John Ziman

"Postacademic Science": Constructing Knowledge with Networks and Norms

Science as a cultural form

A lot has happened to academic science since Peter Medawar taught us to see research as *The Art of the Soluble*, nearly thirty years ago (Medawar, 1967). So much has changed that a quite new research culture – "postacademic science" – is emerging. This paper is concerned with the philosophical impact of these changes.

Let me make it quite clear that I am not suggesting that present-day scientific results might be less secure, or less in accordance with the true nature of things, than they were thirty years ago. But even the strictest realist would agree that the progressive unveiling of nature is not a very systematic process. How far we have got in that process – that is, what counts as scientific knowledge at any given moment – is obviously influenced by how research is organised, who is involved in it, what they think they are doing, what is regarded as good work, and other similar considerations. In other words, some aspects of the philosophy of science cannot be disentangled from certain features of the current research culture.

Academic science

Academic science emerged in France and Germany in the first half of the 19th Century. Since then it has evolved into a characteristic social activity, and spread around the world. As the name suggests, it is typically associated with higher education, but is also found in a number of other institutional settings, especially under governmental patronage. It does not have any system of overall control, and although its practices and principles are remarkably uniform, they are not formally codified. For this reason, it is best thought of as a culture, in the anthropological sense, rather than as an organised structure.

One of the questions that can be asked about a culture is whether its practices, rules, traditions and conventions can be related to a set of more general principles. In 1942, Robert Merton (Merton, 1973: 267–278) suggested that academic science was governed by an "ethos" embodying a set of functional "norms", This type of sociological analysis is now considered very questionable, but it does provide useful pegs

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on which to hang a general account of some familiar *social* characteristics of academic science, and to relate them to some well-known *cognitive* features of scientific knowledge.

Elements of the scientific ethos

Let us go through the Mertonian norms one by one. The norm of *communalism* requires that the fruits of academic science should be regarded as "public knowledge". It thus covers the multitude of practices involved in the communication of research results to other scientists, to students, and to society at large. It is no accident, for example, that academic science is closely associated with higher education, or that academic scientists are concerned about the inadequacy of "public understanding of science".

This norm has a deeper significance. In effect, it enjoins the pooling of personal knowledge gained from individual experience. From this shared experience we infer the existence of an external world, on some of whose features we find that we agree. Despite all the arguments of doubting philosophers, scientists are instinctive realists – like most ordinary mortals.

The norm of *universality* requires that contributions to science should not be excluded because of nationality, religion, social status or other irrelevant criteria. In practice, this *multicultural* ideal is achieved very imperfectly. It does imply, however, that scientific propositions should be general enough to apply in any cultural context. This norm thus explains why philosophers of science focus on fundamental theories that claim to reduce and unify a wide variety of phenomena.

The notion that academic scientists have to be *disinterested* seems to contradict all our experience of the research world. What it means is that in presenting their work publicly they must repress their natural enthusiasm for their own ideas, and adopt a neutral, impersonal stance. Many academic scientists do not have to boost themselves to make a living, because they holdi permanent posts as university teachers, and undertake "pure" research without commercial applications. This is a very important norm, since it underpins the philosophical *objectivity* of academic science.

Originality energises the scientific enterprise. Academic scientists are not always inspired by curiosity, but they are expected to be "self-winding" in their choice of research problems and techniques. Their most cherished traditions celebrate and sustain this aspect of academic freedom. This is the norm that keeps academic science progressive, and open to novelty. For example, many philosophers of science stress the creative role of *conjectures*, that is, bold thrusts of intellectual originality, continually attacking the frontiers of ignorance.

Scepticism, on the other hand, is the normative basis for many academic practices, such as carefully controlled critical controversy and peer review. This norm is not a licence for systematic philosophical doubt, nor for total sociological relativism. It merely stresses the constructive role of *refutation* as the natural partner of conjecture in the production of reliable knowledge. This social mechanism thus tests the claims of academic science in terms of rational qualities such as logical and factual consistency.

The concept of a definite "scientific method" is now considered highly questionable. The most that metascientists will say nowadays is that science is a body of knowledge "regulated" by certain general principles. These principles are usually considered quite abstract and "philosophical". What I have tried to show, is that they are closely connected with various "sociological" features of academic research.

CUDOS institutionalised

There is much more to the practice of academic science than individual activity guided by a general ethos. Even the loneliest

"seeker after truth" must eventually interact with other people, if only as informed critics or supporters. Academic science could not function without some sort of internal social structure.

This structure is provided by *subject specialisation*. Academic science is divided into *disciplines*, each of which is a recognised domain of organised teaching and research. It is practically impossible to be an academic scientist without locating oneself initially in an established discipline. The fact that disciplines are usually very loosely organised does not make them ineffective. An academic discipline is much more than a conglomerate of university departments, learned societies, and scientific journals. It is an "invisible college", whose members share a particular research tradition. This is where academic scientists acquire the various theoretical paradigms, codes of practice and technical methods that are considered "good science" in their particular disciplines.

Specialisation does not stop there. The sub-division of disciplines into very narrow research specialties seems to be an unavoidable feature of academic science (Ziman, 1987). But this reveals contradictions in the academic ethos. Most academic scientists can only satisfy the norms of originality and scepticism by concentrating for years on what is known, what is conjectured, and what might be feasible, in a limited "problem area". Unfortunately,this is often inverted into a pedantically sterile or fashionably conformist ideal of "truth". Excessive specialisation also encourages sectarianism and cognitive fragmentation, thus offending against the norms of communalism and universalism.

A recognised discipline or sub-discipline provides an academic scientist with a home base, a tribal identity, a social stage on which to perform as a researcher. The academic ethos says nothing about individual motivation. Note, however, that the Mertonian norms combine into the acronym "CUDOS" – that is, "acclaim", or "prestige". The argument is that academic scientists undertake research, and make public their findings, in exchange for "recognition" by their peers. The citations in the literature, prizes and medals, exalted titles and other tokens of communal esteem are not just frippery: they are important functional elements of the academic culture.

One question, however, remains unanswered: how do academic scientists make a living? the academic ethos seems to take it for granted that research is a personal *vocation*, rather than gainful *employment*. Academic scientists are often deeply committed to their work, but they are seldom "amateurs" in that sense. The peculiar feature of academic science is that it developed as an activity engaged in principally by "academics", whose official employment is to teach, rather than to do research. Everybody knows, of course, that university teachers usually owe their posts to their proven research competence, and earn further promotion by their research achievements. Nevertheless, the convention is that this research is "their own work", which they are free to undertake and benefit from entirely as individuals.

The existence of academic science as a distinctive cultural form thus depends on the willingness of universities and other institutions to provide personal time and other resources for an activity from which they do not directly profit and which they do not directly control. This applies particularly to bodies that support full-time researchers on essentially the same terms, regardless of whether they perform other services, or even whether their contributions to knowledge are of any great significance. This is not the occasion for a discussion of the benefits that academic science brings to society, nor for a demonstration that these benefits far outweigh their out-of-pocket costs. The key point is that academic science relies on public and private *patronage*, in the broadest sense of that old-fashioned word. Its whole ethos is based upon the belief that the pursuit of knowledge is of value in itself, and that only very knowledgeable people can be trusted to pursue it effectively.

New Modes of Knowledge Production

Much more could be said about academic science as an "implementation structure" for the pursuit of a common purpose by a community of individuals activated by a shared culture. This very schematic account does show, however, the linkages between the main features of the culture and some important characteristics of scientific knowledge. Many questions about these linkages remain to be answered. We know for, example, that scientists seldom abide perfectly by their norms: does that lead to serious imperfections in the output of their research? In any case, shouldn't academic science be considered to be just a sub-culture of society at large? What are its relationships with other knowledge-based sub-cultures, such as industrial research or technological practice?

Unfortunately, academic science is changing so rapidly that the real question is: what is taking its place? Some of these changes simply reflect scientific and technological progress. As always, the dedication of science to originality is drawing it into quite novel modes of activity. Individual achievement is being merged into the collective action of multidisciplinary teams. Communication is being speeded up electronically, until it becomes instantly global. Instrumental sophistication is making it much easier, but much more expensive, to do good science. Although these may look like natural technical developments, they involve radical changes in many traditional practices and attitudes.

In a recent work (Ziman, 1994), I concentrated on the forces pressing on academic science from society at large. In effect, the whole enterprise, having grown steadily for centuries, has now become too large and expensive to be allowed to go its own way. The governments that mainly fund academic research are putting strict financial ceilings on their patronage, and are trying to get better value for their money. They are insisting that researchers should become much more accountable, more responsive to societal needs, more directly concerned about the quality and impact of their products, and so on. The transition to a "steady state" regime is thus imposing on academic science a number of requirements that are quite foreign to its ethos, and thereby transforming it as a cultural form.

This metamorphosis is still going on. I have been reluctant, personally, to speculate on what will eventually emerge from the present jumble. But now six very distinguished metascientists – let me call them the GLNSST group, for short – have boldly presented a credible scenario for the future of science (Gibbons *et. al.*, 1994). In sum, they argue that the academic mode of knowledge production is being replaced by a very different activity, which they call "Mode 2".

To some extent the GLNSST argument merely extrapolates current trends within academic science, such as the growth of multidisciplinary teamwork and inter-institutional networking. But it also suggests a decisive break with the academic tradition in relation to conditions of employment, problem choice, criteria of success and other important features. In other words, "Mode 2" is not just a "new mode of knowledge production": it is a formula for a possible new research culture.

The GLNSST group note that Mode 2 has evolved outside academia, and will not necessarily supersede "Mode 1" in its traditional setting. But this is a very real possibility, which ought to be taken seriously. At least it provides a coherent model for "postacademic science". Following a methodological principle that I learnt as a theoretical physicist, I shall explore this model as a cultural form, and ask what sort of knowledge it might be expected to produce.

Networking Intellectual Property

Although GLNSST present Mode 2 as a coherent activity in its own right, they describe it mostly in terms of its differences

from Mode 1. For this reason, Merton's list of norms again provides a convenient analytical frame. We start, then, with the norm of communalism. This looks easy. Just as "community" was the keynote of academic science, so "network" has become the token of postacademic science. In both cases, science is visualised as a *communication system*, where information obtained at certain nodes is transmitted to other nodes, whether these be individual researchers, research groups, specialist communities, corporate bodies, or the general public.

Nevertheless, certain technical and procedural developments have produced significant cultural and cognitive effects. The increasing density, multiple-connectivity and immediacy of electronic communication draws individual researchers together into collective action. This is not just a matter of facilitating active teamwork by geographically dispersed researchers. It makes it feasible for novel observations and theories to be discussed in detail with distant colleagues – or even sceptical rivals – as they emerge. Databases and archives can be searched thoroughly for relevant ideas and information. An electronic text can be amended so easily that it need not come to a firm conclusion until this has become acceptable beyond refutation. The various phases of the research cycle – discovery, justification, criticism and revision – merge together in an off-the-record process involving a whole cluster of informal contributions. The material that does eventually get into the official scientific literature may thus already represent a wider consensus than the group of authors to whom it is officially attributed, and should therefore be less tentative, more convincing, sounder in fact and logic, than is normal in academic science.

On the other hand, Mode 2 networks are typically very heterogeneous. Academic scientists are regularly teamed up with researchers who are not bound by the norm of communalism, and are not professionally dependent on their contributions to "public knowledge". Mode 2 knowledge is *proprietary*. Research results which an academic scientist would have published as soon as possible are now defined as "intellectual property", and may be kept secret for longer or shorter times for commercial reasons. This has the effect of weakening the traditional mechanisms motivating prompt publication. It also means that the knowledge appearing in public out of postacademic science may lack significant items which are only known to a privileged group, such as the employees of a particular industrial firm.

Problem solving in local contexts

From a sociological point of view, Mode 2 fully satisfies the norm of universalism. The networks of communication and collaboration are global. It is not necessary for a researcher to move bodily to an established research centre to do good science. Researchers located in industrial firms, government laboratories, charitable foundations and universities may work together in the same team. Even the tribal boundaries between disciplines are disregarded. As GLNSST point out, this universalism is not a remedy for gross inequalities of resources, facilities or competence between countries or institutions: but that is a much larger issue than can be considered here.

On the other hand, postacademic science may not favour *metaphysical* universalism. Mode 2 evolved outside academia, as a technique for applying science to practical matters. It is organised intellectually around the solution of *problems*, rather than directed towards the production of knowledge as such. It follows that the knowledge that is actually produced is intrinsically *local*, rather than *universal*. Even though it may have wide theoretical implications, it is not shaped by a preference for unification and generality. What counts as "good science" in Mode 2 may be technical skill at problem solving, rather than advancement of our understanding of the natural world.

This does not mean that postacademic science must necessarily be "useful". It should not be thought of as inverting the academic tradition of "purity" and "uselessness". Utility will simply cease to be a demarcation criterion. One of the main features of Mode 2 is that it draws on, and generates, problems, techniques and research results from all parts of the conventional "R&D spectrum". Basic research and technological development already interpenetrate one another: in the long run, they will become inseparable.

Instead of unification postacademic science favours *finalization* (Böhme *et. al.*, 1983). Like most general terms used to describe the research process, this is an ill-defined concept, with ambivalent policy resonances. But it indicates the way that research in a particular area may become more "mature", until it is guided by an unchallenged theoretical *paradigm*. Research programmes are then be formulated within a stable framework of concepts and techniques, and directed towards specific ends. "The art of the soluble", as practised intuitively by individual researchers, gives way to an explicit rationale for the way in which problems are posed and attacked.

Finalized research is not free of uncertainty, and its objectives are not necessarily utilitarian. The orientation of Mode 2 towards specific problems actually permits a looser, more experimental approach, where the problem itself provides the focus of continued effort. This approach works well in the study of natural and artificial phenomena in problem areas whose contexts are partly universal and partly local. But it is also perfectly capable of taking on the well-posed basic problems that arise naturally in the regions where traditional disciplines interact or overlap.

Finalization favours interdisciplinary research, since it obviously benefits from the *reduction* of observed phenomena to more fundamental principles. But postacademic science will probably not be driven by reductionism as a metaphysical ideal. A striving for "local" understanding, without preconceived notions of what might require explaining or be acceptable as an explanation, may even be more effective in closing the gaps in the knowledge map than a single-minded pursuit of general intellectual unity.

Incorporating Interests into Knowledge

The norm of "disinterestedness" was always difficult to sustain. Even university teachers engaged in "pure" research have strong professional interests, and are not completely shielded from economic and political pressures. In Mode 2, knowledge is produced by teams of researchers networked across a wide range of organisations. The diverse working conditions of these researchers cannot be disregarded. Those who are on short term contracts of employment are not in a strong position to show independence of mind. Those who are employees of industrial firms must always be aware of the potential commercial value of what they produce. In the latter case, the suspicion of bias cannot be entirely discounted just because the research seems disconnected from any possible application. An industrial firm is not a charity. If it does not think it will benefit in some way from the work, then how can it justify paying its employees to do it?

Postacademic science will surely be too deeply entangled in networks of practice to be considered free from such influences. For most sociologists and philosophers of science, this is not a new thought. The notion of a truly objective, disinterested "seeker after truth" is not consistent with the realities of social existence. We all have interests and values which we are bound to promote in our scientific work, however hard we try to suppress them.

But the actual effect of these factors can be exaggerated. The essence of the academic ethos is that it defines a culture designed to keep them as far as possible under control. Academic science does often

manage to live almost up to its ideals. Mode 2, by contrast, does not just "produce" knowledge: it is a culture where knowledge is *constructed* in accord with the commercial, political or other social interests of the bodies that underwrite its production. Although these interests may also incorporate scientific values, this is a culture where socio-economic power is the final authority. If that is indeed the way that postacademic science is going, then the sociological relativists who insist that scientific knowledge always serves such power will be proven right after all.

Who sets the problems?

Mode 2 research is performed in a "problem context". This is nothing new in principle. Philosophers and scientists agree that the identification of a significant but soluble problem is a vital stage in the research process. The question is: who *sets* the problems. Academic science works on the assumption that researchers are free – within reasonable limits – to set their own problems. In Mode 2, by contrast, researchers work together on problems which they have not posed personally, and which they may not even have chosen collectively as a team.

According to GLNSST, Mode 2 problems are set in "hybrid fora" where the researchers involved may have more or less weight, depending principally on how close the problems are to practical application. But even basic research does not take place in a power vacuum. It has to be supported financially and administratively by bodies whose interests go beyond the mere production of knowledge. They naturally exercise these interests at the point of maximum leverage – that is when research problems are being set. All policy talk about foresight, priorities, accountability, etc. is really focused on "problem choice".

Postacademic science will surely not be given over completely to "commissioned" research. It will be on the lookout for technical virtuosity by individuals and research teams. But Mode 2 tends to define the highest form of scientific "creativity" – the construction of soluble research problems – as a group phenomenon. This is why academic scientists set such great store by "responsive mode" funding, which provides them with almost their only opportunities to demonstrate this attribute as individuals. Even then, success in formulating fundable research proposals may simply reflect a shrewd eye for what is currently regarded as "good science".

Academic science has always worked on "Darwinian" principles. Scientists undertake research and offer results on innumerable different problems. Knowledge advances in unforeseen directions by the retention of the small proportion of these results that survive rigorous testing. It is a very wasteful process in detail, and seldom produces outcomes that perfectly fit our current needs. It just happens to be extraordinarily effective at generating remarkably reliable knowledge.

Postacademic science will continually strive to improve on this process. It will try to push the process in desired directions by strongly favouring research on particular problems. It may thus be very effective in overrunning disciplinary frontiers, in order to construct detailed local maps of potentially useful areas.

Postacademic science will also try to eliminate waste by ensuring that all research projects are well-designed, and directed towards well-posed problems. But the efficiency of a Darwinian process depends on having a highly diversified stock of variants, as well as a highly selective environment. The effect of "collectivizing" problem choice must be to limit the range of variation of research projects. Mode 2 problems are socially pre-selected, on hypothetical grounds, before they are actually tackled. This may mean that the science that gets done is "better" and more "relevant" than if it were left entirely to the idiosyncratic judgements of individual scientists. But it also means that a few wild conjectures never get a chance to show their hidden capabilities, which are just occasionally revolutionary.

What counts as excellence?

Mode 2 replaces peer review of research outcomes by *quality control* of people, projects and performance. But this usually embodies a much broader notion of "excellence" than the traditional academic criteria for "good science". Proven competence as a researcher may count for less than a good record as an *expert* at solving societal, environmental or commercial problems. This expertise need not be very specialised. It may be just an ability to enter a temporary research team smoothly, and make a useful contribution. The research quality of a team may be confused with its success in getting funding. More and more importance may be attached to entrepreneurial and managerial skills as the research process becomes part of a larger cycle of action – for example, in the successive stages of bringing a techno-scientific innovation to market.

Paradoxically, postacademic science could become so obsessed with accountability, performance monitoring, contractual scrutiny, and other forms of "quality control" that it sacrifices the quality of the procedures themselves to their sheer quantity. Mode 2 research does not promote the establishment of groups of practitioners in stable positions of intellectual authority. In the absence of human reference groups, assessment procedures may be automated. Quality control is then made to rely on surrogate indicators of performance, whose legitimacy may well be questioned on scientific grounds.

In other words, mode 2 downplays the role of systematic intellectual *criticism*, which is the key to the validity of academic science. In contexts of application, practical utility must eventually be effective as a selection mechanism, even if only in pragmatic terms. But in fundamental research, where this mechanism cannot operate, what Merton called *organised scepticism* is the only real protection against the embodiment of serious errors in the knowledge that is produced. Perhaps a higher level of cognitive insecurity is a price that will have to be paid as postacademic science becomes more entangled with "trans-epistemic" issues, involving societal, environmental and humanistic values.

From specialised knowledge to expertise

The world of practice does not carve itself up neatly along the joints between the academic disciplines. In the context of application, all problems require a *multidisciplinary* approach. This is equallt true for research into fundamentals, where the most interesting areas for the exercise of intellectual curiosity are *interdisciplinary*. The most radical feature of Mode 2 is that it strives to take a broader view than can be achieved from within any one discipline.

In Mode 2, specialists from different disciplines work together as a team. Ideally, their different approaches to the problem fuse together into a coherent attack with a comprehensive solution. Between them, they construct a *transdisciplinary* schema of paradigms, techniques and expertise that provides a framework for further advances. In traditional academia, one would say that a new specialty was emerging, and expect to see it institutionalised as a regular discipline. But Mode 2 is not geared for such a development. There is no organisational or intellectual structure on to which a research team can crystallise and exploit its transdisciplinary capabilities. After a certain time, the participants must break away and form new configurations, around new problems, requiring a different mix of skills.

The GLNSST vision of free-floating, mutable, transdisciplinary schemas lacks a social context. In reality, practical problems seldom appear out of nowhere, without antecedents. The world where research is to be applied is already highly structured. That is, the problems to be tackled will normally be set and funded by their organisational "owners", such as industrial firms, government departments, health services, etc. In the effort to overcome the

academic vice of narrow specialisation, postacademic science may find that it has put itself into the hands of bodies that are even more parochial, fragmented and restrictive than the disciplines from which it has escaped.

Where do the pipers collect their pay cheques?

Mode 2 researchers work in shifting teams, like small firms producing goods for a competitive market. This system attaches the individualism of academic science to small groups, and motivates them with entrepreneurial insecurity. This insecurity is endemic. Even if such a group is not an independent entity, always risking its future in the competition for funding, it cannot provide stable employment for most of its members. As teams reconfigure to tackle new problems, some researchers will have to move elsewhere to make room for new people with new skills. Academic scientists are often demoralised by undue persistence in tenured, specialised research. Mobile researchers hired as professional problem-solvers may be equally demoralised by lack of stable opportunities to establish or exercise their specialised expertise.

The Mode 2 model of an open system of economically independent entrepreneurial groups also assumes the existence of a *market* for research services and results. Markets of this kind do exist in certain high technology industries, although close study shows that small research enterprises survive there mainly through their connections with very large commercial firms. Government funding of more basic research is often organised around competitive project grants, customer-contractor arrangements and other market concepts. But the entities that actually compete in the provision of these services are seldom free-standing. The researchers who network across the world are mostly full-time employees of universities, government laboratories, charitable foundations, or industrial firms. They may have to fight to keep their jobs, but they do not have to take financial responsibility for the elaborate facilities that they actually use in their research.

Mode 2 research looks attractively unbureaucratic, but it is really heavily capital-intensive. It is funded by a complex of governmental bodies, large public institutions and private corporations. It could not exist without this economic base. Postacademic science will surely not be able to duck the central questions of science policy: who will pay the pipers, and what tunes should they be called on to play?

Postindustrial science

The new mode of knowledge production described by GLNSST is a very different culture from "Mode 1". In fact, it evolved in a different social niche. The systematic use of scientific research to solve practical problems is at least as old as academic science. Medicine, engineering, agriculture, mining, and particularly manufacturing industry, have all nurtured *applied science*, and benefited immensely from it. This is where Mode 2 came from, and where it still largely fits.

Applied science has expanded so rapidly and diffused so widely that it now greatly exceeds academic science in scope and scale. But it never developed a homogeneous culture. It is distributed in pockets throughout society, and in each pocket it is shaped to fit local practices. Clinical medicine, for example, is a very different profession from engineering, and organises its research activities very differently.

Nevertheless, the industrial applications of science have been so dramatic that applied science is often identified with the research and development activities of large commercial firms. By the 1960s, *industrial science* had emerged as a characteristic way of life.

Technically and cognitively, industrial

science was the twin of academic science, and yet it was organised on quite different principles. Indeed, these principles are almost the inverse of the Mertonian norms (Ziman, 1995). The standard form of industrial science was *Proprietary, Local, Authoritarian, Commissioned,* and *Expert.* I have not time here to go through these characteristics one by one, but it is no accident that they spell out *PLACE.* The stereotypical "industrial R&D laboratory" offered a lifelong organisational career in a hierarchy of technical specialties. In effect, it was a managerial microcosm of the industrial firm in which it was embedded.

Since that time, however, industry itself has changed. We are said to be entering a "postindustrial" era, characterised by multinational firms, decentralised managerially into small, specialised service units, devolving much work to sub-contractors, coordinated globally by information technology, etc.. And as industrial firms changed their working methods, they restructured their research activities along similar lines. Their R&D laboratories were devolved into multidisciplinary matrices and global networks of temporary project teams, buying in specialist functions from independent contractors, and so on.

In a word, Mode 2 is essentially the "postindustrial" version of applied science. *Postindustrial science* differs from earlier forms by substituting "market" competition for "command" management, but is actually based upon the same principles. As we have seen, it is "proprietary", "local", "authoritarian", "commissioned" and "expert", even if it does not offer such a safe "PLACE" as it used to.

The differences between Mode 1 and Mode 2 are not just signs of a recent change from an old to a new method of knowledge production. They have their roots in the historical distinction between pure and applied research – a distinction that was embodied institutionally in the gap between academic science carried out in universities and industrial science carried out in industrial laboratories. This gap corresponded to a real cultural difference between two social systems. But these systems were always closely connected and dependent on one another. They could not live easily together under the same roof, but the existence of each was always understood to be essential to the continued vitality of the other.

The evolution of industrial science into postindustrial science is altering this relationship. We have already identified a number of factors working towards a single "postacademic" culture. Cognitive developments, such as the "finalization" of many sub-disciplines, are blurring the distinctions between basic and applied research. Technological developments, such as real-time electronic networking, are generating heterogeneous, hybrid teams that over-ride institutional loyalties. Economic conditions, such as the transition to "steady state" funding, are forcing the two cultures into the same organisational mould.

Indeed, a self-conscious effort at a high level of political and managerial authority would now be required to keep the two systems from coalescing in style and function. But such a merger not only raises many practical issues of funding, disciplinary identity, criteria of excellence, career aspirations, intellectual property rights, institutional management and so on. It also brings face to face two very different sets of structural principles.

We are thus faced by the classic sociological situation discussed nearly a century ago by Max Weber. In any such confrontation, the organisational prescripts of PLACE will almost certainly prevail over the communal norms of CUDOS. In other words, as GLNSST surmise, Mode 2 will largely, if not entirely, supersede Mode 1 throughout the world of science. The culture of postacademic science will be predominantly postindustrial.

Postmodern knowledge

The transition from academic to postacademic science will surely leave the

operating philosophy of research unchanged. Scientists will still construct knowledge on the basis of a firm belief in the existence of an external world whose behaviour is intelligibly regular and not disjoint. They will go on theorising, and testing their theories by observation and experiment. They will continue – quite rightly – to resist firmly the philosophical scepticism, sociological relativism, political cynicism, ethical nihilism, and historical incommensurabilism projected on to science by some of its wilder critics. It is not academic science, but academic *metascience* that is in a state of intellectual anarchy, where "anything goes".

Nevertheless, in its postacademic form, science will have shed some of the doctrines of "modernism". In particular, it will not claim to be able to produce a universally applicable answer to every problem. In wider cultural and humanistic circles, "modernism" has now given way to "postmodernism" (Toulmin, 1992). Postacademic science will not only be postindustrial in its social role, and "hyper-modern" in its conspicuous engagement with information technology: it will also be postmodern in its philosophy.

Of course, terms such as "modernism" and "postmodernism" are very ill-defined. To most scientists they sound like cult slogans, deployed for, or against, the most diverse fashions and fads. I believe they have a serious core of meaning, but would not pretend to be able to define it. I am using them simply to indicate that there have been changes in the philosophy of science, and that these are not unrelated to changes in our general philosophical outlook.

The new wave that has swept through the non-scientific intelligentsia in recent years does not seem to be a primary cause, or a primary effect, of what has been happening in science. But there are undoubtedly numerous secondary interactions which may not be insignificant. Some of the likely features of postacademic science do resonate with certain elements of the postmodern critique. In pointing out these resonances, I am simply following a fruitful precept of theoretical physics. As the novelist E.M. Forster put it: "Only connect!" – or was it "Where angels fear to tread..."?

Epistemic features of Mode 2

Let us look again at some of the typical features of Mode 2. In the first place, Mode 2 is not activated by the vision of a unified, universal scientific world picture and does not try to "reduce" every body of knowledge to one that is more "fundamental". This is in line with the postmodern critique of grand theory. Postmodern philosophers renounce the age-old attempt to put human understanding on absolutely firm "foundations". They argue cogently that reductionist explanations of natural phenomena can never be complete or perfect, and that "metanarratives" can never be grounded in absolute, accurate, ultimate truths. Postacademic science will no longer promote the intellectual imperialism of scientific monism.

Mode 2 focuses, rather, on the regions around specific problems. The dense communication networks and transdisciplinary openness of postacademic science will encourage thorough exploration of limited areas, and of the pathways between them. This does not preclude the study of fundamental questions. But instead of advancing head on against conceptual enigmas, research will start with concrete problems and open out in the direction of greater generality.

But as postmodern philosophers point out, the construction of a reliable representation of a local reality usually involves the development of a correspondingly local language. The more elaborate this representation, the more difficult it is to uncouple such a language from its context, and to use it in representing the realities of other problem areas.

As a consequence, postacademic maps of knowledge may well be meticulously detailed and systematic in their coverage, but they will still be divided up into specialised

domains, characterised by mutually incomprehensible technical languages. Indeed, these domains will always be in a state of flux, and often overlap one another. Novel solutions will be found for long-standing problems. Novel problems will arise in new contexts of application. Paradigms, techniques and specialised skills will be continually segmented and recombined into new configurations.

This does not mean that postacademic science will reject operational realism. On the contrary, research will be rooted in life-world problems. The postmodern critique applies to attempts to *represent* objects existing outside the mind. Such a representation can be perfectly real as a guide to thoughts and actions, but that does not require it to be complete, timeless or unique. It can always be analysed more and more precisely, and traced further and further back into wider networks of representation. Postacademic science will be enlarged and enriched by this process of *deconstruction* – typically towards greater generality and abstraction – without necessarily losing contact with the reality.

Postmodernism thus denies that the representation of any aspect of nature must converge towards a unique map. It repudiates the metaphor of the university as a notional "brain", whose permanent modules are academic faculties and departments, each dealing tidily with its allotted discipline. Highly formalised schemes of thought are to be regarded less as strengthening skeletons than as potential barriers to understanding. Here again, postacademic science will not reject such schemes out of hand, but will take a sceptical attitude towards their claims.

Mode 2 is not merely multidisciplinary: it is almost dogmatically *pluralistic*. It welcomes diversity, and is not fearful of possible inconsistencies. The knowledge that it produces is not organised around theoretical issues, and is not automatically subject to clear rules of coherence and credibility. It may combine cognitive and non-cognitive elements in novel and creative ways – witness cognitive science itself – but it can also be a diffuse – even opaque – mixture of theory and practice, ideas and data, designed to meet the needs of a specific application. In other words, in Mode 2 *pragmatism* rules.

Postmodernism similarly celebrates extreme pluralism. It favours wide definitions of knowledge and decentred diversity. It denies the possibility of formulating general rules by which to judge the validity of new ideas, or stable categories into which to place new data. This philosophical doubt is, of course, mostly – well, "philosophical". Postacademic science need not cast itself adrift from its twin anchors of rationality and empiricism. In its traditional sphere of basic research it will surely maintain the critical apparatus of academic science. But it may become incoherently (and unreliably) postmodern in other spheres, where it forms hybrids with research cultures which do not share the same intellectual values, or the same standards of "good science".

Finally – but perhaps most importantly – Mode 2 is permeated with social interests. In postmodern terms, it is consciously *reflexive*. The interaction between the knower and what is to be known is seen to be an essential element of the knowledge. Some allowance can be made for this effect, but it cannot be eliminated.

The parallel with some accounts of quantum theory is obvious, but not relevant. The real point is that postacademic science will always have at least half an eye on the contexts of application from which it gets its problems. It will therefore be dealing with matters where societal values – safety, profitability, efficacy, etc. – cannot be ignored. These values conflict with one another. Individual citizens and independent social groups rightly weight them differently. However hard people try to rise above their personal attitudes, these become significant factors in the cycle of research and action.

Doctrinaire postmodernism deconstructs scientific objectivity out of existence. Postacademic science will surely defend objectivity as an ideal, impossible to realise completely in practice but always to be

respected and desired. But if all research arises in contexts of application, there may never be any occasions where this ideal is paramount. Scientific objectivity is not an abstract philosophical virtue. It is a cultural norm embodied in a web of social practices. Academic scientists internalise the norm of "disinterestedness" through experience in research situations where these practices are systematically observed. It is hard to see how this norm will be sustained when there are few situations yielding the relevant experience.

What price objectivity?

This analysis does not suggest that science is "going postmodern" in the fullest sense. Most of the postmodern features of postacademic science are quite mild, and even benign. Some are much-needed corrections to the excesses of "scientism". Others are welcome antidotes to the decontextualised rationalism that has long plagued the philosophy of science. Other features, again, help rescue the scientific imagination from entrenched specialisation. And localised pragmatism will largely compensate for the fragmentation of theoretical standards of scientific validity.

The decline of objectivity, however, is a much more serious matter. It illustrates perfectly the connection between a cultural norm and a philosophical concept. In the ethos of academic science, the sociological norm of disinterestedness is linked to the regulative principle of objectivity, each reinforcing the other. But postindustrial research has no place for disinterested practices, and postmodern thought has no place for objective ideals. Postacademic scientists will have neither examples of disinterested behaviour to emulate, nor formal standards of objectivity to live up to. Constructive reinforcement will give way to deconstructive decay.

Objectivity is one of the features that make science so valuable in society. It is the public guarantee of reliable, disinterested knowledge. Science plays a unique role in settling factual disputes. This is not because it is particularly rational or because it necessarily embodies the truth: it is because it has a well-deserved reputation for impartiality on material issues. The complex fabric of democratic society is held together by trust in this objectivity, exercised openly by scientific experts. Without science as an independent arbiter, many social conflicts could only be resolved by reference to political authority or by a direct appeal to force. That was the historical experience out of which scientific institutions such as the Royal Society emerged, and its wisdom remains our cornerstone.

The postmodern critics of science insist that its claims to objectivity are false, and actually conceal powerful established interests. It is noteworthy, however, that anti-establishment groups also appeal to objective scientific knowledge to advance their causes – for example in environmental disputes. Even metascientific anarchists should realise that by "unmasking" the "ideology" of objectivity they are breaking their own swords in the struggle against their most feared opponents – the corporate and governmental enterprises that drive postindustrial science.

Is there any way of avoiding this loss? The trouble is that scientific objectivity is an emergent *cultural* property of academic science. It was not an attribute of any previous knowledge-producing culture, and we have no theoretical models for other cultures with similar attributes. The best that we can do is to determine the functions that it serves in academic science, and the features that sustain it there. We can then ask whether these features could be maintained in postacademic science, or at least in some segment of it.

We thus return to the question whether postacademic science is bound to be completely "postindustrial". The GLNSST group define Mode 2 in that way, and surmise that it will supersede Mode 1, even within the university. This surmise is much more significant than it appears at first sight.

In particular, the suggestion that all research will eventually relate to problems arising in the context of application has profound consequences. Combined with the demand for accountability in the formulation of problems, it opens up the whole of academic research to the influence of external interests.

We only have experience of one way of countering this trend. But that would take us back to the central tradition of academic science. This was not an ethos or a bundle of communal practices. It was, quite simply, *patronage*. It was the convention by which society provided resources for the production of knowledge without insisting that they should be accounted for, in prospect or retrospect, in utilitarian terms. This convention may now seem elitist, irresponsible and inefficient, but it worked remarkably well in its time. We abandon it at our peril.[1]

NOTES

[1] This paper is the substance of the 1995 Medawar Lecture of the Royal Society, delivered in London on 29 June, and in Edinburgh on 27 September 1995. I am grateful for helpful comments from Phoebe Isard and Helga Nowotny.

John Ziman
27 Little London Green
Oakley, Aylesbury, Bucks HP18 9QL
England

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