

# Science And Society

The Organization of Inquiry by Gordon Tullock. Duke University Press, 1966.

*By Frederick C. Kreiling\**

Do we now have the Third Culture that C. P. Snow saw coming to life? It would appear so. A good deal of work is currently being produced by a mixed group of scholars and intellectuals who are concerned with the problems of science and civilization and anxious to remedy the isolation and intellectual fragmentation that have arisen from intense specialization. Professor Tullock's absorbing book falls into this general category. More specifically, he is concerned with science as a social system, with the socio-economic implications of science and with the behavior of the scientific community.

Why do men inquire, he asks, and comes to the hardly surprising conclusion that some men inquire because they are genuinely curious, others because they are induced to do so by the gains that will accrue to them. Fair enough, as far as it goes, but it does not go far enough. The really crucial discoveries, from which science takes its inner life, seem to require such talent and depth of motivation that no general theory can really account for them. What was it that impelled Newton? Hope for fame? Neurosis? Fortunate historical circumstance? It is very hard to say. All three, and much more besides. Indeed, scholars have only within the last decade begun to describe his achievement with the degree of precision it deserves and there are probably not more than a dozen men in the whole world who really understand the Principia in its innermost depths. Before we can have a good theory of scientific

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\* Frederick C. Kreiling is professor of history at the Polytechnic Institute of Brooklyn, and a specialist in the history of science.

creativity we must know more about particular scientists and the conditions under which they lived and worked.

The vision of the unity of the sciences goes back to Aristotle. It was discussed at length during the seventeenth century especially by Leibniz. It is a beautiful and reassuring vision, but it is not quite, as Mr. Tullock believes, an accepted fact on which we can rely. Each science deals with separate categories of facts that are treated experimentally or observationally by persons who know the accepted theories and whose special skills have been sharpened by constant practice. The combined set of theories, applications, instruments and practices, that Thomas S. Kuhn has aptly called the paradigm of a science, is shared by a group of adherents. Under normal conditions they proceed to work on more detailed and refined questions which all refer to the general paradigm as given. At no point in history, least of all today, have the set of paradigms in existence taken together been entirely consistent. Before we can talk meaningfully of science as a unity, we should have to develop a language of the utmost precision that would, as Leibniz hoped, enable us, for example, to speak of both physics and psychology in consistent terms. We should, in brief, require a science of science.

To his credit, the author is keenly aware of the pitfalls in writing about science. He knows very well that there is more myth than truth in what is commonly held to be the theory of induction. Yet he cannot resist, albeit apologetically, spending a chapter on it. He correctly points out that human beings habitually make patterns out of bits of available information, and that this process, extended and refined, is probably the means by which theories are related to sets of facts. But he tends to neglect the historical dimension, in particular the fact that the researcher's attention is conditioned by the sheer existence of certain fields of inquiry and of preferred modes of investigation at any given time. Suppose, let us say, that during the seventeenth century a great epidemic attacked Western Europe, a disease that had the effect of altering the perception of Europeans so that they focused on one set of phenomena to the exclusion of others and that they proceeded to examine the preferred set. Later researchers would certainly be disposed to continue in the chosen areas, but at the price of neglecting equally or even more important ones. Scientists generally investigate classes of facts that have previously been established as significant. There is nothing, in principle, that cannot be examined scientifically, but the existence of particular scientific

fields has been determined in large part by historical situations.

Does the law of diminishing returns apply to science? A good number of writers, including the author of this work, think that it does. They share among other things the view that the degree of specialization may well be higher than optimal at least in traditional fields, and there is a good deal of evidence to support their contention. Indeed, it is even possible, as Stephen Toulmin has suggested, that the rate of increase in science as a whole is declining. No growth curve in nature ever continues at a constant rate.

As science claims an increasing share of the Federal budget its costs become a matter of concern, although they are minute in comparison with those of the war. More serious are the effects of support on science itself. The more the scientist depends on Federal grants, the more he is obliged to "justify" his work to government agencies that tend to measure "success" by results that can be put into immediate use. The administrator inevitably begins to think of the planning of research. Mr. Tullock is, (rightly, I think) against excessive planning in the sciences, for the historical record clearly shows that individual creativity has been by far the most significant source of innovation, whether in pure or applied fields. However, when one contemplates the effects of the untrammled expansion of certain kinds of technology, one is not inclined to trust Adam Smith's 'invisible hand' as the best means of control.

We cannot forget, as Mr. Tullock tends to, that business as usual for the automobile industry will not help the traffic problems of the cities, which could be ameliorated at least by the use of much smaller vehicles, nor that costly moon shots may not be as important in the long run as efforts to resolve the present ecologic crisis.

Sir Karl R. Popper, whose thought provided the philosophical inspiration for this work, is justly celebrated as one of the greatest living philosophers of science. He has stressed, among many other things, that freedom of inquiry is imperative for the growth of science; that many of the standard modes of explaining science mislead by giving the impression that scientific truth is absolute; and that one of the most important features of scientific procedure is that it requires hypotheses to be cast into

a form in which they not only can be verified, but more significantly, proved wrong.

I share with Popper and Tullock the liberal view that science has been a potent force for individual freedom in the past. But doubts arise for the present and future at the realization that so much scientific activity in recent years has been shrouded in secrecy or held as the special province of experts who brook no disagreement except from their colleagues. One wonders what John Stuart Mill would have thought of the words of one astute and humane observer of both science and the establishment who writes that: "unfortunately, the constraints of secrecy, although they disturb the comparative judgment, do not disturb the scientific process. In more liberal days, in the days of Rutherford's Cambridge, Bohr's Copenhagen, Franck's Göttingen, scientists tended to assume, as an optimistic act of faith, as something which ought to be true because it made life sweeter, that science could only flourish in the free air. I wish it were so. I think that everyone who has ever witnessed secret science and secret choices wishes it were so. But nearly all the evidence is dead against it. Science needs discussion, yes; it needs the criticism of other scientists; but that can be made to exist, and of course has been made to exist, in the most secret projects. Scientists have worked, apparently happily, and certainly effectively, in conditions which would have been thought the negation of science by the great free minded practitioners. But the secret, the closed, the climate which to earlier scientists would have been morally intolerable, soon becomes easy to tolerate. I even doubt whether, if one could compare the rate of advance in one of the secret sciences with one of those which is still open to the world, there would be any significant difference. It is a pity." (C. P. Snow, Science and Government, New York 1962, p. 68) We must at least not overestimate the liberating effects of science in the contemporary world. There can be no modern industrial nation without it and its potentialities for improving the lot of all mankind are immense, but the possibility of secret science as a medium of political control calls up the nightmare of the super-state armed with electronic listening devices and computerized dossiers on every citizen.

Mr. Tullock believes that the social sciences are decidedly "backward" because they are incapable of exact prediction. Worse, he thinks practitioners in those fields are mainly of the "induced" variety, rather than persons with a sustaining natural curiosity, and, as an economist,

he is unhappy to be classed among them. The belief in the alleged inferiority of the social sciences, which he also shares with Popper, arises, I believe, from the unfortunate practice of comparing them with physics. It is extremely unlikely, perhaps even undesirable, that anything as precise as physics will ever be derived from social science, and in my view, it is a tactical error for social scientists to worry so intensely about exact measurement to the exclusion of more profitable concerns. This is especially true at a time when previous physical models of the universe have undergone such profound transformations. Biology, particularly evolutionary studies, and above all ecology are probably more useful models. In these sciences, to which neither Popper nor Tullock have devoted much attention, the age-old quest for an irreducible essence has been accompanied by great interest in interactive processes, and it is precisely this type of study which is more useful in understanding social or political issues. Furthermore, the motivation of the best social scientists has been in no way inferior to that of the best natural scientists. (Every field has its modest contributors, perennial journeymen and so on; they help provide the social niche in which the best minds can function.) The issues social scientists confront are really much more difficult. Even Max Planck confessed himself terrified at the perplexities of economics and turned back with relief to the relative simplicity of physics.

On the whole Professor Tullock has written a provocative and interesting work which should prove very useful as an introduction to the problems of science in the matrix of modern culture.