THE SELECTED WORKS OF GORDON TULLOCK

VOLUME 3

The Organization of Inquiry

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VOLUME 1 *Virginia Political Economy*

VOLUME 2 *The Calculus of Consent: Logical Foundations of Constitutional Democracy* (with James M. Buchanan)

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The Organization of Inquiry

GORDON TULLOCK

Edited and with an Introduction by

CHARLES K. ROWLEY

Liberty Fund

Indianapolis

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Printed in the United States of America

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Frontispiece courtesy of Center for Study of Public Choice,
George Mason University, Fairfax, Virginia

09 08 07 06 05 C 5 4 3 2 1
09 08 07 06 05 P 5 4 3 2 1

Library of Congress Cataloging-in-Publication Data
Tullock, Gordon.
The organization of inquiry / Gordon Tullock ; edited and
with an introduction by Charles K. Rowley.
p. cm.—(The selected works of Gordon Tullock ; v. 3)
Originally published: Durham, N.C. : Duke University Press,
1966.
Includes bibliographical references and index.
ISBN 0-86597-522-1 (alk. paper)—ISBN 0-86597-533-7 (pbk.)
1. Research. 2. Research—Methodology. 3. Science—
Social aspects. I. Rowley, Charles Kershaw. II. Title.
Q180.A1T78 2005
001.4′2—dc22

2003065991

LIBERTY FUND, INC.
8335 Allison Pointe Trail, Suite 300
Indianapolis, Indiana 46250-1684

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INTRODUCTION

Gordon Tullock wrote *The Organization of Inquiry*[1] during the mid-1960s, probably the most productive decade of his career. From a purely technical perspective, this book stands out as his best-written single-authored work. The book sets out his own views on scientific method—views that he would faithfully reflect in his subsequent scholarship.

Early Methodological Influences

Because Tullock is a largely self-taught economist, his exposure to scientific method came not from the economics classroom but from his legal training, his reading, and his direct association with two leading scholars, namely Karl Popper and James M. Buchanan. The first part of this introduction traces these intellectual influences that helped to shape the book.

Let me begin, as Tullock surely did, with his legal training at the University of Chicago. Although Chicago during the 1940s was less wedded to black-letter law than were most of its rivals, the methods of the natural and social sciences were minimal elements of the curriculum. The primary focus of Chicago legal training, at that time, was inductive rather than deductive in nature and was based on a detailed evaluation of "binding legal authority" derived from a limited number of legal precedents.

Chicago, in conformity with all other leading schools of law, trained lawyers to seek out the "universal truth" of the law through a careful selection of a number of singular statements encompassed in the written judgments of the higher courts. They were right to do so, since this is the thrust of precedent and *stare decisis* in the Anglo-Saxon legal system. However, the movement from singular to universal statements (induction) was already, following David Hume,[2] anathema to the approach endorsed by almost all economists (deduction).[3]

1. Gordon Tullock, *The Organization of Inquiry* (Durham, N.C.: Duke University Press, 1966).

2. David Hume, *An Enquiry concerning Human Understanding* (1777) (La Salle, Ill.: Open Court, 1907).

3. Mark Blaug, *Economic Theory in Retrospect* (Cambridge: Cambridge University Press, 1997), 690.

In pursuit of the inductive approach, lawyers are trained to move from the observation of facts to the formulation of theory, something that runs directly counter to the approach recommended by Karl Popper. Furthermore, because they are typically concerned with the detailed facts surrounding a particular case, lawyers are inclined to be skeptical of the model-building approach of economics, and especially of the generalizations that economists derive from such models.

Finally, because each case requires an overarching judgment derived from all relevant factual evidence and applicable law, lawyers are especially skeptical of evaluating partial relationships on *ceteris paribus* terms. Thus, any scholar trained in the law will be tempted to approach economics from a perspective that is radically different from that which is reflexively accepted by scholars trained in the natural and social sciences.[4] The degree to which such initial prejudices can be overcome by assiduous reading will become evident later when the discussion turns to Tullock's contribution to scientific method.

The three scholars of scientific method whose writings most influenced Tullock's thinking are Joseph Schumpeter, Karl Popper, and Michael Polanyi.[5] The relevant contributions of each will be reviewed in turn.

Joseph Schumpeter is one of the most highly regarded twentieth-century scholars of the history of economic thought. Schumpeter continually reminds the reader that all scientific theorizing begins with a "vision"—the preanalytic cognitive act that supplies the raw material for the analytic effort. "Analytic effort starts when we have conceived our vision of the set of phenomena that caught our interest, no matter whether this set lies in virgin soil or in land that has been cultivated before."[6] Schumpeter's interpretation of this initial phase of scientific theorizing differs from that of Popper, in the sense that it is less "pure" and potentially more open to ideological interpretation.

> Factual work and "theoretical" work, in an endless relation of give and take, naturally testing one another and setting new tasks for each other, will eventually produce *scientific models,* the provisional joint products of

4. Charles K. Rowley, "Social Sciences and Law: The Relevance of Economic Theories," *Oxford Journal of Legal Studies* 1 (winter 1981): 391–405.

5. Joseph A. Schumpeter, *History of Economic Analysis* (New York: Oxford University Press, 1954); Karl R. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959); and Michael Polanyi, *Personal Knowledge* (Chicago: University of Chicago Press, 1958).

6. Schumpeter, *History of Economic Analysis,* 42.

> their interaction with the surviving elements of the original vision. . . . Now it should be perfectly clear that there is a wide gate for ideology to enter into this process.[7]

Even if one accepts Schumpeter's hypothesis that science is ideological at the outset, that does not imply that the acceptance or rejection of scientific theory is also ideological.[8] If scientists are truly objective in their search for truth, they will falsify or fail to falsify their theories, however devised, by solely nonideological criteria. That is a key insight of Karl Popper.

The point of departure for Popper, in his *The Logic of Scientific Discovery* (*Logik der Forschung,* 1934), concerns the method of basing general statements on accumulated observations of specific instances. This method, known as induction, was the recognized hallmark of science prior to Popper's revolutionary contribution. It was the foundation on which Newtonian physics had long been accepted within the scientific community as the revealed truth of the law of nature.

As early as 1748, David Hume had already raised awkward questions concerning the inductive method,[9] notably by pointing out that no number of singular observational statements, no matter how large, could logically authenticate an unrestrictively general statement.[10] Troubling though this observation was, in the absence of an alternative approach, scientists continued to rely upon inductive reasoning for the better part of two centuries.

Popper's seminal achievement was to provide an acceptable solution to the problem of inductive reasoning. He starts by indicating that there is a logical asymmetry between verification and falsification. In terms of the logic of statements: no number of observations of white swans justifies the universal statement, "All swans are white"; whereas one observation of a black swan justifies the universal statement, "Not all swans are white." In this important *logical* sense, empirical generalizations are conclusively falsifiable, but not conclusively verifiable.

Methodologically, however, it is always possible, because of perceived error, for scientists to reject an observational statement that proves a theory false.

7. Ibid.

8. Bryan Magee, *Philosophy and the Real World: An Introduction to Karl Popper* (La Salle, Ill.: Open Court, 1985), 29–30.

9. David Hume, *A Treatise of Human Nature* (1748) (La Salle, Ill.: Open Court, 1907).

10. Magee, *Philosophy and the Real World,* 15.

Inevitably, scientists may abuse this escape mechanism. Popper therefore suggests, as an article of method, that scientists do not systematically evade refutation, whether by introducing ad hoc hypotheses or ad hoc definitions or by always refusing to accept the reliability of inconvenient observations. Scientists should instead formulate their theories as unambiguously as possible and should expose them as ruthlessly as possible to the test of falsification.

Popper urges that scientists not abandon their theories lightly in response to adverse observations. Instead, they should treat adverse observations as an opportunity to rigorously reexamine their theories. In this sense, Popper is a naive falsificationist in logic but a critical falsificationist in methodology.[11]

Finally, Popper provides a presumptive answer to the issue later raised by Schumpeter concerning the process by which theories are formed. Is this initial step, inductive, based on data observation? Popper's answer is as follows: because it is neither scientifically nor logically significant how a theory is formed, it follows that no method of formulating theory is illegitimate. The process of theory formulation is psychological, not logical.

Tullock spent six months working with Popper at the Center for Advanced Studies at Palo Alto during the mid-1950s. Through this association, Tullock discovered an interest in science that eventually culminated in his writing *The Organization of Inquiry.*

Tullock also acknowledges the influence of Michael Polanyi's *Personal Knowledge.*[12] In the book, Polanyi rejects the ideal of scientific detachment and replaces it with the ideal of personal knowledge, thus recognizing the importance of human behavior in scientific investigation. Polanyi regards knowing as an active comprehension of things known, an action that requires skill. Polanyi cautions, however, that personal participation of the knower in all acts of understanding does not imply that knowledge is subjective. Comprehension is neither an arbitrary act nor a passive experience. It is a responsible act claiming universal validity. Personal knowledge in this sense is an act of commitment, and as such it is inherently hazardous.

Finally, I must briefly mention the insights on scientific method obtained by Tullock through his professional association with James M. Buchanan, namely, the central importance of both microeconomic theory and the self-interest axiom in explaining economic behavior and institutional evolution.

11. Ibid., 18–19.

12. Polanyi, *Personal Knowledge*.

The Organization of Inquiry

In this book Tullock focuses attention on the social organization of science. Each of the book's eight chapters raises important questions about science and provides relevant answers. This introduction offers a brief overview of the book and identifies some of the key insights.

In chapter I, Tullock notes that a gigantic worldwide scientific enterprise exists without any conscious coordination. He poses such questions as the following: How do scientists engage in apparently cooperative contributions in the absence of central planning or hierarchic organization? and Why are scientific contributions worthy for the most part of the public's trust? The answers to these and other related questions form the basis of the remainder of the book.

In chapter II, Tullock explores the various influences that motivate scientific inquiry. Scientists, he argues, undertake investigations either because they are curious or because they hope to use the information obtained for some practical purpose. These two motives, he claims, roughly correspond, respectively, to the general fields of pure and applied research.

Tullock challenges the validity of the then strongly prevailing view, at least in the academy, that pure science is superior to applied science, explaining why the real-world interaction between the two approaches is far more complicated. By focusing only on successful examples of the subsequent practical implications of pure research, all extant studies bias the sample in favor of pure research.

Tullock also challenges the view that pure scientists in some sense are superior, presumably because they are not motivated by financial gain. He notes that most pure scientists are funded through salaries, which suggests that money can, in fact, induce curiosity. He compares the effectiveness of prize monies with the effectiveness of fixed incomes in inducing effective pure research. He also compares the relative effectiveness of journal editors with the relative effectiveness of university administrators in monitoring and ranking the quality of scientific research. In 1966 some of the answers posed by Tullock were revolutionary and deeply upsetting to many in the scientific community. With the passage of time, however, Tullock's speculations on these matters have entered into the scientific mainstream.

In chapter III, Tullock deals with the subject and methods of scientific inquiry. Tullock defines a subject of inquiry as anything that arouses curiosity or that might prove to be practically useful. He acknowledges, but rejects, the views of such skeptics as Bishop Berkeley who argued that there is no proof that the real world corresponds to the sense impressions of those who seek to understand it. Instead Tullock claims that modern scientists firmly believe that there is an objective reality that they are engaged in uncovering.

In pursuing this objective reality, scientists place only limited faith in the truth of the specific theories they promulgate. They are sensible to do so, because the history of science has been the history of disproving specific theories. In this respect, Tullock endorses the falsificationist philosophy of Karl Popper, at least within the field of pure science.

If pure scientists seek the truth, Tullock speculates, applied scientists seek useful information. Therefore, applied scientists can use theories that are known to be false if such theories provide satisfactory solutions to practical problems. This explains why applied scientists continue to use the laws of Newtonian physics to deal with a wide range of human problems long after the theory was disproved by Albert Einstein at the cosmic or quantum level.

With respect to the methods of scientific inquiry, Tullock clearly holds fast to his Popperian training. Out of the infinite universe of possible theories, those that conflict with the evidence are the first to be ruled out. Where no theory survives this test in all aspects of its predictions, the theory that is of the higher order of generality will be preferred. Simplicity is also a rule of importance.

In this process of scientific method, Tullock argues that we gain little by asking which comes first, the hypothesis or the data. The two are often inextricably intertwined. For purposes of his book, Tullock chooses to commence with data collection, which is followed by the formulation of the hypothesis, by further data collection, and finally by the testing of the hypothesis. He claims, following Popper, that the crucial problem of science is not whether the hypothesis is derived according to proper procedure but whether it survives attempts at falsification.

In chapter IV, Tullock focuses on data collection as a major activity that leads to the formulation and testing of scientific hypotheses. Following is a brief discussion of the key elements in the chapter and how they relate to Tullock's personal experience.

Tullock suggests that scientists cannot formulate hypotheses in the absence of data, even if such data are strictly limited to personal observation. As demonstrated in volume 1 of the series, Tullock is archetypical of this approach.[13] He deals in some detail in this chapter with the sources of such data, including the nature of the educational system. He discusses the likelihood that such sources will be accessed by curious pure scientists, induced-curious pure scientists, and applied scientists.

In an era before the information-technology revolution, Tullock's concern, with respect to the testing of scientific theories, focuses on problems of data collection, classification, and dissemination. He notes that the problem is not one of simply accumulating information, relevant or not, but rather one of excluding data that are highly unlikely to be relevant to future scientific work and then of focusing on the careful indexing of the data. In his own work, especially in public choice, Tullock has persistently encouraged scholars to create relevant databases to support empirical testing of important hypotheses.

In chapter V, Tullock directly confronts the problem of induction, which, even now, almost four decades later, is still before the scientific community. Given his initial training in the law, it is significant that Tullock does not even discuss the role of induction in justifying a hypothesis but rather only in establishing the initial hypothesis.

Induction, in Tullock's interpretation, involves the discovery of general principles or patterns in terms of which deductive logic can explain factual data. He illustrates his argument by reference to a number of cases in which an individual perceives patterns in the data that his sensory organs receive. Such flashes of insight, he argues, explain why sometimes an outsider will discover things that have otherwise escaped the experts. Could it be that Tullock is explaining his own behavior as an outside contributor to the scientific process?

In chapter VI, Tullock directs attention to issues concerning the verification and dissemination of scientific results. He confronts directly the possibility that scientists may lie to advance their careers and claims that the high degree of truthfulness in scientific research comes not from the superior moral probity of individual scientists but from the scientific community in which they labor. For reasons that Tullock outlines, this is especially true of the incurious pure scientist, less so of the curious pure scientist and the applied scientist.

13. Gordon Tullock, *Virginia Political Economy,* The Selected Works of Gordon Tullock, ed. Charles K. Rowley, vol. 1 (Indianapolis: Liberty Fund, 2003).

Once scientific theories are formulated and tested, scientists disseminate promising results through journals and other publications. Tullock clearly approves of this mechanism, though he identifies potential weaknesses in editing and in refereeing, and suggests a number of timely reforms, not all of which have yet been implemented.

In chapter VII, Tullock identifies reasons for the backwardness of the social sciences in research and in scholarship. He largely rejects the viewpoint, widely held even now by many social scientists, that social science is inherently more difficult than natural science because of the absence of controlled experiments. He places the blame instead squarely on differences in the social environments that exist between social scientists and natural scientists.

Tullock notes that new natural-science discoveries are always supported initially by a minority of the scientific community but eventually extend to the majority once they have withstood independent testing and are seen to be fruitful for practical applications. Acceptance by the general public follows in due course.

Social scientists, on the other hand, are often motivated to conceal the truth, for nonscientific reasons, with respect to findings that might be offensive either to themselves or to public opinion at large. The possibility of practical application is also more limited, lowering the standard to which their theories are exposed.

These checks and balances operate less effectively in the social sciences than in the natural sciences because there is less similarity of ends and, consequently, less voluntary cooperation.

In chapter VIII, Tullock concludes *The Organization of Inquiry* by outlining a number of practical proposals for improving the quality of scientific output. Two proposals are especially worthy of mention because of the particular insights they offer into Tullock's own worldview.

The first proposal underlines the classically liberal nature of Tullock's philosophy. Because most important scientific projects require only limited funding, they should be funded individually and not be included as part of a large, creativity-stifling, bureaucratic package. To avoid this outcome, and for the same reason, foundations that award research grants should also be small.

The second proposal underscores Tullock's healthy regard for the developing tenets of the public choice research program. Because of the importance of output rather than input, a much larger proportion of scientific research should be stimulated by direct prize awards. The system of prizes should be directed at two objectives, namely, specific discoveries and unspecified developments. Tullock suggests that a number of competing prize-awarding bodies, independent of the political process, would best protect science from cronyism and political lobbying.

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PREFACE AND ACKNOWLEDGMENTS

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;[1] nevertheless, Popper's approach necessarily rubbed off on me, and I became interested in the problems of science. Since I felt that I had little chance of making any significant addition to Popper's work on the philosophy of science, my inquiries were directed toward the problem of science as a social system. Philosophically, my debt to Dr. Popper is so heavy that I decided to acknowledge the debt here, instead of attempting to footnote his work in every case where it was relevant.

I have never met Michael Polanyi, but the reader will, no doubt, notice his influence also. Here, again, I have decided to omit most footnotes in the text and to handle the matter here. Although the main focus of Dr. Polanyi's work[2] is different from mine, there is clearly a close relationship.

I owe a further, rather diffuse, debt to the large number of scholars who in recent years have produced so much research on science. Most of this work, however, has added to my general knowledge, but not directly helped me in my work. I have, for example, read *The Structure of Scientific Revolutions*[3] with profit and pleasure, but it will not be further mentioned in this book. This is not because I regard it as unimportant but because it deals with different problems. In this it is typical. Most of the recent work has been done by people whose basic orientation is sociological, while mine is economic. There is no necessary conflict between sociologists and economists, but they do ask rather different questions. The work, particularly the empirical work, by the sociologists has enlightened and informed me, but it is generally not directly relevant to the problems investigated in this book.

1. The eventual outcome of my work was *The Politics of Bureaucracy* (Washington: Public Affairs Press, 1965).

2. In addition to *Personal Knowledge* (Chicago: University of Chicago Press, 1958), Polanyi has written numerous articles on science. His *Logic of Liberty* (Chicago: University of Chicago Press, 1951) contains much of interest to the student of science.

3. By Thomas S. Kuhn (Chicago: University of Chicago Press, 1962).

Although the study of the history of science is not new,[4] its present development is so much greater than at any previous period that it can almost be regarded as an invention of our present generation. This fact has both helped me and raised a minor but difficult problem. I have used numerous examples drawn from the history of science to illustrate theoretical points. The problem of whether I should footnote them all, thus insulting those of my readers who know their history, or whether I should assume that anyone who reads a book on the organization of science will need no authority for statements such as that Einstein was unable to get an academic job when he graduated was difficult. I have ended up with a compromise which will probably satisfy no one.

My colleague, Dr. James Buchanan, has assisted my work in many ways. In addition to many direct suggestions and comments, I have profited from his general methodological approach. His insistence on both imagination and rigor in the construction of theories has been an invaluable stimulus. One of the anonymous readers of the Duke University Press also must receive a good deal of credit for the final version. He (or she) made almost sixty specific suggestions for changes, of which I accepted over fifty, with a resulting major improvement in both the style and matter.

Last, having distributed credit where it is due, I must allocate some blame. The Duke University Press is solely responsible for any errors in spelling, punctuation, etc., which may occur in the book. I have never been any use at all as a proofreader, and the Press should have taken this fact into account in preparing the book for publication.

4. Adam Smith himself wrote a "History of Astronomy" in his youth. See Nathan Rosenberg, "Adam Smith on the Division of Labour: Two Views or One?" *Economica* (May, 1965), 127–39.

THE ORGANIZATION OF INQUIRY

CHAPTER I

THE SOCIAL ORGANIZATION OF SCIENCE

The purpose of this book is to answer, or attempt to answer, certain questions about science. I should like to be able to say that these questions have deeply interested scientists and that my solutions will be widely welcomed as settling important problems. Unfortunately I cannot do so. Leaving aside the problem of the correctness of my answers, the fact remains that I have been unable to find any indications that scientists have asked the questions to which I address myself. The unwary might take this as proof that the problems are unimportant, but scientists, fully conscious of the importance of asking new questions, will not make this mistake. Personally I think that the questions are important, and the answers, if not earthshaking, at least significant enough to justify adding one more to the fifty thousand or so books that will be published this year. In the first paragraph I can hardly expect the reader to share my faith, but I think that I can ask that he maintain that open but skeptical frame of mind which characterizes the best scientific thought.

In order to set the problem in its framework, let me begin with a lengthy quotation from a speech by Lord Brain.[1]

> . . . scientists often have no more in common with each other than that they are all seeking knowledge by means of scientific methods. Professor A uses these methods to investigate the light from receding nebulae, while Professor B is interested in the physiological clock which regulates the habits of shore-inhabiting crustaceans in relation to the tides. Dr. C is investigating the atomic nucleus and anti-matter, and so on through to Professor Z, who is studying the virus-carrying capacity of mosquitoes in a tropical forest. These scientists have probably never met one another. They may differ in age, sex, race, language, religion, and their general mode of life, and none of them may be interested in what the others are doing. As for the remote effects of their scientific activities, what Professor A does may be of importance for our ideas about the origin of the universe, while Professor B's work may have some implications for the stor-

1. As former president of the British Association for the Advancement of Science, Lord Brain gave this speech December 27, 1964, at a meeting of the American Association for the Advancement of Science at Montreal. It was published in *Science,* 148 (April 9, 1965), 192–98.

> age of information in the brain, and possibly for our understanding of the relationship between the brain and the mind. Dr. C deals with a subject which has already had profound importance in relation to the development of nuclear energy and today is likely to interest the philosophers of physics who are concerned with the ultimate nature of matter and the relationship between the observer and what he observes. And Professor Z's investigation of viruses concerns a scientific topic of great importance for our understanding of cell behavior, information at the molecular level, the nature of the gene, and the cancer cell. The immediate social effects of his work may well be the elimination of a particular group of diseases in tropical areas, and a resulting increase in the local population, which is already too great for its food supplies. Unless they are rather exceptional men in their particular field of work, none of these scientists may be much interested in its more remote implications. At any rate, they can all be first-class scientists without such an interest.
>
> I chose these examples at random, but I could well have chosen any other of the varieties of scientific work being practiced by the hundreds of thousands of scientists in the world. Scientists, of course, meet one another to exchange ideas, to promote their own particular branch of science, or science in general, or because they are aware of its social implications. Nevertheless, such collective activities, important though they may be in themselves, play a small part in their lives. Scientists, though they must always be aware of the work of their fellows in their own fields, are essentially individualists; and the body of knowledge to which they are contributing is an impersonal one. Apart from contributing to it, they have no collective consciousness, interest, or aim.

Note that Lord Brain never asks how it happens that these scientists who "have probably never met one another" and "may differ in age, sex, race, language, religion, and their general mode of life" are nevertheless contributing to an essentially co-operative activity. It happens that the particular examples he has chosen are in different fields of science, but if he had chosen men in the same field, say nuclear physics, they would still differ radically in "age, sex," etc. They are "essentially individualists," and "unless they are rather exceptional men in their particular field of work, none of these scientists may be much interested in its more remote implications." Clearly, however, the scientists are contributing to these remote consequences. What is the mechanism which leads the scientist "by an invisible hand to promote an end which was no part of his intention?"[2] There is no central co-ordinating organization, and few scientists consciously try to make their research contribute to remote and distant goals. Further, there is no more reason to believe that there is some sort of divine guidance for science than for economic activities.[3]

Obviously, however, the work of these highly individualistic scientists is not really independent. It is co-ordinated by something, so that Lord Brain can, quite correctly, say that the individual parts do fit together and lead to remote and unintended consequences. The scientists have never inquired into the nature of the social mechanism which provides this necessary control. Almost every scientist in the world would agree that cancer is more likely to be overcome by giving research funds to a large number of separate scientists without any central control over their research than by setting up a major hierarchy to plan each step of the scientific advance. The most effective way of "organizing" science seems to be the most perfect laissez faire. This, however, is a superficial view. Science is not unorganized. There exists a community of scientists, and this community is a functioning social mechanism which co-ordinates the activity of its members.

Another question which Lord Brain did not ask relates to the accuracy of the work done by the individual scientists. How does it happen that we can depend upon scientists not only to refrain from faking research results, but to exercise the most extreme precautions to insure accuracy? Although fraud and/or carelessness are not completely unknown among scientists, they are remarkably rare. The reliability of scientific reports is probably higher than that of any other form of literature. This phenomenon is so much a part of the existing system that most scientists simply take it as a given. Like Lord Brain, they do not ask the reasons for this extraordinary level of accuracy. Here, again, the answer lies in the organization of the scientific community.

This community is a most peculiar one, with its members living in different countries and speaking different languages. Further, it is not even geo-

2. Adam Smith, *The Wealth of Nations* (New York: Modern Library, 1937), p. 423. Smith continues: "Nor is it always the worse for society that it was no part of it."

3. The probable explanation for the fact that the phrase "invisible hand," which occurs only once in Smith's book, has been so widely quoted is the misapprehension that it refers to divine control. Smith as a follower of Leibniz probably felt that any well-functioning system in nature reflected the design abilities of the "divine clockmaker," but the purpose of *The Wealth of Nations* was to explicate the quite mundane mechanisms which controlled economy.