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SOILS, NATURAL SCIENCE, AND MODELS[1]

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We examine the last half-century of natural science, with special reference to its ethos and to changing public attitudes to the autonomy and accountability of the scientific community. The content of soil science places it uneasily between natural science on the one hand and the world of professional practice on the other. Very different attitudes to personal responsibility at the two ends of the continuum make for potential conflict. In recent decades the computer/modeling symbiosis has burst upon the scene. Current modeling practice fits more readily into the professional segment of the continuum than into the natural science segment. A disturbing aspect is that computer modeling has largely supplanted laboratory experimentation and field observation as the research activity of students. The future of soil science is contingent on how everyone's perceptions of natural science and of model validation evolve; it is the perceptions of soil scientists themselves which are most important.

The Editor-in Chief's invitation for this article reminded me that *Soil Science* has been an institution for most of this century and brought home to me also my own antiquity. My first paper in *Soil Science* was submitted when the journal was just half its present age. Lowell Douglas enjoined contributors to this special issue to "say what they think is important", with the hope that they would "challenge their fellow soil scientists". I shall strive to obey.

I have thought it might be useful if, in this contribution, I stand back at a distance from soil science and offer comment on the present (and possible future) course of natural science on the one hand and of modeling involving soils on the other.[3] The ongoing course of soil science is inextricably enmeshed with both.

NATURAL SCIENCE AND THE SCIENTIFIC COMMUNITY: THE IDEAL

When my adventures into soil-water research began in 1947, the world was a simpler place and most definitely a simpler place for scientists. There was a clear picture of the tasks of natural science and of the responsibilities and ethos of the scientific community.

At that time most scientists, had they stopped to think about it, would have agreed with the picture of the scientific community presented in the writings of, for example, Polanyi (1951), Bronowski (1951, 1961), Popper (1959), and Merton (1968). In this view, research is directed toward the extension of certified knowledge, with knowledge understood as empirically confirmed and logically consistent predictions. This regimen of observational confirmation and logical consistency is not only efficient but is believed to be right and good, a moral as well as a technical prescription. There stems from it an ethos based on four essential norms: universalism, communality, disinterestedness, and organized skepticism.

Universalism demands that race, nationality, or class have no bearing on freedom of entry to the scientific vocation or on judgments of the validity of a scientist's work; it thus comes into conflict with criteria of racial, national, and class membership. *Communality* requires that scientific knowledge must reside in the public domain. "Secrecy is the antithesis of this norm; full and open communication its enactment." The norm of communality is often in conflict with commercial and military interests. The norm of *disinterestedness* requires that the scientist's assessment of the truth of a scientific statement be uncontaminated by his interest in fame, reward, or the promotion of social interests. The public and testable character of science sustains this norm. Infrequency of fraud in science carries no implication that scientists are specially virtuous or free of self-interest. It is simply that they are subject to rigorous internal

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[3] The term "natural science" embraces both basic and applied aspects of the physical and biological sciences and is in contradistinction to the "social sciences".

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policing, to a degree unparalleled in other fields. External policing is seen by the scientific community as not only redundant, but inherently ineffective. The norm of *organized skepticism* requires that a scientist must ultimately accept nothing on trust, neither the work of his predecessors nor ideological pronouncements on either scientific theory or scientific goals. These four norms can be fulfilled only if the scientific community maintains adequate autonomy. If any of them is infringed, scientific work loses effectiveness and integrity.

You may well see this picture as a faded brown snapshot from a distant past that bears no relation to present-day reality. It is, of course, an ideal towards which it is increasingly difficult (and in some degree unrealistic) to aspire. But I do not invoke this ethos of science as an empty exercise. It stays with us as the standard, and deviations from it are a warning of danger. When it ceases to hold the loyalty of the leaders of science, the whole scientific exercise is in peril.

The scientific ethos evidently applies most unequivocally to research in well-established fields of natural science. But it has significance also for the practice of professions applying those sciences. The norms of disinterestedness and organized skepticism are prerequisite to innovative and impartial scientific and professional services. It is difficult to envisage high quality performance not founded on respect for the ethos.

AUTONOMY, ACCOUNTABILITY, AND RELEVANCE

I need scarcely remark that, over the last four decades or so, many threats to scientific autonomy and scientific norms have arisen. A major one is the ubiquitous argument that, since society at large, predominantly through government or industry, provides the resource for science, science is there to obey society's demands and to be responsible, accountable, and relevant.

This claim of society cannot be gainsaid: nor can the need of the scientific community for autonomy. There is potential conflict between the reasonable expectations of society and the no less proper needs of science. In part the conflict arises from misunderstanding, and this misunderstanding, to some degree, stems from the failure of scientists to explain adequately to society how science works, to give laymen a feel for the environment conducive to creative and productive research. All too often today scientists seem forgetful of their calling and submit passively to being overmanaged into a state of creative impotence.

Let us look at some sources of the difficulty. To begin with, let us be quite clear: autonomy of the scientific community carries no implication of indifference to practical problems. Indeed, any scientist worth his salt is on the alert to identify practical problems within his present and potential range of competence, and to extend his work in those directions. In the happy circumstance where the scientist's judgment of the importance and the solubility of a problem agrees with that of his patrons, his work is seen to be relevant. Difficulties may arise, however, when, on the basis of his special knowledge of the scientific issues, he is obliged to disagree with his patrons on the feasibility, efficacy, or practical significance of a scientific project.

The concern of scientists with the solubility of a posed problem is a frequent source of irritation to nonscientists. Scientific research is "the art of the soluble" (Medawar 1967): there are no prizes and no thanks from society for the scientist who spends his time and the resources of society in the attempt to solve problems beyond his competence or, indeed, problems beyond reach of the methods of natural science.

There is another source of misunderstanding of the scientific community. A significant, well-run, research project may require 5 years or more, a period often longer than the tenure of politicians, CEO's, and bureaucrats and, indeed, the duration of "ideas in good currency" in society (Schon 1971). Failure to grasp this can lead to a simplified view of accountability, the dangers of which must be recognized.

These considerations do not argue against the accountability of scientists to society, but they do underscore the need to examine very carefully what it entails and just how it is to be exercised. How narrowly should accountability be interpreted? And does it make sense if the auditors in the matter lack an appreciation of what science is, of how it works, and of what it can and cannot achieve? The failure to grasp this point is epitomized in the common complaint, "If scientists can put men on the moon, why don't they cure cancer?" The ingredients of the prescription for putting men on the moon were the requisite scientific knowledge, which was well known, mostly from the 19th century and earlier, and the funds for the needed technology. As for the cure of cancer, the funds for any needed technology are doubtless available, but the required basic biological knowledge is simply not at hand. Scientists themselves have been at least partly to blame for this misunderstanding. The limits of science have not been much dwelt on by its propagandists in the years since World War 2.

On the other hand, preoccupation with soluble problems must not become an excuse for timidity on the part of scientists. As Ortega y Gasset (1944) said, "Life cannot wait until the sciences have explained the universe scientifically. We cannot put off living until we are ready." This raises issues concerning immature and ineffective fields of inquiry and the related matter of "trans-scientific" questions. Two relevant problems have been discussed by Alvin Weinberg. He has written about the ripeness (or otherwise) of a field of science for attacking problems (Weinberg 1963); and in Weinberg (1972) he discussed the category of questions which "can be stated in the language of science [but which] are unanswerable by science". Most scientists accept the responsibility of bringing their expertise to bear on the trans-scientific questions raised by society, but it must be understood that when scientists do this they are straying into uncharted territories outside the well-ordered public realm of certified knowledge. As Dr. Weinberg has said, "The most science can do is to inject some intellectual discipline into the republic of trans-science". To claim more for science in this context is to misrepresent it and to arouse false expectations.

MARXISM PERSISTS (EVEN IF COMMUNISM IS DEAD).

The potential mismatches between the scientific enterprise and society at large are much exacerbated by a shift in the public values of Western society over the last half-century. As we shall see, the West seems to have been overtaken by elements of Marxism, even if the most eager protagonists of the new mores see themselves as stout (and now victorious) warriors against Marxism and its evil ways.

Karl Marx imagined he was a friend of science, but his pronouncements have done it much damage. As Bertrand Russell (1951) put it: "Science used to be valued as a means of getting to *know* the world; now [after Marx] . . . it is conceived as showing how to *change* the world." The actual words of Marx (1845) were: "Philosophers have only *interpreted* the world in various ways, but the real task is to *alter* it." (For Marx's "philosopher" read "scientist", a word not yet in general currency in 1845.)

It would be quite false, of course, to pretend that Marx was the first to value the technological uses of science. Indeed the founders of the Royal Society were concerned with the implications of their science in the practical arts as much as with knowledge for its own sake. Bacon, with his experiments of light and his experiments of fruit, had it both ways. But Marx was the first to set up the quest for power as the prime purpose of science.

Unsurprisingly, this Marxist view of science is immensely attractive to people dominated by the love of power, and this holds good regardless of their nominal political convictions. This scientific Marxism is more readily accepted and, I think, more prevalent outside the ranks of practicing scientists than within them. But, in a world where politicians, administrators, and literate laymen are hooked on scientific Marxism, scientists who are *not* are under constant pressure.

There is no doubt that the Western world no longer holds in the highest esteem the one-time self-evident virtues of the life of the mind, the pursuit of understanding and the love of ideas. Today their place is taken all too often by simplistic materialism, the pursuit of economic advantage, and the lust for power. It is noteworthy that Eastern communism has lost out to the West, not in consequence of any unsuppressible yearning of the human spirit for freedom, but rather because Western materialism looks more efficient than the Eastern variant.[4]

In a percipient review Noel Annan (1988) charts the shift in values with special reference to the British scene. He attributes the Thatcherite enshrinement of things over ideas as, in part, a conscious effort to punish the intelligentsia for being snobbish about trade and unresponsive to the plight of Britain's moribund industries. A similar shift has certainly happened in Australia and also, insofar as I can see,

[4] After completing this article, I came on the remark of Max Perutz (1990) in a different but related context: "Marxism may be discredited in Eastern Europe, but it still seems to flourish at Harvard".

in North America and (perhaps in lesser degree) Western Europe.

MATERIALISM AND MANAGERIALISM

But influences beyond Mrs. Thatcher and her imitators have certainly been at work. One which seems all-pervasive and, in my view, profoundly damaging to all centers of intellectual activity, whether they be universities or government or industrial laboratories, is *managerialism.*

I have much enjoyed Harvard, and I shall be grateful always for the pleasures and benefits of my sojourns there. But I believe Harvard deserves much blame for the sad state of the administration of scientific research and academic undertakings throughout the Western world.

In 1908 Harvard set up the Graduate School of Business Administration. At the time it no doubt seemed a useful and worthy notion to bestow on the brutal world of commerce the refining influences of Harvard academia. But Harvard, like Frankenstein, has made a monster. By the 1940s management had become the thrust of the Harvard Business School, and management continues to be enshrined as its central theme and its gift to humanity. Well-scrubbed young MBA's march out across the world spreading a management doctrine based on the totally false premises that what is important about all tasks is what they have in common, and that the bottom line must be measured and expressed in dollars.

Even when the task is as obviously unique and peculiar as scientific research, these disciples are not diverted from their doctrine and their purpose: they simply outdo Procrustes in lopping and stretching the anatomy of research to fit a schema based on the freezing of peas or the bottling of beer.

The consequences for science of instituting managerialism are saddening to behold. Unnerved scientific administrators, cowed by their management consultants, dismember scientific teams of world stature and set up systems of line management. With rare exceptions, the top rungs are occupied by failed scientists, by flawed scientists who have abandoned understanding in favor of power, and by gray bureaucrats blissful in their ignorance of what science is about. Almost all have forgotten (or never knew) that "Research is not a hierarchical activity in which purposes are generated at the top and gradually refined and made specific as they filter down to the level of the bench worker" (OECD, 1971), and that in creative and productive research environments there must be as much freedom as possible at as low a level as possible.

Harvard's sin is to have conferred its academic authority and respectability on a trivial, damaging, and degrading approach to one of man's greatest enterprises.

THE SCIENTIFIC COMMUNITY: THE REALITY

There are many lacunae in the foregoing account of the difficulties science faces in today's world. Perhaps the most serious is that the reader might suppose from the foregoing that I see the scientific community as the blameless and misunderstood victim of a philistine world. Such a picture would be wide of the mark. The scientific community has brought many of its troubles on itself. I instance, in particular, the following: the distorted and inflated claims made for science over the last half of the 20th century; the dilution of the scientific community by an influx of persons attracted not by science as vocation but as a source of money and jobs; in relation to project assessment, and resource allocation, a damaging lack of concern for the originality, scientific standards, and productivity of teams and indviduals; and an equally damaging uninterest in quality control of scientific papers. These deficiencies have not gone wholly unnoticed. The report of the White House Science Council (1983) was very much to the point, though it seems to remain unheeded.

MODELS AND MODELING

We avert the gaze from the present state of research and "research management" to examine what goes on in the name of modeling. These days in the corridors of scientific power one never hears mention of "science", let alone "scientific standards". The buzz-words are "manager", "management", "decision-maker", "stakeholder", etc. The new vocabulary, however, admits some words that the innocent might suppose to have scientific content, and I think of "model" and "modeling".

Personally, I have trouble in comprehending how modeling seeks to do anything different from what natural science has been trying to do for at least 300 years. Perhaps, after all, Newton and Einstein were simply "modelers", and it may be that what sets them apart is that they were especially wise and especially humble (Philip 1975a). I was pleased recently to hear the Director of the Swiss Nuclear Safety Inspectorate affirm this congruence in the following words: "What has changed since is the jargon. In waste management rather than of theories we speak of models, and instead of testing theories we validate our models. But the essential concept and the ultimate goal have always been the same: To find truth" (Niederer 1990).

I was much heartened by this affirmation that models have to do with truth. That this was a conjunction outside my experience might be construed from what follows.

MODELS AND COMPUTING

It is no accident that the emergence of modeling is contemporaneous with the emergence of the electronic computer. The computer makes feasible calculations of a magnitude and complexity almost unimaginable in pre-computer days. Without doubt, the advent of the computer has opened up in significant and beneficial ways certain areas of natural science which depend on very heavy data processing and/or very heavy computation. Beyond this, the computer is the natural and almost essential and inevitable tool of the makers of very complicated models. These often purport to furnish means of predicting events in the labyrinthine, poorly understood, and ill-specified natural world around us. I have in mind models set up to deal with problems, such as those of land-use planning, agricultural production, forestry, water conservation, waste disposal and environmental pollution, and ecosystem assessment and management. Superpose on these models, if you will, the further dimension of climatic change, global or regional. Models for all these purposes will have (or at least should have) as important components sub-models of the relevant soil processes.

One manifestation of the computer-model symbiosis is that the two offer similar and intermingling pitfalls and temptations. These are indeed many and grave. I shall treat some of them in what follows, but with no implication that I have exhausted the list. I refer the interested reader to various writers who cogently discuss matters I neglect. Firstly, I urge you to read the chapter marvellously entitled "The computer: Ruin of science and threat to mankind" of Truesdell (1984). The illustrious mechanician, in his inimitably pungent style, explores the ultimate vacuity that can follow when man abandons using his mind in favor of the computer. Secondly, I commend to you Andreski (1972), in particular the chapter "Quantification as camouflage". Andreski addresses obscurantism and models in the social sciences, but the implications for the transcientific contexts that impinge on natural scientists are clear. Weizenbaum (1976) offers a human and humane commentary touching on both themes and much more. Berlinski (1976) dissects these issues further. *Inter alia* he discusses instability and failure of predictability in models and the "craving without content" for vast, all-embracing models. Finally, I refer to Passioura (1973), one of few remonstrations against the deficiencies of large-scale modeling specific to a context close to soil science.

MODELS AND THE REAL WORLD

From the viewpoint of natural science, and indeed from any viewpoint concerned with truth, a disquieting aspect of computer-based modeling is the gap between the model and the real-world events. There is reason to fear that the gap will not grow smaller and that worry about it may ultimately just fade away.

Of the making of models there is no end. The component parts may be well-founded in natural science, may be a crude simplification, or may be no more than a black box which has taken the modeler's fancy, and, equally, the prescriptions for fitting the components together may or may not be well-based. Beyond the question of the validity of the model's machinery, there are also difficult questions of the quality both of the parametrization of the components of the model and of the data inputs.

Many modelers, in their enthusiasm, seem not to be fazed by these problems but push on regardless. Many appear to be possessed by their models, to have little interest in the real-world processes, and to be oblivious to the unrealities of their parametrizations and input data.

A disturbing aspect is that computer modeling has largely supplanted laboratory experimentation and field observation as the research activity of both undergraduates and graduate students. (Computing is said to be cheap and other forms of research expensive. Its real monetary cost is often concealed by accounting systems friendly to computing, but it is the *concealed* *nonmonetary* cost which is troubling.) Most raw PhD's seeking a job at our Centre proudly present themselves as computer jockeys, incurious about real-world phenomena and innocent of laboratory and field skills, yet blissfully unaware of their inadequacy for serious research.

A recent international panel on hydrologic education (Nash et al. 1990) reports as follows:

> "One urgent educational problem which has reached crisis proportions in many universities, is the lack of field and laboratory experience. This is a problem at all levels and in many disciplines and has existed for long enough to become self perpetuating through the next generation of faculty. The consequences in hydrology are both profound and disturbing especially with the current emphasis on conceptual modelling. Although such models constitute useful tools in the investigation of the physical world, exclusive or undue reliance on them may tend to separate students from the realities they are supposed to study. In the absence of appropriate testing, models take on an aura of reality in the minds of their users and become a source of unsound science and practice".

This shadow-boxing surrogate for science is not, of course, confined to universities, as readers from government and industrial laboratories will know very well. But it is sad to see the young being canalized thus. They cannot be blamed. For them to develop otherwise would require a mental and moral integrity we cannot expect in young persons given open slather to play around with glamorous space-age toys. At this point I cannot resist quoting the dictum popularly attributed to Felix Franks, the British physical chemist: "Modelling is rather like masturbation—a pleasurable and harmless pastime *just so long as you don't mistake it for the real thing*".[5]

MODELS, NATURAL SCIENCE, AND TRUTH

Where does all this leave us relative to Niederer's affirmation that the ultimate goal is *to find truth*? I much admired Ueli Niederer's intellectual integrity and courage in parading the awkward word "truth" before an audience mostly of modelers. He moderated the challenge in some degree by pointing out that the search for truth in natural science is not without flaws and vaguenesses, and he invoked the viewpoint of Kuhn (1970) that "to put it simply and brutally, a scientific theory by definition is true if it has gained consensus among the experts of that particular science . . . . It is perhaps the greatest merit of Kuhn to have identified and analyzed this irrational but all too human component of science" (Niederer 1990).

Niederer went on to offer a Kuhnian consensus-based prescription for the validation of models, but he was careful to include in the minimal consensual group not only the modelers but scientists not directly involved, who by their scientific background are well able to judge at least some particular aspect of the model. This was an interesting attempt at reconciling, on the one hand, the criteria of natural science, and, on the other, the professional need for decision and action. In what follows we touch on the potential for conflict between the two.

PROFESSIONAL PRACTICE, MODELS, PERSONAL RESPONSIBILITY, AND DECISION-MAKERS

We revert to Ortega y Gasset's dictum. The world cannot grind to a halt until all the answers are in from natural science, and, indeed, were they in, there would remain vast and embarrassing trans-scientific gaps. The world's professionals (engineers, agronomists, foresters, planners, etc.) cannot afford to stand on scientific niceties; they must get about their business.

Engineering schools epitomize education for a profession. The message I took away from my engineering course in the 1940's was that all things were understood and that all a young engineer needed to know was what handbook to use. One fears that today we may have an updated version of the old message: "All things are understood, and all a young engineer needs to know is what software to use".

From the viewpoint of many professional practitioners that may well seem eminent good sense. It is the professional's rôle to get on with the job with minimum fuss and delay, and he is best able to do this without hesitation when he avoids personal responsibility (and, indeed, legal liability). Where there are laws, regulations,

[5] I thank Dr Franks for informing me subsequently that the original remark, phrased a little differently, was made by an unnamed French scientist at the NATO Advanced Study Institute on Interfacial Aspects of Phase Transformations, Erice, Sicily, August 23 to September 9, 1981.

standard specifications, handbooks, rule-of-thumb equations, and extant software and models, there is a clear road ahead for him. Should the exercise turn out badly, it is not his fault. He has merely obeyed the instructions of his bosses and the injunctions of his profession.

I need hardly remark that this mind-set is diametrically opposed to that of natural science (at least as an ideal) and is a most negative preparation for a research career. Many involved in soil science and kindred fields drift uneasily between the two camps of natural science and professional practice, and it is not surprising that some become uncertain in their values and motivations.

How can we resolve this conflict? I do not know. But it would be a beginning if the dichotomy were frankly recognized and comprehended, and if each camp were to understand the other's values and to accord the other its essential rôle.

Beyond the professionals sit the decision-makers. Understandably they are enthusiastic about models. Like the rest of us, they enjoy good news, and the news the messengers bring is that models, sanctified by the authority of the computer, will solve their problems. Decision-makers can thus join the throng of those ducking personal responsibility: their decisions are forced upon them by the pronouncements of the computer. Conversely, it is not unknown for an unscrupulous decision-maker to seek out models and modelers that give the answers he wants. This, of course, requires modelers skilled in adjusting the model to yield the desired result; there is no place for the unpredictable output of the less skilled.

THE NEXT 75 YEARS

What does all this suggest about soil science, and indeed *Soil Science*, 75 years from now? It is my fervent hope that both will still be in existence, and prospering; that the journal will still be a rallying point for the development of new morphological, biological, and physicochemical concepts specific to soils and for application in the soils context of insights and techniques from the whole gamut of natural science. But it may well be that by 2066 we shall be deep into the electronic Dark Ages. Both the science, and the journal, may have been supplanted by a battery of expert systems, some of them designed on the self-fulfilling premise that the user is an imbecile. Where we shall be depends on how everyone's perceptions of natural science, of models, and of truth, evolve; it is the perceptions of soil scientists which are most important.

ENVOI

The Editor-in-Chief's invitation said, "I want to give young scientists something to think about, possibly something to motivate them. Possibly some more advanced scientists might also take note".

There is, I suppose, plenty to think about in this diagnosis of the continuum in which soil science sits. And there are challenges aplenty. How to assure for research its proper milieu of freedom of action and spirit of intellectual adventure? How to do the best we can with the trans-scientific problems all around us without compromising our standards and our honesty?

But, from this jeremiad of an old man, motivation for a young scientist? Let me offer two negatives and then a string of positives. You should definitely not be in soil science because you expect to make money at it. Nor should you be in it just out of warm feelings for your fellow human beings. If, on the other hand, you have a burning curiosity about how and why things happen in that complicated and fascinating world out there; if you seek the delight of pioneering fresh understanding of the processes of that world; if you want the pleasure of using the concepts and techniques of some seemingly unrelated scientific field to solve a long-standing puzzle of the natural environment: then, young person, soil research can offer you more than a lifetime of fascinating problems to unravel.

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