Trust in Numbers

The Pursuit of Objectivity in Science and Public Life

Theodore M. Porter

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Preface

SCIENCE is commonly regarded these days with a mixture of admiration and fear. Until very recently, though, English-language historians of science were more likely to resent its pretensions than to fear its power. Here resentment grew out of reverence. Karl Popper and Alexandre Koyré, who gave form to brilliant traditions in the philosophy and history of science beginning especially in the 1950s, agreed that science was about ideas and theories. Koyré gave priority to thought experiments over the work of hands and instruments, and wondered, famously, if Galileo had ever performed any experiments at all. Popper allowed that experimentation could falsify theories, but held that the real work was done when the theory was adequately articulated. Experimenters had no more than to carry out what the theory dictated. Both praised science as a model of intellectual and philosophical achievement. Neither provided any reason for thinking that science could have much to do with technology. Still less could the hierarchical imagination of the historian or philosopher of science conceive that social science was authentically powerful.

This problem of the relations of science to technology inspired nothing like the heated (and, it now seems, empty and incoherent) controversy over the relative merits of "externalist" and "internalist" explanations of scientific change. Rather than arguing, much of the profession took for granted that science had the loosest connections with the practical world of engineering, production, and administration. In retrospect, I can see that my graduate training provided ample opportunity to form a more judicious view. My teachers learned earlier than I did to appreciate the limitations of seeing the scientific enterprise mainly as a pursuit of theory. Still, I think I was not unusual among historians of science of my generation in thinking that the widespread linking of science and technology or of science and administrative expertise involved something fundamentally spurious, that these supposed connections brought undeserved credit to each enterprise by making science seem more practical and its "applications" more intellectual than either really is.

A critique of this nature underlay my original formulation of this project. I planned to examine the history of neoclassical economics, the most mathematical of social science disciplines—indeed, possibly the most mathematical of all disciplines. Economics values most highly this supremely abstract mathematics, yet somehow economists sustain the

image of a discipline capable of telling businesses and governments how to manage their affairs more effectively. I expected to show through an analysis of the relations of economics to policy that academic economics was a kind of sport, empty of implications for economic practice.

That is not the book I have written. It didn't take long to realize that neoclassical economics has had many critics who were better informed than I was likely to become. I found also that the economics discipline involves a greater variety of tools, aims, and practices than I had appreciated, and while I still think there is need for a more profound consideration of the relations between economic mathematics and the practices that support forecasting and policy advice, I am not the one to undertake it. In any case, my earlier suspicion that mathematics and policy were almost independent worked badly as a way of formulating a historical project. Its validity was even more damaging than its shortcomings. If, indeed, neoclassical mathematics is irrelevant to the economic world, my history of the relations between economics and policy would turn into the history of nothing at all.

So I have taken here a different tack. The interpenetration of science and technology, I now concede, is unmistakable, especially in the current century. That of social knowledge and social policy is only slightly less so. How are we to account for the prestige and power of quantitative methods in the modern world? The usual answer, given by apologists and critics alike, is that quantification became a desideratum of social and economic investigation as a result of its successes in the study of nature. I am not content with this answer. It is not quite empty, but it begs some crucial questions. Why should the kind of success achieved in the study of stars, molecules, or cells have come to seem an attractive model for research on human societies? And, indeed, how should we understand the near ubiquity of quantification in the sciences of nature? I intend this book to display the advantages of pointing the arrow of explanation in the opposite direction. When we begin to comprehend the overwhelming appeal of quantification in business, government, and social research, we will also have learned something new about its role in physical chemistry and ecology.

My approach here is to regard numbers, graphs, and formulas first of all as strategies of communication. They are intimately bound up with forms of community, and hence also with the social identity of the researchers. To argue this way does not imply that they have no validity in relation to the objects they describe, or that science could do just as well without them. The first assertion is plainly wrong, while the latter is absurd or meaningless. Yet only a very small proportion of the numbers and quantitative expressions loose in the world today make any pretense of embodying laws of nature, or even of providing complete and accu-

rate descriptions of the external world. They are printed to convey results in a familiar, standardized form, or to explain how a piece of work was done in a way that can be understood far away. They conveniently summarize a multitude of complex events and transactions. Vernacular languages are also available for communication. What is special about the language of quantity?

My summary answer to this crucial question is that quantification is a technology of distance. The language of mathematics is highly structured and rule-bound. It exacts a severe discipline from its users, a discipline that is very nearly uniform over most of the globe. That discipline did not come automatically, and to some degree it is the aspiration to a severe discipline, especially in education, that has given shape to modern mathematics.[1] Also, the rigor and uniformity of quantitative technique often nearly disappear in relatively private or informal settings. In public and scientific uses, though, mathematics (even more, perhaps, than law) has long been almost synonymous with rigor and universality. Since the rules for collecting and manipulating numbers are widely shared, they can easily be transported across oceans and continents and used to coordinate activities or settle disputes. Perhaps most crucially, reliance on numbers and quantitative manipulation minimizes the need for intimate knowledge and personal trust. Quantification is well suited for communication that goes beyond the boundaries of locality and community. A highly disciplined discourse helps to produce knowledge independent of the particular people who make it.

This last phrase points to my working definition of objectivity. It is, from the philosophical standpoint, a weak definition. It implies nothing about truth to nature. It has more to do with the exclusion of judgment, the struggle against subjectivity. This impersonality has long been taken to be one of the hallmarks of science. My work broadly supports that identification and tends to the view that this, more than anything else, accounts for the authority of scientific pronouncements in contemporary political life. Once again, though, I am reluctant to make science the unmoved mover in this drive for objectivity. In science, as in political and administrative affairs, objectivity names a set of strategies for dealing with distance and distrust. If the laboratory, like the old-regime village, is the site of personal knowledge, the discipline, like the centralized state, depends on a more public form of knowing and communicating. Quantification is preeminent among the means by which science has been constructed as a global network rather than merely a collection of local research communities.

Some of the best and most fashionable recent work in science studies has aimed to understand science as a thoroughly local phenomenon. The genre of microhistory, which has enjoyed brilliant success in cul-

tural history, has become influential also in the history of science. I have learned a great deal from this work, and I hope I have adequately appreciated its virtues. It provides a superb point of departure for studies of science, precisely because it renders the universality of scientific knowledge problematical. But it does not simply negate it. Science has, after all, been remarkably successful at pressing universal claims and gaining international acceptance. Explaining this achievement, and unpacking its implications, ought to be central problems of the history of science. The account I give here is mainly cultural and, broadly, political. I suggest that the problems of organization and communication faced by science are analogous to those of the modern political order. This is not meant to imply that science is not constrained in important ways by the properties of natural objects, nor even that the forms of language and practice I discuss are independent of those properties. I do not claim that quantification is nothing but a political solution to a political problem. But that is surely one of the things that it is, and our understanding of it is poor indeed if we do not relate it to the forms of community in which it flourishes.

The argument, as I have presented it so far, is as much sociological or even philosophical as historical. Since I am unlicensed in both the former domains, I tremble at the thought of writing a book that is not securely historical. The flow of topics and arguments in the book, however, is hard to reconcile with narrative or analytical history. Indeed, the book does not conform well to any established genre of scholarly writing. But there is, I like to think, some method to this madness. I should perhaps explain at the outset the pressures and strategies that have given shape to this study.

I began, as I have already explained, with the intention of studying the modern history of social quantification in relation to academic disciplines. Soon I found myself paying more attention to professions and bureaucracies. This research, much of it in primary sources, is presented in chapters 3 and 5–7, and is used in support of various arguments elsewhere. It is the heart of the book. These chapters attest to my allegiance to the standards of my own discipline, which requires general explanations to prove themselves in analytic narratives that respect the cultural richness of real historical situations. The other chapters are more general, even theoretical, and draw heavily on other scholarship. They appear here partly as conclusions from my properly historical material, but the more empirical chapters are not at all innocent of the perspective they present. On the contrary, I found that I needed to think through the issues with which they grapple before I was able to write the narrative sections.

As it appears here, the book is divided into three parts and nine chapters. The first part is about how numbers are made valid—that is, how they are standardized over wide areas. Chapter 1 is concerned with aspects of the natural sciences, chapter 2 with the social. Chapter 3 is about their relation, and argues that this practical quantifying activity has been at least as central to the identity and ethos of modern science as any aspiration to formulate broad theoretical truths. Chapter 4 discusses the forms of political order that permit or encourage quantification. It examines some of the moral and political issues raised by this drive to create rigorous quantitative rules in domains previously occupied by a more informal style of judgment.

The second part presents some notable attempts at social and economic quantification in an explicitly political and bureaucratic context. I argue that the transition from expert judgment to explicit decision criteria did not grow out of the attempts of powerful insiders to make better decisions, but rather emerged as a strategy of impersonality in response to their exposure to pressures from outside. Chapter 5 treats nineteenth-century British actuaries, who were able to resist these pressures, and twentieth-century American accountants, who were not. Chapters 6 and 7 support a similar but subtler contrast involving the use of the economic analysis of costs and benefits by nineteenth-century French engineers and twentieth-century American ones. While, as I urge in part 1, numbers and systems of quantification can be very powerful, the drive to supplant personal judgment by quantitative rules reflects weakness and vulnerability. I interpret it as a response to conditions of distrust attending the absence of a secure and autonomous community.

Part 3 undertakes to apply the perspectives developed for professions and bureaucracies in part 2 back to the academic disciplines. Chapter 8 assesses the bearing of bureaucratic cultures on science, then shows how inferential statistics became standard in medicine and psychology as a response to internal disciplinary weakness and external regulatory pressures. Finally, chapter 9 examines the moral economy of scientific communities. I argue there that the seemingly relentless push for objectivity and impersonality in science is not quite universal, and must be understood partly as an adaptation to institutional disunity and permeable disciplinary boundaries.

I make no pretense to having written a general history of quantification. I include very little before 1830, and almost nothing from outside of western Europe and North America. The geographical limitations are perhaps less forgivable than the temporal ones, and the history of colonialism, of international organizations, and of centrally planned economies all provide extremely rich materials for the history of quantification. I discuss frequently the best-established academic disciplines, but

treat none of them in depth, preferring to concentrate on the role of quantification in applied fields such as accounting, insurance, official statistics, and cost-benefit analysis. Even within these constraints, I have been anything but exhaustive. Each of the topics just mentioned could form the subject matter for an entire historical subfield. So could many others that I have not discussed at all. Perhaps the highest ambition I can reasonably entertain for this book is that some of them will. If so, it may be possible in some decades to survey the field systematically. My main reason for discussing a range of topics and countries rather than writing a monograph on one is to suggest something of the potential richness of the field. This strategy presupposes another of my central goals: to convince readers that the history of quantitative objectivity is after all a potential subject of inquiry, and not simply a miscellany.

The last thing I would want, though, would be for this topic to become a new, autonomous specialty. One of the really heartening developments in history of science in the last decade or so has been the breakdown of its isolation. It brings me no small satisfaction that history of statistics has been noticed and increasingly is being studied in academic units devoted to literature, philosophy, sociology, psychology, law, social history, and various of the natural sciences, as well as in history of science and statistics itself. I am even more hopeful for the history of quantification as it bears on the cultural study of objectivity. Indeed, there is already a considerable literature, most of it very recent, that relates directly to the questions I ask in this book. So far there is nothing like a single discussion, but rather a variety of local conversations, largely isolated by discipline. I think the barriers are breaking down, and hope that this book will help to level a few sections of the wall(s). I have drawn freely and extensively on several bodies of scholarly literature, mainly because they are indispensable to my argument, but also in the hope that those who have contributed to or learned to appreciate one of them will find themselves unexpectedly in an integrated neighborhood—and like it.

Acknowledgments

SINCE so much of this book is a synthesis of other people's work, the text and notes themselves must stand in for a proper expression of my obligations. Nor is there space to acknowledge individually all those friends and antagonists who asked provocative questions or made helpful comments in response to earlier presentations and publications of ideas that have found their way into this book. I thank Ayval Ramati for expert research assistance, and David Hoyt for help in preparing the manuscript. I have benefited from comments on the entire text by Lorraine Daston, Ayval Ramati, Margaret Schabas, Mary Terrall, and Norton Wise, and on specific chapters by Lenard Berlanstein, Charles Gillispie, and Martin Reuss.

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provided a fine solution (plus or minus sixty miles) to this problem of academic geography. Finally I want to thank my parents, Clinton and Shirley Porter, my wife, Diane Campbell, and my son, David Campbell Porter, for their love and patience.

University of California, Los Angeles
March 1994

INTRODUCTION

Cultures of Objectivity

> "Whatever logic is good enough to tell me is worth WRITING DOWN," said the Tortoise. "So enter it in your book please."
> (*Lewis Carroll*, "What the Tortoise Said to Achilles," *Mind*, 1895)

"OBJECTIVITY" arouses the passions as few other words can. Its presence is evidently required for basic justice, honest government, and true knowledge. But an excess of it crushes individual subjects, demeans minority cultures, devalues artistic creativity, and discredits genuine democratic political participation. Notwithstanding such criticism, its resonance is overwhelmingly positive. Attacks are rarely directed at true objectivity, but rather at pretenders who use it to mask their own dishonesty, or perhaps the falseness and injustice of a whole culture. Most often it is not closely defined, but simply invoked to praise or blame. In the United States, scientists, engineers, and judges are generally presumed to be objective. Politicians, lawyers, and salesmen are not.

There remains the delicate question of what these attributions of objectivity mean. It is not merely an all-purpose honorific, for it applies more readily to the despised bureaucrat than to the indispensable entrepreneur. It has, however, several distinct senses, which tend to reinforce the positive associations of the term and at the same time to obscure it. Its etymology suggests an acquaintance with objects. Paradoxically, to us, until the eighteenth century these were usually objects of consciousness rather than physical things; real entities existing outside of us were called subjects. But in current philosophical usage, objectivity is very nearly synonymous with realism, while "subjective" refers to ideas and beliefs that exist only in the mind. When philosophers speak of the objectivity of science, they generally mean its ability to know things as they really are.[1]

An earlier generation, the positivists, considered such claims merely metaphysical, and hence meaningless. But they did not disdain using the term. There are other ways of construing the objectivity of science. The most influential has defined it by an ability to reach consensus. Normally it suffices if that consensus holds within a specialist disciplinary community. We might, with Allan Megill, call this "disciplinary objectivity," by

contrast to the "absolute objectivity" of the preceding paragraph. This form of objectivity is not self-subsistent. Its acceptability to those outside a discipline depends on certain presumptions, which are rarely articulated except under severe challenge. Specialists who claim objectivity should provide some evidence of their expertise. They should comport themselves appropriately. They should appear reasonably disinterested, or at least should not expect to speak authoritatively where their own individual or professional interests are at stake. We trust physicists to tell us about phase transitions in supercooled helium, but we are more skeptical if they appear as paid expert witnesses in court, or when they tell of the great economic advantages that will attend the construction of a superconducting supercollider.

Still, physicists control a large territory on which they are not called upon by outsiders to justify their conclusions. Disciplinary objectivity is made conspicuous mainly by its absence. Where a consensus of experts is hard to reach, or where it does not satisfy outsiders, mechanical objectivity comes into its own. Mechanical objectivity has been a favorite of positivist philosophers, and it has a powerful appeal to the wider public. It implies personal restraint. It means following the rules. Rules are a check on subjectivity: they should make it impossible for personal biases or preferences to affect the outcome of an investigation. Following rules may or may not be a good strategy for seeking truth. But it is a poor rhetorician who dwells on the difference. Better to speak grandly of a rigorous method, enforced by disciplinary peers, canceling the biases of the knower and leading ineluctably to valid conclusions.

The tension between the disciplinary and the mechanical senses of objectivity is a central concern of this book. But these two senses will not be discussed only on the terrain of science, and so it is important to consider also the meanings of objectivity in explicitly moral and political discourse. In most contexts, objectivity means fairness and impartiality. Someone who "isn't objective" has allowed prejudice or self-interest to distort a judgment. The credibility of courts depends on an ability to elude such charges. They do so in large part by placing disputants in a highly controlled situation and authorizing independent judges and jurors to resolve the facts and apply the law. The objectivity of jurors means little more than their presumed disinterestedness, since by definition they lack special expertise. Judges too are expected to be impartial, though they should also be trained professionals. Their expertise must include an ability to follow the rules—mechanical objectivity—but there is no avoiding the judicious exercise of discretion.

Two of the three meanings discussed in Kent Greenawalt's *Law and Objectivity* pertain directly to objectivity as fairness. "Legal determinacy" refers to the ability of any lawyer or other intelligent person to

reach the same conclusions about what the law means. It does not require that existing law be morally defensible, but only that different judges will apply the law to most cases in the same way. So defined, this kind of objectivity is not the preserve of disciplinary insiders, though it may be that only those who have immersed themselves in the culture of law can attain this consistency of judgment. Greenawalt observes, next, that treating people impersonally according to "objective standards" is central to what we call the rule of law. This generally entails a rigid schedule of punishments for various criminal acts, and a minimum of opportunity for discretionary adjustments based on subjective inferences about character and intentions. Both these senses of objectivity imply that rules should rule, that professional as well as personal judgment should be held in check. They point to the alliance of objectivity as an ideal of knowing and objectivity as a moral value.[2]

It is important to understand that mechanical objectivity can never be purely mechanical. Greenawalt offers as an example the simple instruction, spoken by a manager as a subordinate enters her office: "Please shut the door." It requires some experience of the world, and perhaps also of the office in question, to know which door, and when; to judge whether to mention first some reasons why it should remain open; and also to understand that if the company president suddenly appears at the door, the directive should be put aside. Rarely does any of this need to be spelled out, at least within one culture. Similar questions, including some much harder ones, will arise in filing papers, keeping accounts, taking a census, or preparing a graph. Especially in law, philosophy, and finance, where clever people make a business of exploiting ambiguities, much of what would otherwise go without saying ends up having to be said.

Mathematical and quantitative reasoning are especially valued under these circumstances. They provide no panacea. Mapping the mathematics onto the world is always difficult and problematical. Critics of quantification in the natural sciences as well as in social and humanistic fields have often felt that reliance on numbers simply evades the deep and important issues. Even where this is so, an objective method may be esteemed more highly than a profound one. Any domain of quantified knowledge, like any domain of experimental knowledge, is in a sense artificial. But reality is constructed from artifice. By now, a vast array of quantitative methods is available to scientists, scholars, managers, and bureaucrats. These have become extraordinarily flexible, so that almost any issue can be formulated in this language. Once put in place, they permit reasoning to become more uniform, and in this sense more rigorous. Even at their weakest point—the contact between numbers and the world—methods of measurement and counting are often either

highly rule-bound or officially sanctioned. Rival measures are thereby placed at a great disadvantage. The methods of processing and analyzing numerical information are now well developed and sometimes almost completely explicit. Once the numbers are in hand, results can often be generated by mechanical methods. Nowadays this is usually done by computer.[3]

The growing role of quantitative expertise in the making of public decisions is a development well known to scholars. Yet we have no satisfactory histories of it. This is due mainly to a failure to integrate two rival views of the development of quantitative methods, and of expertise generally. One narrative treats their history as the progressive accumulation of truer, or at least more powerful, methods. The other reduces them to ideology, to be explained mainly in terms of social structures of domination, though with due regard to the often nefarious aims of their individual purveyors. These are the arguments of partisans, who for the moment have forgotten the value of nuance. But it is not merely moderation that is called for. Expertise, much more even than science, is not understandable as simply the result of solitary thinking and experimenting, or even of the dynamics of a disciplinary community. It is a relation between professionals—often academic scientists or social scientists—and public officials. Their appreciation for expertise, in turn, reflects their relationship to a still wider public. To understand the circumstances under which quantitative objectivity has come into demand, we need to look not only at the intellectual formation of experts, but even more importantly at the social basis of authority.

We now have a few studies that have taken this insight as their point of departure. One argument, particularly influential among American historians, holds that the social science of the 1890s and 1900s arose from a new sense of interdependence among Americans, and ultimately from the social and economic processes that produced that interdependence.[4] There is doubtless something to this, even if a world economy did not abruptly form in the late nineteenth century. But the form of expertise that arose in specific response to this sense of interdependence is not the most important kind, and it is not at all characteristic of public uses of social science. It amounts, in Thomas Haskell's account, to a philosophical understanding of human interdependence, providing the consolation of explanation to a bewildered public. In fact there were a variety of rival forms of explanation of the industrialized social world, not all of them consoling, and most coming from preachers or labor organizers rather than professors. Academic social scientists have had only the most modest success in forming public opinion. The principal audience for their expertise is a bureaucratic one, usually with the acquiescence of elected officials.[5] The public culture licenses academic spe-

cialists not to issue general pronouncements, but to assemble very specific findings.

To be sure, this is not the only kind of expertise. There is a kind of wisdom that comes from long experience, which often is passed on from parent to child or master to disciple. In modern times, personal experience and contact with a master have increasingly been supplemented or replaced by formal instruction at a university or other educational institution. There the ineffable skill of the craft or guild is, so far as possible, made formal and explicit, and thus the secrets of the trade are deemphasized. To citizens of large-scale democratic societies, this is more acceptable because it is more open and less personal. Nevertheless, expert knowledge is almost by definition possessed by only a few, and no such art is ever reduced to a handful of rules that can be looked up and mastered by anyone with a textbook. Thus the intuition or judgment of specialists continues to command a degree of respect, even if the doctor, for example, cannot explain exactly why the problem must be in the liver. Still, both physicians and patients have learned not to be satisfied with an opinion based on little more than intuition. Better to apply an instrument, to take a culture, to produce some specific evidence.

In public even more than in private affairs, expertise has more and more become inseparable from objectivity. Indeed, to recur to the previous example, it is in part because the relation of physician to patient is no longer a private one—due to the threat that it might be opened up in a courtroom—that instruments have become central to almost every aspect of medical practice. In public affairs, reliance on nothing more than seasoned judgment seems undemocratic, unless that judgment comes from a distinguished commission that can be interpreted as giving representation to the various interests. Ideally, expertise should be mechanized and objectified. It should be grounded in specific techniques sanctioned by a body of specialists. Then mere judgment, with all its gaps and idiosyncrasies, seems almost to disappear.

This ideal of mechanical objectivity, knowledge based completely on explicit rules, is never fully attainable. Even with regard to purely scientific matters, the importance of tacit knowledge is now widely recognized.[6] In efforts to solve problems posed from outside the scientific community, informed intuition is all the more crucial. The public rhetoric of scientific expertise, however, studiously ignores this aspect of science. Objectivity derives not mainly from the wisdom acquired through a long career, but from the application of sanctioned methods, or perhaps the mythical, unitary "scientific method," to presumably neutral facts. There should be no room for the biases of the researcher to corrupt the results. It is, of course, possible for investigators or officials to be impartial as a result of their inherent fairmindedness, or perhaps their

utter indifference to the outcome, but how can we know? In a political culture that idealizes the rule of law, it seems bad policy to rely on mere judgment, however seasoned.

This is why a faith in objectivity tends to be associated with political democracy, or at least with systems in which bureaucratic actors are highly vulnerable to outsiders.[7] The capacity to yield predictions or policy recommendations that seem to be vindicated by subsequent experience doubtless counts in favor of a method or procedure, but quantitative estimates sometimes are given considerable weight even when nobody defends their validity with real conviction.[8] The appeal of numbers is especially compelling to bureaucratic officials who lack the mandate of a popular election, or divine right. Arbitrariness and bias are the most usual grounds upon which such officials are criticized. A decision made by the numbers (or by explicit rules of some other sort) has at least the appearance of being fair and impersonal. Scientific objectivity thus provides an answer to a moral demand for impartiality and fairness. Quantification is a way of making decisions without seeming to decide. Objectivity lends authority to officials who have very little of their own.

Part I

POWER IN NUMBERS

Now it must here be understood that ink is the great missive weapon, in all battles of the learned, which, conveyed through a sort of engine, called a quill, infinite Numbers of these are darted at the enemy, by the valiant on each side, with equal skill and violence, as if it were an engagement of porcupines.
(Jonathan Swift, "The Battle . . . between the Ancient and Modern Books," 1710)