (pp. 77–85). In his final chapter one of his major recommendations is that large funding organizations should appropriately fund very expensive projects (like building atom-smashers), but beyond that, they “should confine themselves to cataloguing and indexing knowledge” (p. 165). He even mentions computers: “The possibility of using computers several orders of magnitude larger than any now contemplated to ‘search’ the whole body of knowledge for specified information does exist, but is not for the immediate future” (p. 85).

This one I will comment on. Gordon, meet Google: this is one problem that seems to have been solved. And isn’t it mete that yet another spontaneous order, that of the Internet, provided the means for solving it?

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