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Is Statistics a Science?

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[The Chairman's Address to the Medical Section of the Royal Statistical Society, delivered at their 201st meeting, on January 31st, 1978]

"Thought is primarily practical; and only in the second place theoretical . . . without theory there would only be a few rudimentary types of practice, but without practice there would be no theory at all."

Collingwood, *The New Leviathan.*

Summary

A distinction is drawn between science and technology. The former has been studied by Karl Popper and his ideas are widely accepted; the latter has had less attention. The inter-relations of the two are considered and it is pointed out that neither can be said to underlie or dominate the other though practitioners of each are often intolerant of the alternative approach. Statistics may have a more important role to play in technology than in science; it may itself best be considered as a technology rather than as a science. These ideas are discussed in the context of the teaching and practice of statistics in general and medical statistics in particular.

Keywords: POPPER; COLLINGWOOD; SCIENCE; TECHNOLOGY; OVERLAP OF CONCEPTS; THE AND PRACTICE

The received wisdom concerning the nature of science and its methodology is, of course, that elaborated over the past forty years by Sir Karl Popper. Popper's work (besides being written in an English of a clarity and grace to shame most native speakers of the language) is of great historical interest. It arose, as he clearly states, as a direct reaction to the realization that Newton's theory of gravitation was incorrect and had to be replaced by that resulting from Eintein's general theory of relativity. The impact of this discovery is not too easy for us to realize today. Even in my schooldays, special relativity had been made comprehensible by Eddington and Born, and if Riemannian geometry was a bit beyond the sixth-form syllabus the overall drift of general relativity was becoming fairly clear. It was not so much that Einstein was right, as that Newton was wrong, that put the cat among the pigeons. Newton's theory had seemed to represent true knowledge, secure, tested and unassailable, attained by man for the first time in his entire history and the foundation for unforeseeable, unlimited advances; if this crumbled, what was there left? As always, the poets see clearest—

Nature and Nature's laws lay hid in night;
God said "Let Newton be!" and all was light.
It could not last; the Devil shouting "Ho!
Let Einstein be!" restored the status quo.

Popper's solution to the problem is too well known to need more than the briefest summary from me. Man, he claims, cannot know the truth (and even if he could, he could not know that he knew it). But science is the search for truth; how then can it proceed? The answer, which seems to me to represent one of the relatively few solid advances in the history of philosophy, is that science can proceed by constructive self-criticism. We do not know, we cannot *know*, that Einstein's theory is true, indeed we can almost be sure that it is not; but we can be quite certain that it is nearer to the truth than Newton's theory. This is because Einstein can beat Newton on his own ground; the triumphs of Newtonian gravitation, from the fall of the apple to the discovery of Neptune, are equally well explained by general relativity, but Einstein succeeds where Newton fails, in such crucial tests as the perihelion of Mercury or the gravitational bending of light.

On the Popperian model, then, science grows by framing hypotheses and subjecting them to tests of ever-increasing severity. Progress is achieved by the fact that each successive hypothesis has initially to pass the same tests as its predecessor, and then at least some of those that its predecessor failed. This is a very different story from that told in the older textbooks, where science's job was the framing of general laws derived by induction from a multitude of particular facts. Popper's views on induction are again very well known; he claims that he has solved, or rather disposed of, the problem of induction by asserting that the inductive derivation of generalizations from particulars is no part of science. In his approach, the generalizations come first, and the particulars, the observations used to test the generalizations, cannot even be thought about sensibly until the generalizations have been formulated.

It is not to be expected that Popper's ideas should have been accepted *in toto* by the philosophical community, though there seems to be remarkably widespread agreement that they represent a major advance on what had gone before. The most influential criticism, from Lakatos and Kuhn for example, has tended to use them as a foundation for further advance. But it is even more remarkable how well accepted Popper's ideas have become in scientific circles. It used to be notorious that most practising scientists were simply not interested in the philosophy of science, and those who were (with a few outstanding exceptions such as Eddington) were either not very good scientists or not very good philosophers or both. Things are different today, when a scientist of the standing of Sir Peter Medawar can propagate and contribute towards Popperian views, and when it sometimes seems difficult to open a scientific journal without finding these views under discussion. For the first time for some centuries, there appears to have grown up a measure of fellow-feeling between the scientist and the philosopher, a feeling that the philosopher is not only sympathetic towards the scientist's activities but knowledgeable about them as well, and may indeed have something to contribute towards making these activities more productive.

I am no philosopher and (as will appear) I am not at all sure that I am a scientist, but I think that statistics in general, and medical statistics in particular, may throw light upon some aspects of Popper's work and may raise some issues which I feel that it is inclined to neglect. At first sight, of course, the theory is one which a statistician finds very attractive—indeed, I am inclined to suggest that many statisticians have been Popperians all their professional lives. In Fisher's *Statistical Methods*, Section 2, we read—

> "The statistical examination of a body of data is thus logically similar to the general alternation of inductive and deductive methods throughout the sciences. A hypothesis is conceived and defined with all necessary exactitude; its logical consequences are compared with the available observations; if these are completely in accord with the deductions, the hypothesis is justified, at least until fresh and more stringent observations are available."

Popper might hesitate over that "justified" but the passage does not seem to me to conflict with, or even to caricature, his views. Moreover, we as statisticians are very familiar with a crucial step which Popper himself is inclined to gloss over. This is the confrontation of the hypothesis with the observations. The topic is treated in Section 37 of his *Logic of Scientific Discovery* but it cannot be said that this is the best part of that remarkable book. To a statistician, the question "Are the data consistent with the hypothesis?" is the one that he answers—or attempts to answer—whenever he undertakes a test of significance.

If scientists of repute find value in Popper's theory and if statisticians used to significance testing find that they are implicit Popperians, what is it that I am proposing to criticize? There are two spots that seem to me to itch. First, it is widely agreed among statisticians (if less so among the more naive users of statistics) that significance testing is not the be-all and end-all of the subject. The committed Bayesian will have nothing to do with it and the more conventional will usually urge the greater importance of estimation and other approaches. Moreover, it is by no means unknown for scientists to belittle the role of statistics in their work—the remark "If I need a significance test, I know I've done a bad experiment" is sadly familiar.

More importantly, the Popperian vision of the scientist as the devoted seeker for truth applying tests of ever-increasing stringency to his theories does not seem, on consideration, to be an entirely realistic picture of the activities of many of the colleagues with whom I have collaborated over the years. As one example, I have been working for a year or so with a team of 12 European laboratories concerned to develop an accurate and precise method of measuring calcium in serum. A good deal of money has been spent, a large number of experiments has been done and some quite pretty problems of design and estimation have arisen, but any hypotheses tested have been downright trivial, the more obvious ones being actually misleading. The outcome looks like being not so much a contribution to truth as simply a methodology that will provide determinations with a measurably greater accuracy than that previously achieved by a given expenditure of laboratory effort. Another example comes from the huge clinical trial now proposed of the treatment of mild-to-moderate hypertension. Previous work has shown that control of severe hypertension reduces the incidence of strokes and, the two conditions being divided at a quite arbitrary point, there is little doubt that the control of the lesser condition will do so too—to some extent. The question to be answered is not whether the treatment will have an effect, but whether the effect will be great enough to warrant the cost, in a generalized sense. Neither of these two efforts seems to me to fall naturally into the Popperian framework.

The background of ideas that I shall use is provided by another of my culture-heroes, the late R. G. Collingwood. Collingwood is a lesser figure than Popper, indeed he may be mainly known today for the discussion of his views on history in Chapter 4 of *Objective Knowledge*. The two men are both notable for their immensely readable prose and for a burning belief that the topics they discuss, however inadequately, are not parlour games but matters of vital importance to the society within which they live. I resist with difficulty the temptation to outline some of Collingwood's political and social ideas and confine myself to recommending anyone concerned about Western civilization and its discontents to read *The New Leviathan*, *The Principles of Art* and the bad-tempered but brilliant *Essay on Metaphysics*. For now I want to take up one or two other points. First, Collingwood insisted, in a very Popperian spirit, that reasoning is not so much a matter of propositions *per se* as of questions and answers, so that the truth of a statement cannot be properly assessed unless the question to which it is intended to be an answer is properly ascertained. The moral of this is not to be too quick to accuse a writer of being wrong or of talking nonsense; it is not always easy to find out what questions he was asking himself, and they may not be the questions that you would like to have answered. Secondly, the methods of philosophy differ from those of natural science. In particular, while scientific categories are mutually exclusive, the categories of philosophy overlap; I may categorize motives into those of pleasure, right and duty, but this does not mean that a right act is necessarily unpleasant or that an act of duty must be against the law. Ignoring this leads to one of two fallacies, magnificently named by Collingwood the "Fallacy of Precarious Margins" and the "Fallacy of Identified Coincidents", according as we try to draw precise boundaries where no precise boundaries exist, or assume in the absence of precise boundaries that no distinctions can be made. A philosophical distinction, says Collingwood, is always a distinction without a difference.

With all this in mind, let me return to Popper and ask again why it seems that his description of science and the scientific method does not seem to fit much that goes on in scientific establishments. Perhaps these establishments are not appropriately named; perhaps what I and my colleagues have been involved with all these years is not really science, not really what Popper is talking about. If this is so, it would explain why what he says does not seem entirely to the point; but the category into which these activities fall must substantially overlap the category of science in the Popper (or proper) sense if at first sight and in common parlance the two are so thoroughly confused.

The suggestion I want to make cannot be described as outlandish. I merely want to propose that much of what is commonly described as science comes more appropriately under the heading of technology. I could soften the blow to the point of imperceptibility if I substituted for "technology" the term "applied science". What I do think is worth discussion is the relationship between the activities described by these two terms. That such a discussion is not superfluous is suggested by the Dainton and Rothschild reports and the reception given to them in the scientific community. Both reports gave much of their space to the distinction I am trying to make and in doing so they seem to me to provide between them textbook examples of Collingwood's two fallacies; at least this shows that the point is not one of purely academic interest.

What then distinguishes technology from science? Let me put my own view as forcibly as I can. I hold that, in contrast to the scientist, the technologist is not concerned with truth at all. This sounds extreme, but I think I can justify it from recent and well-known experience. We all know that Newton's theory of gravitation is untrue; it is simply not the case that any pair of bodies attract each other with a force proportional to the product of their masses and inversely proportional to the square of the distance between them. What is more, it is untrue in a very fundamental way, not because the true exponent is, say, 2·01 instead of 2 but because the very notions of force, mass and distance as Newton used them do not stand up under critical attack. Yet the greatest technological achievements of the century, the Apollo moonflights, were undoubtedly based upon Newtonian calculations for their circumlunar navigation. Newton's theory may be untrue, even fundamentally untrue; but at the same time, it is, in a way, very nearly true, true *enough* as it were, true (as we say) for all practical purposes. Einstein may have swallowed Newton, but (as James Blish puts it) he has swallowed him alive. Again, a good deal of technology is based upon practically no theory at all. This applies, for instance, to the design of ships and to a surprising extent to that of aircraft, including the space shuttle. They may be based upon very extensive experimentation, but this is by no means the same thing.

That the technologist is not concerned with truth may sound a bit shocking but it should come as no surprise to a Popperian. The mark of the technologist is that he must act; everything that he does has some sort of a deadline. He has to manage, therefore, with as much truth as is available to him, with the scientific theories current in his time, and if these are subsequently proved to be untrue (the fate of all scientific theories), or are even known already to be so, he must still do the best he can. One of his tactics is embodied in the splendid institution of factors of safety, where structural members are carefully designed to take the known stresses according to the best available theories, and are then made 100 per cent or so stronger again just in case the theories are not quite right.

If science is the search for truth, then the life of science is research and a scientist is by definition one who is engaged in research. On the other hand, there is no particular reason why a technologist should do research. He need not be an innovator at all, and even if he is he may well be concerned with new applications and combinations of existing techniques or in their improvement, rather than in the discovery of genuine novelties.

There is, however, no reason why a technologist should not do research. If an engineer is someone who can do for five bob what any fool can do for a pound, a good engineer will always be wanting to cut the cost to 4s. 9d., and because nobody knows what he can do until he tries, it will be necessary to test possible new techniques experimentally. But it will be noted that there is a large difference of principle between technological and scientific experiments.

We have arrived in fact at what constitutes—what must constitute, if Collingwood is right—the essential difference between scientific and technological research, the difference in the questions that they ask. The questions of science demand the answer "yes" or "no" —does or does not the theory stand up under test? As statisticians, we know that things are not quite so easy, that the answers "maybe" or perhaps "probably" will sometimes be the best we can get, but the form of the questions is unchanged. The questions of technology are seldom of this form. Almost invariably they are of a quantitative nature calling for some kind of numerical answer. The commonest agricultural experiments, for example, are fertilizer and variety trials. In neither of these is there any question of the population treatment means being identical (as opposed to "quite close together"—remember that identity, not approximate equality, constitutes the null hypothesis); the objective is to measure how big the differences are. Exactly the same conclusion can be drawn from the widespread use of response-surface designs in industrial experimentation; again, the question of zero treatment differences simply does not arise.

If this is the difference between science and technology, what (to avoid precarious margins) is the relationship between them? This seems to me to be a far more difficult question and one to which I am entirely clear that I do not have a satisfactory answer. Collingwood holds that overlapping categories belonging to a single concept are related in what he calls a scale of forms. Of two such categories, one will exemplify the nature of the concept in a fuller, more satisfactory way than the other; indeed, in a very Popperian way it will tend to subsume the other by solving all its problems as well as those peculiar to itself. When this happens, the relation between the two categories becomes one not merely of distinction but of opposition. From the viewpoint of the higher category, the lower fails to exemplify certain aspects of the concept and to this extent fails to exemplify it at all.

This works quite well when applied to the distinction between science and technology. Technology, in spite of what I have said, can be regarded as some kind of search for truth—it is true, after all, that the average effect of farmyard manure on maincrop potatoes is to increase the yield by about a ton per acre on average, though such truths have a way of becoming unavailable after a lapse of time—but in this respect it appears as the subordinate category to science. We are not surprised to find, therefore, that the scientist often regards the technologist as simply a second-rate kind of scientist—"a mere technician" is the phrase. Kuhn, for example, refers to the use of existing theory to predict factual information of intrinsic value. "Scientists", he says, "generally regard this as hack work to be relegated to engineers or technicians." Or earlier, here is Poincaré in *The Value of Science*:

> "The search for truth should be the goal of our activities; it is the sole end worthy of them. Doubtless we should first bend our efforts to assuage human suffering, but why? . . . If we wish more and more to free man from material cares, it is that he may employ the liberty obtained in the study and contemplation of truth."

The trouble is that the whole argument works equally well the other way round. Just as technology has something to do with truth, so science is commonly supposed to form a basis for action, indeed to provide the means by which we can achieve an ever-increasing prosperity. However, in its proper business of searching for truth, science is notoriously careless about costs and consequences. We thus find the engineer contemptuous of the scientist—now the phrase is "a mere academic"—whose disregard of practical matters lies in his view somewhere between incompetence and irresponsibility.

It is certain that the outcome of this split is disastrous. It can develop in one (or both) of different directions. The two may go their own ways regardless of each other, the technologist cutting himself off from possible progress by refusing to keep up with the advances of science (muttering to himself "what was good enough for my granddad is good enough for me") and the scientist closing his eyes to the needs of his fellow-men and denying responsibility for the uses or misuses to which his discoveries are put. Alternatively, each may ape the other. The technologist puts on his gown and attempts to demonstrate that his efforts can be as useless as the next man's; the scientist (with Francis Bacon whispering "knowledge *itself* is power" over his shoulder) assures the grant-awarding agency that next year, or only a little later, some kind of practical application of his theoretical work is bound to turn up. The whole thing can be studied in detail, speeded up like a time-lapse movie, in the history of computing in Britain. In my view this split is far nearer the roots of this country's economic troubles than the more orthodox split between the Two Cultures à la C. P. Snow.

It is normally assumed that technology depends upon science in that the business of technology is supposed to be the devising of methods for using the discoveries of science, and this is certainly true to the extent that the good technologist will use all the scientific tools available to him, plus all the hints for developing new ones. What is false is the assumption, tacit in the phrase "applied science", that this is the whole story. It is important to remember that many of the really big technological advances—fire; the wheel; the domestication of the common farm crops and animals; the reef and bowline knots—took place before science was even invented and have been competently exploited by societies to which the notion of pure science would be simply incomprehensible. Even today, as I have indicated earlier, there are large branches of technology whose work is politely described as empirical and whose links with science of any kind, let alone current science, are few and far between.

A better case can be made (with Roger Bacon) that science depends upon technology. From society's viewpoint, science is a luxury, or at best a long-term investment of capital, and as such simply cannot be afforded until that society is sufficiently technologically advanced to exploit its environment efficiently. When Einstein said that the best occupation for a scientist was that of a lighthouse keeper because it would give him time to think, he was presupposing all the technology that built and maintained the lighthouse, plus that involved in manufacturing paper and pencils and in printing books and journals and delivering them to the foot of the tower. But, more fundamentally, the critical phase of scientific advance is mediated by technology. This phase consists in the confrontation of theoretical predictions with reality, and as theories become more sophisticated and the differences between their predictions more minute, so technological advances are needed for the confrontation to be in any way decisive. Just as the deadly sin of the legislator is to pass a law that he does not know how to enforce, so the deadly sin of the scientist is to ask a question that he does not know how to set about answering; it is technology that widens the range of answerable questions. A glance at the letters to *Nature* will show how much of current scientific work not only depends upon, but actually originates in, such technological novelties as computers, lasers, radiotelescopes and gas-chromatographs.

In reality, of course, the relationship has to be some kind of complementarity—of co-inherence, to use Charles Williams' useful word. I have already stressed the dependence of science on technology, and the reverse dependence, though far from absolute, is no less clear. It is liable to be illustrated whenever anyone says: "What we need in this field is some new ideas"—usually this means that piecemeal advances in technique have been pushed to their limit and some unifying principle is needed to suggest new ones. This state of affairs is not unknown in several branches of medical research.

It may well be asked why these issues should be raised at a meeting of the Medical Section of the Royal Statistical Society. I can offer several reasons. First, I think that the topic is important. I have mentioned Dainton and Rothschild; it seems to me quite clear that those responsible for directing our affairs, both within and without the scientific community, are at least as confused about it as I am and if you share my view of its importance this is a distressing state of affairs. I felt this particularly acutely after being the employee successively of two Research Councils, organizations which seemed to me on the whole to have struck the balance between science and technology more effectively than any others that I knew of and which were under attack for what I thought were misguided and inadequate reasons.

In the same vein, those who educated me did not seem to have paid any attention to the distinction I am making, or if they had they seemed to take the common line of assuming a straight dichotomy with science on the top; and no great change seemed to have taken place by the time my children came to be educated.

Medicine comes into the picture as containing some of the problems I have dealt with in particularly acute forms. There is much argument as to whether medicine is really an art or a science, but I think this is misplaced; it is precisely that intimate blend of knack and know-how based on solid theoretical foundations that I think of as technology. Moreover, the problem of balance has been solved more successfully in medicine than anywhere else I know. Medical science is prestigious, well supported, even reasonably well rewarded, but both its organizers and its practitioners are fully conscious that it is ultimately at the service of medical practice; medical practitioners at all levels are expected, both by their fellows and the public, to keep abreast of the advances in medical science and are provided with the means to do so, such as the weekly journals.

All the same, medical research does illustrate the problems that arise when the differences between science and technology are glossed over. These have been most clearly thought out by Schwartz and Lellouch in their book on clinical trials. My distinction between science and technology corresponds almost exactly to theirs between the explanatory and pragmatic approaches. They show, in my view conclusively, that the two approaches lead to quite different experimental methodologies, to the extent that an attempt to mix the two will usually lead to experiments from which no useful conclusions at all can be drawn. It is possible to argue that a purely scientific clinical trial is normally to be ruled out on ethical grounds, and if this is granted, then the continued assumption, by both doctors and statisticians, that the proper conclusion of such a trial is a statement of significance is one that seriously needs reconsideration.

But, most of all, as a professional statistician I think that the distinction and the balance between science and technology are vital matters for our profession as a whole. I referred earlier to the paradox that on the one hand certain aspects of statistics appear to be tailor-made for the scientist's use, while on the other these same aspects are sometimes regarded as suspect both by the scientists and by the statisticians themselves. Let me set against this the whole-hearted acceptance of statistical methods in many branches of technology and the fact—for so I would regard it—that most of the major advances in statistical methodology have taken place against a technological background, in organizations such as Guinness's Brewery, Rothamsted, I.C.I. and Bell Telephone Laboratories. If the newly qualified statistician does not instantly enter upon a teaching career, he will find that many more technologists than scientists will be anxious to take advantage of his skills. This has important consequences when he or his teachers come to decide what those skills should be. If technologists demand quantitative rather than yes–no answers, the fundamental basis should be an approach and a knowledge of techniques, such as experimental design and linear models with their generalizations, which concern themselves with providing such answers.

But the link between statistics and technology is closer than that. I have been careful so far not to give a formal definition of technology, holding with Collingwood that definitions come at the end, not the beginning, of a philosophical enquiry; but I have perhaps come far enough to venture a working definition to argue about. Let me try out "the efficient achievement of pre-defined ends", leaving the word "efficient" to beg the important questions. Now research workers in both science and technology define ends which they wish to achieve and they need technology to achieve them. As there are specific pieces of technology depending upon the problem under study, so I propose that there are general technological principles that are applicable across wide areas of research; these principles, the technology of research itself, consist of statistical methodology. As statisticians, certainly as applied statisticians, we do best to think of ourselves not as mathematicians, nor as scientists, but as technologists.

Again, if true, this is important. As research technologist, a statistician will be involved in research, and if he is a good statistician the research will be to that extent more effective.

But there is no particular reason why it should be statistical research. Taking medical statistics as an example, some medical statisticians may be involved in the development of new and better methods for the acquisition and analysis of medical data, but they may be outnumbered by those whose main business is to apply methods developed by others to specific research problems. There may not—should not, indeed—be any sharp dividing line between the two. It is next to impossible to do good work in statistics without first-hand knowledge, preferably well up to date, of applications and the best possible discipline for a statistician, one uniquely available to him in the scientific community and one which is the peculiar glory and challenge of his professional life, consists in bouncing his statistical ideas off the critically receptive minds of colleagues whose specialities are not his own. He can be confident in advance of a sufficiency of challenge, of occasions when the textbook methods, for all their plausibility, do not quite fit the problem at hand, or when he and he alone is equipped to discern that the methodological barrier blocking progress in one field of application has already been scaled in another. If this is to be a mere technician, I for one find it nothing to be apologetic about. And if I am concerned with assessing a fellow-statistician for employment or promotion and find that the bulk of his publications are collaborative and have appeared outside the purely statistical journals, I shall not feel like asking him (as I was once asked): "When are you going to get down to your own work?"

There are a host of other aspects of the science/technology split that it would be interesting to discuss. In the social sciences, for instance, Fisher's science-oriented dogma that "an experiment may be said to exist only in order to give the facts a chance of disproving the null hypothesis" has risen to the status of a Kuhnian paradigm, so that it is a legitimate and damning criticism of a research project to say that "it has not specified the null hypotheses that it intends to test". It would be interesting to discover whether this has happened because some older paradigm became untenable, or whether the custodians of the mysteries derived from recollections of their statistical education their ideas of how a properly prestigious science ought to be conducted. Had they instead been brought up on Popper's *Open Society and its Enemies* with its concept of social technology, they might have taken a different line; and, if Professor Barnard is right in saying that the regularities of the social sciences may be entirely transient, perhaps it might have been a more appropriate one.

Or again, at a more general level, we might focus on the concept of "enough". Science knows nothing of "enough". A theory cannot fit the facts "well enough"; any misfit, however small, must be taken as a hint for future research and only accepted if other priorities insist that this research be postponed. Once "good enough" is accepted, scientific progress comes to a halt. On the contrary, "enough" is almost a defining characteristic of technology. A structure must be strong enough to stand up to the stresses imposed on it, but it is bad technology to make it too strong. Quality control on the production line, in the pathology laboratory, or elsewhere, should ensure that accuracy and precision are good enough for practical usefulness; it is a misuse of funds to "improve" them beyond this point.

But finally, like all important issues, it comes down to a matter of morals. We live, they tell me, in a scientific world, and certainly in one whose every aspect is shaped by technology. Whatever the responsibility of the scientist—no Einstein, no Hiroshima, to put it as brutally as possible—there is no doubt that the technologist's activities are designed to have consequences and that he must consciously assume responsibility for these consequences, even the unforeseen ones—and there are always and invariably some consequences that are unforeseen; the fact that we act from incomplete truths ensures this. Like Ulysses in the *Commedia*, the scientist may pursue truth merely because it is there, in the comfortable faith that his is the highest calling of mankind. Not so the technologist. There is no arguing about it—no technology, no penicillin; no technology, no nuclear power stations; no technology, no napalm. It is not a load to be taken up lightheartedly. And the statistician, poking his numerate nose into the whole of technology, must realize that he has to bear his share of the load. The official portrait of the statistician as the impassive, unprejudiced fact gatherer is a very nice one, but we cannot escape as easily as that. The future shape of society, the future existence of society, is in the hands of the mere technicians. It will be well if both society and we technicians bear the fact in mind.

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