Theodore M. Porter

Statistics, Social Science, and the Culture of Objectivity*

It is common in the social sciences these days to treat culture as a category of explanation to be invoked when no adequate account of customs and behaviors can be given in terms of rationality. This approach is especially favored by economists, game theorists, and other proponents of rational choice theories. These models are so powerful or so flexible that almost every kind of action has been construed as following from the rational pursuit of individual self-interest. Cultural anthropologists see a narrower domain of rationality, opening a correspondingly large space for culture, but even they often presume that culture and rationality are in opposition. I think this is untenable. Cultures always have their rationalities; standards of reasonableness are not universal, but always and inevitably reflect culture. That does not mean they can have only local validity, especially not in an era of economic globalism and cultural imperialism. It does mean that rationality is conditioned by political forms, economic circumstances, institutions, laws, and customs. And that is true not only of Trobriand Islanders or Vietnamese peasants, but also of European bureaucrats and American scientists. There are cultures of objectivity. A preoccupation with quantification and statistical analysis defines a particularly important one in our time.

For present purposes, it is best to leave aside the common use of objective as roughly synonymous with truth and consider it instead as the opposite of subjective. Subjectivity generally refers to emotion and feeling as opposed to knowledge, and to the personal, idiosyncratic, or biased rather than to what is detached, universal, and just. One way to escape the personal is to focus on nature, regarded as entirely external to ourselves and as separate from the world of moral values. In principle, economies, societies, and even human bodies and

* Research for this paper was supported by NSF grant SBER94-12396.

minds can also be regarded as alien and hence as objects. Especially in regard to the human domain, such distancing is not easy, and claims to achieve it may often involve deliberate or unwitting deception. Perhaps, also, this quest to escape what is tainted by our values and subjectivities and to gain access to something wholly external is itself the cultural expression of a human need or desire. In any case, knowledge of nature or society is in practice never the product simply of a confrontation between the investigator and objects of study, never a matter merely of the rational beliefs of isolated individuals. In every culture possessing science in a form we would recognize, knowledge depends on reaching agreement within a culturally-defined body of competent practitioners. That means there must be customs or rules governing the activity of knowledge-making.

This has not gone entirely unnoticed. Scientists and the public alike speak readily of „scientific method", presumed to be formal, timeless, and explicit. Historians of science have by now become deeply skeptical of method talk. We tend to emphasize shared techniques, implicit understandings, and cultural codes over methods and rules. Skill, negotiation, judgment, and trust are important for all the sciences, and their exercise is often accepted almost without question in normal scientific practice.[1] The effort to specify the rules of scientific procedure and to exclude what is loose or informal has usually been associated with weaker, less prestigious, contested, or politically exposed disciplines, often social ones. Not only have researchers in these fields tried often to limit themselves to the study of phenomena that can be quantified, but they have sought also to exclude subjectivity by articulating rules, frequently statistical ones, of inference and reasoning. Often this is done in the name of universal scientific method. I hold that the insistence on impersonal rules in science is a cultural response of conditions of distrust within their disciplines and in the larger society.[2]

This pervasive opposition to the subjective has become more and more widespread in recent times, but has perhaps been cultivated most assiduously in America. One of its important associations is with a culture of pluralism, democracy, and distrust, and it is very much a political value. While the pursuit of objectivity pertains to the way that disciplines and bureaucracies organize the attainment and use of knowledge, it also reflects, and helps to create, the values of the

1 On the importance of local, implicit knowledge and community in science, see: H. M. Collins, Changing Order, Los Angeles, 1985; Martin Rudwick, The Great Devonian Controversy, Chicago, 1985; Sharon Traweek, Beamtimes and Lifetimes, Cambridge, Mass., 1988; Steven Shapin, A Social History of Truth, Chicago, 1994.

2 I argue this way in my book, Trust in Numbers: The Pursuit of Objectivity in Science and Public Life, Princeton 1995.

larger political order. Relatively private or tacit knowledge, using methods that are nonrationalizable – though presumably not irrational – works best within tightly-knit groups. More public, explicit, and impersonal forms of rationality are fit for larger publics, audiences reaching far outside the boundaries of research schools and specialist communities. Quantification, at least as used in science and bureaucracy, is located preeminently in this second category. It is a technology of distance, a kind of public knowledge, embodying an aspiration to the universal. The insistence in some fields that a result means nothing until it has passed a statistical test of significance at some predefined level is very much a gesture towards public accountability.

The concept of accountability is suggestive.[3] Inferential statistics is a form of accounting, an accounting of permissible belief. We can learn something about the uses of quantification in sciences like sociology and economics by asking first about this great prototype of quantitative reasoning. Social and behavioral scientists have usually assumed that quantifying represents an effort to be like physics. This is not simply false, but there is another sense, equally valid, in which it may be understood as an effort to be like accountancy. As the publication manual of the American psychologists, in at least one edition, urged researchers in relation to tests of statistical significance: „Treat the result like an income tax return. Take what's coming to you, but no more."[4]

The reverence for rules of statistical inference implied here is not simply an expression of mathematical rationalism or of faith in method. It embodies a kind of renunciation. As I argue below, that renunciation can have a very interesting personal dimension, but it should be seen first of all as a social act, a sacrifice of individuality to the larger public. Rarely is it simply voluntary, for in many cases it is sternly enforced. This is obviously true of tax accounting, and it can also be true of inferential statistics. The statistical test and the tax return make the same demands: you should not be greedy; and you should follow the rules. Accounting is famous for being boring. Most people hardly know how to distinguish accounting from bookkeeping. Indeed, it is hard to see how tax accounting could be anything other than an exercise governed by strict rules and authoritative canons of interpretation. We might ask why. It is not because anybody has yet discovered a formula for fixing tax obligations so as to maximize

3 For an introduction to scholarship on accounting, see: Anthony Hopwood and Peter Miller, eds., Accounting as Social and Institutional Practice, Cambridge 1994; Michael Power, ed., Accounting and Science: Natural Inquiry and Commercial Reason, Cambridge 1995.

4 American Psychological Association, Publication Manual, 2nd ed., Washington D.C. 1974, 19.

the sum of personal utilities, or (according to a different ethics) to adhere to universal standards of justice. The reason for inflexible rules is because otherwise people would shirk their responsibilities. If we merely specified some guidelines and asked them to contribute their fair share, they would emphasize the hardships under which they suffer, or point to the vast contribution they have already made to the good of society in the form of nonfinancial contributions. They would seek out loopholes of all kinds. Indeed, even with a detailed code they seek out loopholes of all kinds. One of the occupations of tax accountants is to identify and then to stretch these loopholes, and in this way to undermine tax systems. In response, tax authorities like the *Internal Revenue Service of the United States* issue volume after volume of new regulations in order to define ever more explicitly the scope of the deductions and exemptions. Then the tax accountants set to work on these. It is a form of struggle, leading to ever more arcane tax codes and ever larger bodies of bureaucrats and professionals to manage the whole business. The experience of tax accounting shows how difficult it is to maintain a system of rules when individuals have an interest in undermining them. But the alternative is to trust citizens to contribute in proportion to ability and to benefits derived – which would severely test our public-spiritedness – or to empower a body of experts, accountant-kings, to tell each of us what we ought to pay. They might conceivably do a better job than the rules. But the administrative problems would be huge, and there would emerge sharp local variations. We would soon begin to suspect the tax authorities of arbitrariness and hence of abuse of power. Such a tax system is almost unimaginable, even to accountants.

Other varieties of accounting can more easily be conceived as depending more on judgment and not merely on quantitative rules. An example is auditing. Anybody in the United States who owns stock gets a printed statement every quarter or every year which includes a set of audited accounts. Very few of us know how to read these accounts at all, any more than most of us understand very well the mathematics that lies behind the statistical test that social scientists and medical researchers read and use. Experts can learn something from printed accounts, but they also know that many of the numbers do not mean what they appear to mean, and it may well be impossible from the statement alone to know what they ought to mean. Often, perfectly legal accounts can be thoroughly misleading. Why not include an expert opinion about the quality and financial soundness of the firm?

This could often be very helpful. But there are good reasons why no such opinions generally appear in the printed accounts. It is naturally a little tricky for outside accountants to announce that their client is on the verge of bankruptcy, despite a very favorable balance sheet. If, on the other hand, the accountants were to express a favorable opinion which proved wrong, they would expose themselves to lawsuits from disgruntled investors. Finally, and perhaps most crucially, there is strong pressure from government regulators to employ well standardized categories, and to let these speak for themselves. Some powerful spokesmen for the American accounting profession argued vigorously in the 1920s and 1930s that the numbers by themselves are never sufficiently informative, and are often misleading; that accountancy is an art of interpretation; and hence that the great campaign for standardization run by government regulators should be resisted. The public, they urged, would be much better off relying on professional judgment. These leaders of accounting largely failed. The accounting profession had to set up its own standards board to avoid having a government agency, the *Securities and Exchange Commission*, dictate everything to them. The regulators feared that standardization was necessary to avoid a loss of investor confidence in the accounts. The accountants had to sacrifice their expert judgment on the altar of uniform regulation, public accountability, and administrative convenience.[5]

Scholars sometimes identify this universal reliance on numbers and calculation with technocracy, the rule of experts. This claim has some merit; it does take real social power to sustain a profession based on calculation, and to induce others to accept the validity of the numbers it creates or certifies. But there is a kind of weakness implicit in a calculating profession – at least in a profession that is expected to calculate according to rules, and that is not allowed to express opinions about what the numbers mean, what they exaggerate, and what they conceal. American accountants were driven to an almost exclusive identification with a rather small set of relatively standardized numbers because their position was in several crucial ways weak and exposed, and because courts and regulators were unwilling to trust them to exercise what might be called their hermeneutic function. Of course we do not need to invoke distrust to understand why accountants use numbers at all; where there is money there are bound to be numbers. But the unique identification of accounting with quantification is indeed a sign that this is a relatively weak profession. We might define 'elite' as referring to those who are entitled to exercise discretion over matters of public interest. In this sense, accountants have to a considerable degree been subjugated by higher powers, and their deference to routines displays their lack of elite status.

5 Theodore M. Porter, Quantification and the Accounting Ideal in Science, in: Social Studies of Science 22 (1992), 633–652.

To be sure, the work of accountants is far from purely mechanical. Many surreptitious acts of judgment are concealed by the gray suits, by the appearance of merely performing routine computations. The rules are never self-enforcing. It normally requires other experts to determine where they have been violated. But this does not mean that they are entirely without force, as if we were compelled to choose between absolute liberty and utter constraint. Rule-following is always a matter of degree. Accountants are subject to relatively strict rules, in consequence of their modest prestige, the sensitivity of the domain they occupy, and their vulnerability to outside pressure.

The early history of cost-benefit analysis, an extension of accounting, reveals some of the pressures that have promoted mechanical objectivity. Cost-benefit analysis has been subjected to much criticism, especially from the left, where it is identified with „technocrats" and „econocrats". The implication is that through such methods, economists have usurped a part of the political domain. Now this is in a way true. But, as with accounting, the credibility of cost-benefit measures is based to a large degree on the assumption that they are impersonal and objective, because they are dictated by rules. That is, economists are permitted to operate at the node of power because they are assumed to be self-effacing. At least it is regarded a damning criticism whenever they can be shown to have acted arbitrarily or creatively. So, while it is true that these methods were put in place to contain or bypass public debate, it is not true that they have created a new, anti-democratic elite. Rather, this is a kind of power grounded in a presumption of impotence.

Economists did not fashion cost-benefit analysis out of whole cloth. In fact, they did not fashion it at all. As a political technology, cost-benefit analysis was first worked out by public engineers, in particular by the U.S. *Army Corps of Engineers.* We should not seek to explain this by supposing that it is somehow the nature of engineers to want to quantify everything. The army engineers worked toward uniform rules because they were challenged by powerful political and bureaucratic rivals, for which reason mere judgment became increasingly suspect. Cost-benefit analysis was created and has flourished above all in a political system characterized by unclear lines of power and overwhelming distrust.

There was nothing original in the idea that public investments should produce benefits in excess of costs. Since the seventeenth century, and probably earlier, projects have often had to be defended in these terms. This requirement never began to be routinized until the twentieth century. Even then, methods could be extremely loose. The *Corps of Engineers*, more than most such agencies ever did, operated within a patronage system. The Congressional committees charged to oversee its work were made up without fail of Congressmen who were eager to win new projects for their districts. So they generally did not inquire very searchingly into the rationality and consistency of Corps calculations. But as these decisions were more and more vigorously contested, it became ever harder to maintain a relaxed, informal, patronage system. Water control authorities were increasingly forced to choose between naked politics and quantitative objectivity.

A flood control act of 1936 wrote the cost-benefit standard into law. A finding by the Corps of a benefit/cost ratio greater than one was hereafter required before any flood-control project could be authorized for construction. Within a few years, similar requirements were written for navigation and irrigation projects. These laws did not cause any abrupt changes, but were part of a process of bureaucratic formalization. As important for economic calculation as the cost-benefit standard itself was the huge increase in the scale of water control initiated by these Depression-era acts. More money meant more interest, and also stronger opposition. Opponents often fought their public battles using the language of the new cost-benefit standard. That is, they challenged Corps numbers, and it was mainly in the face of such challenges that the methods of cost-benefit analysis had to be spelled out in detail, and eventually grounded somehow in economic rationality.

Although the *Corps of Engineers* was the kind of agency that came bearing gifts, not everybody liked this. Utility companies considered that power generation on public dams amounted to interference with free enterprise – that the Corps was an agency of creeping socialism. They argued that public bureaucrats did not know much about the economics of electricity, and that real experts could see where their benefit numbers were exaggerated, because they had neglected such essential technicalities as diurnal variation in power consumption. The railroads made similar arguments about canals and waterways – that this was no business of the government, and that sheltered bureaucrats just did not understand the needs of shippers well enough to construct adequate measures of benefits. So utilities and railroads sent representatives to disrupt the traditionally-cozy Congressional hearings on public works. They made the Corps explain its sampling methods, its basis for calculating the expected volume of freight on a waterway, its determination of savings over alternative forms of transportation, and its assumptions about interest rates. Although the opponents won few battles, their unfriendly inquiries made it very difficult to defend agency discretion.

Still more serious problems arose because of interagency conflict. In principle, the field of hydraulic engineering was divided up in a sensible manner. The Corps was in charge of navigation and flood-control projects. The *Bureau of Reclamation,* in the *Department of Interior,* constructed big irrigation dams to promote the agricultural development of arid public lands in the western states. The *Department of Agriculture* tried to help small farmers and to prevent soil erosion, in part through the control of water run-off. In practice, these missions overlapped, and the agencies fought bitterly. The really outstanding example of interagency fighting involved the Kings River in California. There, more pointedly even than in other similar instances, the measurement of benefits became the main point of contention in the struggle over which agency should build a large dam. Eventually a presidential directive, invented for the occasion, dictated that the balance of benefits between irrigation and flood control was the proper basis for assigning the project to one of the rival bureaus. Unfortunately the Corps and the *Bureau of Reclamation* each had its own cost-benefit methods, and inevitably they led to contradictory results. At the top levels of government, Congress backed the Corps and President Franklin Roosevelt supported the Bureau. This lack of agreement opened the issue up to local political contest, which soon penetrated almost every aspect of this project. By 1940, a few years after serious planning began, the process was completely out of control.

It became impossible for the two agencies to settle the issue quietly through negotiation, and open political fighting over this issue lasted more than a decade. It seemed that the only way to avoid such terrible fighting in the future was to establish uniform methods for measuring benefits and costs. Economic rationality did not count for much if its validity was only local – if different bureaucracies subscribed to different standards, or even applied the same standards differently. In the end it was necessary not only to define methods but also to recruit experts when applying them. Increasingly, these experts were drawn from among academic economists. The process of codification was promoted under supervision of the *Bureau of the Budget,* and later also of the courts. The move to uniform methods implied a sacrifice of agency autonomy and of traditional planning methods.[6]

Modern mathematical statistics was founded on a similar sacrifice. Indeed, it was in a way born of an ideal of personal renunciation. The notion of statistics as a set of quantitative methods standing above every particular application was not quite invented, but was first made workable, by the applied mathematician and general polymath Karl Pearson. His mathematical and discipline-building work in the two or three decades after about 1892 are widely recognized as the founding of a discipline. The background to his achievement, though, has never been adequately

6 Porter, Trust in Numbers, see footnote 2, chapter 7.

appreciated. Pearson was a remarkably able scientist and intellectual. Not least among his achievements was his distinctive and highly influential *Grammar of Science*, which linked philosophy of science to moral philosophy. In some ways it does not commend itself to a contemporary sensibility, but it was impressively original, even brilliant, and it is exceptionally revealing about the sorts of aspirations that gave statistics overwhelming appeal in some of the natural sciences and most of the social ones.

We can approach the arguments in Pearson's *Grammar of Science* by way of his own intellectual development. In 1879, aged 23, he finished his Cambridge degree and set off for a *Wanderjahr* in Germany. He was much affected by his experience there. He became a socialist, after a fashion. He changed the spelling of his first name from Carl to Karl. He became intensely interested in German social and religious history. He wrote about that history very skillfully, and one pillar of his socialism was his romanticization of German peasant communities. He also wrote an extremely interesting novella about the experience, which he called *The New Werther*. Like Goethe's *Werther*, this took the form of a one-sided correspondence. Of course we cannot take its contents as being literally true about the author, yet in many ways it mirrors his own experiences as recorded in contemporary diaries and letters. Pearson, universalizing his own youthful *Angst*, introduced the book as a *Zeitroman*, reflecting the mind of a generation „cast creedless on the weary waters of nineteenth-century thought."[7]

The anguish endured by Goethe's Werther is due to the unfortunate circumstance that his beloved Lotte is betrothed, and later married, to another. Pearson's novel begins with a heroic act of will; the protagonist Arthur enjoys a perfect union with his beloved Ethel, with whom he has been „one in impulse, soul, and life", yet determines to renounce it, after the model of *Wilhelm Meister*, in order to purify mankind. Inevitably, it all turns out badly. Ethel becomes more and more distant, and eventually leaves him for Arthur's soul-mate, the Jewish mystic Raphael. In the same way, nature refuses his advances. Arthur began as a thorough romantic. His was a soul „straining to free itself from the empirical"; he wanted to „penetrate Nature's very laboratory". The erotic aspect of this longing is evident throughout, and was consciously dramatized by Pearson. It led only to frustration. Arthur's act of sexual renunciation was also a separation from nature. The connectedness he lost could never be restored. Pearson himself, it is relevant to note, was in the process of becoming a thoroughgoing positivist. The *Grammar of Science* insists that we cannot hope to know what is really in nature – that science can do no more than

7 Karl Pearson (under pseudonym Loki), The New Werther, London 1880, iii.

organize our sensations. We can never embrace nature, merge with it, as Arthur so badly wanted; it always holds itself at a distance. This is the kind of nature of which one can never know causes, but only detect correlations. In short, the outcome of Arthur's heroic renunciation seems to be Pearson's statistical approach to science.

There is also another outcome. Already in Pearson's earliest writings, he called on scientists to be superior to the common run of humanity. He argued that if science is to mean anything at all, it must involve knowledge that transcends the interests and the biases of the individual. On that account, he made the scientist into a kind of priest, a „minister of freethought." In science, he explained, „[t]here must be no interested motive, no working to support a party, an individual, or a theory; such action but leads to the distortion of knowledge, and those who do not seek truth from an unbiased standpoint are, from the freethinker's standpoint, ministers in the devil's synagogue."[8] We can see in such sentiments, and even in much later writings, a continued yearning for community, for a merging of the self into a larger social whole, of the sort that his Arthur could never quite attain. By the first edition of his *Grammar of Science*, published in 1892, he was placing much less emphasis on the selflessness of scientists. The renunciation required for public knowledge could not be left to the individual conscience. It had to be institutionalized somehow. To this end, Pearson developed his language and his philosophy of scientific method.

The *Grammar of Science* construed science as a paragon of socialist morality, the submission of individuality to the social good. Science, Pearson argued, makes us good citizens by cancelling our private selves. „The scientific man has above all things to strive at self-elimination in his judgments, to provide an argument which is as true for each individual mind as for his own". The highest aim of education is to infuse students with an understanding of scientific method. In this way, the state fashions ideal citizens, people who can „form a judgment free from personal bias".[9] We should not exaggerate the humility of Pearson's stance. This form of self-effacement tended to exalt men like himself, men whose command of method enabled them to stand above personal, selfish concerns. They were able to speak not merely in their own names, but as mouthpieces for a disembodied science. They did so, however, not simply because they had acquired specialist, technical knowledge, but above all because they had sacrificed selfish interest to public standards and impersonal knowledge.

8 Karl Pearson, The Ethic of Freethought, in: The Ethic of Freethought and Other Essays, London 1888, 19–20.

9 Karl Pearson, The Grammar of Science, New York 1957 (reprint of third ed. 1911), 6, 8.

It turns out that Pearson's philosophy was most influential among American social scientists. The escape from interestedness that he preached was precisely what they needed to gain credibility for their knowledge. Social knowledge in the societies of Europe has, at least until very recently, been largely the property of elites, who have had enough credibility of their own that they were not obliged to insist on their relentless objectivity. Denial of the personal was more important in the more democratic and less trusting political culture of America. Pearson's philosophy suited American practitioners of the newly-emerging social sciences very nicely. And his contribution was not limited to philosophy. He also worked out a set of statistical methods through which this objective knowledge might be attained. Statistics came to stand for knowledge that stood above human interests because it arose from an impersonal, almost mechanical, method. The personal judgment of social scientists, like that of accountants, did not count for much. There were powerful reasons to renounce their individuality and to accept the discipline of what they liked to construe as largely mechanical methods for drawing conclusions from data.

This is how the disciplines that were most avid to adopt statistical methods generally wanted to understand them: as uniform, mechanical methods; as a block to dishonesty, incompetence, and wishful thinking. It sometimes surprises social scientists to learn that these rigorous methods of inference were not adopted first of all by the so-called hard sciences, such as physics, but rather by social and biological ones. Inferential statistics, like descriptive statistics, was not the sort of rigorous, expert knowledge that had to be comprehended by basic science before it could be put to work in applied ones. It was naturalized by applied disciplines, and then gradually made its way to pure ones. In medicine, for example, statistical methods were allied most closely to therapeutic testing. Really they were as much a part of public policy as of scientific research, since they were bound up with efforts by the American bureaucracy to regulate the practice of medicine and to control the activities of drug companies. Regulators needed objective tools because discretion and judgment, in this contested domain, were difficult to distinguish from arbitrariness.

In psychology, the crucial subdisciplines for the entry of inferential statistics were parapsychology and educational psychology. While parapsychology had little to do with public policy or bureaucratic regulation, its claims were so suspect that it needed any evidence of objectivity it could muster. The use of statistics in mental testing was bound up with a transformation of schools, the sorting of hundreds of thousands of relatively undifferentiated pupils and the assertion of authority by principals over mere teachers.[10] Here, as in medical testing, these relatively standardized measures served to increase the power of outsiders, like bureaucrats from the *Food and Drug Administration* and educational administrators, and to disrupt the autonomy of those closer to the scene of action, doctors and teachers. At the same time they helped to relieve the suspicions of other outsiders, such as parents, who might think their child should be promoted more quickly or be admitted to a selective university.

This reliance on external standards to justify choices against meddling outsiders is typical of what I call the culture of objectivity. It is a culture of suspicion. It tends to create a kind of Foucaultian world, in which everybody watches and everybody is watched. Or rather, it is part of an attempt to relieve the grounds for suspicion by reducing opportunities to exercise discretion. Statistical analysis is supposed to be largely self-enforcing, if researchers can be supposed to be minimally honest so they will not just make up the data. That is, if the conclusions allowed to researchers are simply a matter of applying accepted tools to solid data, then they cannot very well fudge their results, and we outsiders lose most of our basis for suspicion.

Of course hardly anybody has ever thought it was that simple. But many researchers, really whole disciplines, have accepted this as an ideal, and have worked diligently to approach it. One way of doing so is to rely on automatic instruments – to replace subjective evaluations with physiological readings of pulse, electrical conductivity, and the like. Another is to enact rules for analyzing these numerical readings. This has been one of the main roles of statistics. And yet, as with cost-benefit analysis, it has not been easy to reach uniformity in the methods of statistics. Almost from the beginning, statisticians have disagreed, often vehemently, about the best methods and their proper interpretation. Karl Pearson's statistics was allied to observation rather than to experiment, and he was never very keen on the small-sample methods development by Student (W. S. Gosset) and by R. A. Fisher. Worse, Fisher's brilliant alliance of statistical inference with experimental design in the 1920s and 1930s was soon challenged by Jerzy Neyman and by Pearson's son Egon. Neyman-Pearson statistics was developed as a rival to Fisher's. To this we can add the recent criticism of so-called Bayesians. For a social scientist in the mid-twentieth century, seeking to import the rigorous methods of statistics into his or her discipline, it was not quite clear which school provided the proper foundation. ‚The statistical method' as something unitary was

10 Kurt Danzinger, Controlling the Subject: Historical Origins of Psychological Research, Cambridge 1990.

in many ways a myth, sustained by social and medical researchers who did not know too much mathematics. Psychologists, sociologists, and economists wrote their own statistical textbooks. These presented a composite or eclectic viewpoint, mostly either Fisherian-experimental or Pearsonian-observational, which was put forward simply as statistics. Students were taught that there was one way to analyze data, and that to depart from the protocol was either sloppy or dishonest. This move was consistent with aspirations that had guided statistics for a long time. But it is important to recognize that making objectivity was an active quest, not merely a matter of accepting firm standards provided by the more mature or ‚harder' discipline of mathematics.[11]

To say this is not to deny that psychologists and sociologists turned to statistics for good scientific reasons, reasons related to their subject matters. They used statistical methods to study populations and to permit inferences from collective subjects. But we must also recognize that in some very interesting ways the problems of the various disciplines have been redefined, and their subject matter sometimes narrowed, to accord better with statistical approaches. My concern here is not simply the use of tools designed for collective subjects. It is the insistence on unitary, almost mechanical, strategies of analysis, as if scientific reasoning could be reduced to a formula. Statistics does not have to be conceived this way. Science does not have to be conceived this way. The social sciences have been more self-consciously scientific than the natural sciences in part because of their intellectual disunity. Relatively weak disciplines are like societies of strangers, where everybody is, in a way, an outsider. In such conditions, informal ways of settling differences and reaching understanding do not work very well. Formal rules of what people like to call „scientific method" have been made up to fill the gap.

I would not imply, though, that the spirit of quantification is totalizing or hegemonic. Certainly there has been resistance, and at least until recently the quantifiers have generally been in a weaker position than their opponents. Government by quantitative rules was not a prevailing ideal in traditional societies, and it is far from dominant even now. The political power of aristocracy or of a stable class of political elites has long stood as an alternative and a threat to the culture of objectivity. In Europe, at least, the advance of numerical methods did not introduce the heightened power of experts into a more egalitarian or democratic system, but attended the weakening of old elites. The French social researcher Frédéric Le Play explained in 1885 that statistics are not really needed in states

11 Gerd Gigerenzer et al., The Empire of Chance: How Probability Changed Science and Everyday Life, Cambridge 1989.

with a hereditary aristocracy, whose members have been raised to govern and can do so almost by instinct. But in his own day, he thought, people with no practical experience in public affairs could rise to high office, and statistics might help them to compensate for these shortcomings.[12] Le Play believed that much quantitative knowledge is rather superficial; that deep understanding is really better, but since it is increasingly lacking, we may have to make do with statistics as a poor substitute.

The British political philosopher Michael Oakeshott, writing in 1947, put this somewhat more darkly. The rationalist, he said, is „a foreigner or man out of his social class (...), bewildered by a tradition of which he knows only the surface; a butler or an observant house-maid has the advantage of him.“ Does this mean that rationalists must be ineffective? Oakeshott thought not, alas. Rationalism was spreading through society, eating away its rich inwardness and leaving only surfaces. That is, it destroyed precisely what was too deep for it, and left behind just the part, the least valuable part, that it was capable of understanding. Rationalism, and Oakeshott would certainly have included quantification in this, is a method and an outlook suitable for dealing with the world that it has constructed. Rationalism was none the less shallow for all that, since it never understood the world we are losing.

A quite similar critique of quantification has come from the left, especially what might be called the cultural left. The philosophers of the Frankfurt school thought little of science, and least of all of a style of science that is content to describe surfaces rather than probing for deep meanings. Max Horkheimer and Theodor Adorno argued that positivist science replaces „the concept with the formula, and causation by rule and probability.“ In his autobiographical reminiscences, Adorno thought back to the time when, after emigrating to America, he supported himself by joining in studies of radio organized by the emigré Austrian archquantifier Paul Lazarsfeld. „When I was confronted with the demand to ‚measure culture‘, I reflected that culture might be precisely that condition that excludes a mentality capable of measuring it.“ Happily for the project, but unfortunately for the world, this kind of culture seemed to him to have been obliterated by capitalism. „It is a justification of quantitative methods that the products of the culture industry, second-hand popular culture, are themselves planned from a virtually statistical point of view. Quantitative analysis measures them by their own standard.“

12 This paragraph and the two to follow come from Porter, Trust in Numbers, see footnote 2, chapter 4.

We need not reach so negative an assessment of the culture of quantification as these critics to recognize with them that its significance reaches far beyond the technical domain of science. An insistence on numbers contributes to the disciplining of research, and at the same time involves the imposition of new forms of order on the polity. It is associated with a culture that places little confidence in deep understanding – or should we say the pretension to deep understanding? – and distrusts what cannot be made explicit and reduced to rules. It reaches for universal standards of efficiency and rationality. In its effort to escape from culture it has gone a long way toward defining one. This is not to say that the modern methods of quantification are evil or false, but rather that both their preconditions and their impacts go far beyond the technical into the domain of morality, society, and governance.