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HOW GROUPS CONSTRUCT THEIR SCIENCE

Scientists and students

This paper will look at two contrasting groups, school students and professional scientists, in an attempt to identify some of the social processes by which they establish their scientific knowledge. The two groups will be likely to have many resources in common, such as a natural language mode of communication and a core set of elementary social norms of courtesy, conformity, etc. In many other ways, however, the two groups will be bound to act differently. The students have, for example, the towering figure of the science teacher - towering, that is, in terms of the students' construction of science - and a personal reward structure dependent on examination marks or grade points. The professionals have concerns about salary, promotion and personal reputation, which add a different dimension to their struggle to do 'good science'. Nevertheless there are common elements in the actions required of these apparently different social groups in coming to an understanding of a body of scientific knowledge. By concentrating on these common elements we hope to bring out some of the essential features of the social construction of science.

Philosophical Constructivism

First some elementary points need to be emphasized about the constructed nature of all scientific knowledge. Although constructivism is widely supposed to be a modern concept, it arises inevitably out of Hume's eighteenth century analysis of the 'induction problem'. Once it was clear that the gathering of evidence, by repeated observation or by controlled experiment, could never establish general theory as an unassailable 'fact' which was true for all time, the legs were knocked away from under the old-

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er view of secure and objectively true scientific knowledge. If the verdict of evidence is always equivocal, then the notion of an absolute standard of objectivity is open to devastating criticism, and all knowledge claims - not just those that are avowedly theoretical but also those that are supposedly empirical - have to be seen as human artefacts. Science knowledge is constructed by humans: indeed who could doubt it?

Anyone concerned about belief in science is thus put into a quandary. If you and I, and she and he, all interpret evidence in line with our own favourite images, concepts, analogies and ways of explaining, could it be that the whole scientific enterprise is fragmented beyond repair? How can we go on believing in the unity and uniqueness of a body of knowledge for which there seems no firm basis? These are, of course, traditional philosophical doubts, associated with the logical irrefutability of methodological scepticism, solipsism or relativism. Only now, however, has scientific epistemology begun to face up to the challenge of such doubts, with results that are particularly disconcerting for professional scientists and others who are deeply committed to the unquestionable 'reality' of the world they earnestly investigate and conscientiously describe.

This quandary has been agitating philosophers, delighting sociologists, and horrifying thoughtful scientists for some 25 years (see, e.g., Callebaut 1993). School science has rarely confronted the issues raised by academic metascientists in their debates about 'constructivism' and 'relativism'. This may be due simply to the professional commitment of science teachers to the high status of the subject they teach, and their deference to the unredeemingly positivistic views of their own university teachers.

Educational Constructivism

At the same time, the term 'constructivism' has acquired a very different meaning in relation to school science. It has become standard practice, in the last fifteen years or so, to assign it to the acknowledged and rather unremarkable fact that, before they come to school, children already have some ideas about how, and even why, natural phenomena happen as they do. Observations to this effect have rarely been interpreted in the light of the work of metascientists.

This usage is not only confusing: it is also unfortunate in two ways. In the first place, because it arose partly out of the work of the psychologist George Kelly (1955) it became associated with his particular view that the individual's interpretation of phenomena was essentially similar to the processes of science. His slogan, "every man his own scientist", was presented without any analysis of what might be the purposes and practice of science itself. Whether this was supposed to raise the status of everyday reflection, or to degrade the edifice of scientific knowledge is uncertain.

What Kelly did do, quite unequivocally, was to point out the deeply personal dimension of interpretation. "No man", he wrote," can construe another's meaning". This was the second unfortunate aspect of his approach. In the strictest sense it meant that neither science nor science teaching could have any future except as an internal exercise of the isolated intellect. But it is now abundantly clear (Ziman 1968) that science is a social activity, of which communication is of the very essence. Kelly's constructivism is strictly solipsistic, and would even preclude the *social* relativism that is now widely canvassed amongst epistemologists (Solomon 1994).

Knowledge construction in the classroom

Every act of communication, and especially that of teaching, aims to transfer concepts and their meanings from one individual to another. Of course that does not mean that knowledge can really be handed on, like the party game of 'pass the parcel', in its entirety. No sort of human communication is like that, and not just because parcels tend to shed pieces of packaging as they are passed along. All messages have to be interpreted - we might say 'reconstructed' - by those who receive them.

That might suggest that scientific knowledge is continuously being eroded and transmuted as it is being taught. Quite apart from the affective and intentional mismatch between differing personal constructs, classroom discourse is a 'noisy' medium for reliable communication. Errors and uncertainties of transmission and reception might eventually drown out all the original sense of the scientific knowledge.

There are good psychological and sociological reasons why that potentially disastrous scenario does not normally take place. It is because people strive so hard to understand each other's meaning. After almost every sentence they inquire "Do you see what I mean?". You can hear them in casual conversation making `collaborative sentences' (Barnes and Todd 1976, Solomon 1990) where they follow so closely on the heels of the others' meaning that they actually finish off each other's sentences. This is not just a foible or style of talk; it is an outward sign of the effort all of us put into that *matching* of *meaning* (Mead 1934) which is the essence of all social contact. We need it to preserve our very sanity.

Even the untutored notions which children bring to the classroom show clear signs of this social matching of meaning, or *intersubjectivity*. Almost every one of the researchers who have studied these informal ideas has been struck by the similarities between the children's notions, at least within the same culture or language group. That is one sign that they have been suggested and exchanged through the medium of shared language, as the work of Vygotsky suggested. Another indication of this social origin is their diversity. As philosophers, sociologists and linguists have come to realise (e.g. Wittgenstein 1958, Bourdieu 1977, Halliday 1978), this is an irreducible characteristic of all natural language. In brief, a medium of communication cannot possibly be precise and single-valued if it is to give expression to the infinite multiplicity of concepts, and diverse ways of thinking about phenomena, encountered in different life-world contexts.

Social exchanges, however, do not just confirm and match ideas: they also seek to persuade. This is a process which is carried out more by rhetoric than by logic (Billig 1989). Typically, abstract arguments and counter-arguments are backed up by concrete examples, which can be presented either as very likely and convincing, or manifestly wrong and disconfirming. Both positive and negative exemplifications are thus primarily designed to change other's opinions. These are important processes because all teaching is persuasion at heart (Solomon 1992). Teachers show pupils the theories, skills and concepts accepted by scientists, and try to make them acceptable to the class.

Science teachers thus seek to socialise children into what they perceive to be genuine scientific knowledge. But teachers are usually working a long way behind the research frontier. As will be argued later, they rarely use the scientists' own methodological norms to engage in this persuasion. This would be quite inappropriate, because the classroom group is not the professional group. So teachers use what Larochelle and Desautels (1991) nicely call 'the mechanisms through which common sense makes scientific knowledge digestible'.

The paradox of this endeavour to persuade, couched by the skilful teacher in the familiar 'common sense' mode, is that children may accept it in the same spirit. Thus begins the process of transforming scientific knowledge into a social representation which the public can 'consume' (see Moscovici and Hewston 1983). If they see scientific knowledge as yet more of what Schütz and Luckmann (1973) referred to as 'the social stock of knowledge' it will be taken on board in the same spirit, that is, as an incoherent collection of items of largely unconnected information. Unstructured knowledge of this kind may be useful for answering questions of the 'trivial pursuits' kind, but lacks the decontextualised, interconnected, reliably applicable characteristics by which scientific knowledge is generally considered to differ from life-world knowledge.

The fact that this characterisation of science is now disputed by many metascientists is not to the point. There can be little doubt that scientists locate their scientific knowledge in a distinct 'domain', associated with a distinctive way of thinking. What Wittgenstein called a 'form of life' or 'language game' can only be mastered by a psychological process of socialisation. This may not supplant life-world thinking, which is paramount in daily conversation and which was acquired through an earlier socialisation in the more friendly atmosphere of the home and the school playground. The scientific way of thinking, when or if it is achieved, will normally co-exist alongside the primary life-world way (Solomon 1992). The most exacting task for the learner of science will then be to know when each kind of knowledge should be used. Essentially that too is a decision based on social knowledge.

The sociology of scientific knowledge

The implications for the scientist of strictly *personal* constructivism are absurd and intolerable. Methodological solipsism has always been considered incompatible with any serious scientific epistemology (e.g. Putnam 1981). As we have indicated, however, several slightly different versions of *group* constructivism have recently emerged in science studies (Knorr-Cetina & Mulkay 1983). These are presented under various labels, such as 'relativism', 'social constructivism', 'social epistemology', or 'the strong programme in the Sociology of Scientific Knowledge'. The acronym 'SSK' is conveniently concise as a general designation for this whole complex of notions.

The essence of SSK is that knowledge is not constructed by individuals acting independently, but through the action of social groups into which, for these or other purposes, they are consciously or unconsciously aggregated. Moreover, scientific knowledge is not to be considered an exception to the general sociological principle that people sharing certain interests - tangible or intangible - will tend to share a world view that favours those interests, and that they may well have constructed this world view for just this purpose (Latour 1987).

The imprecision of this principle is all too obvious. The very notion of a social group is very ill-defined, except perhaps for certain very exclusive clubs, very strict religious sects, very secret criminal gangs, and very primitive New Guinea or Amazonian tribes, all of which make a virtue of keeping themselves very severely to themselves. In many cases, the relationship between the interests that the members of a group supposedly share and the scientific ideas that supposedly favour those interests is also difficult to clarify. Metascientists who agree on the general principle are deeply divided concerning its interpretation and significance.

Fortunately, we do not need to enter into these debates. All we need to accept is that scientific knowledge does not come to us from 'on high'. It does not flow out of some source that is external to human social life. Nor, on the other hand, is it created and sustained out of the experience or cogitation of isolated individuals. Scientific knowledge is generated in the course of social interaction and negotiation within more or less distinct human groups. Members of the same group may thus come to share one or another of only a few distinctly different points of view. Indeed, elementary experience of social life, even where legal or political controversy abounds, does not support the ancient maxim, *quot homines, tot sententiae.* Although there can be many opinions on an issue of general interest or concern, there are usually far fewer essentially different opinions than there are people holding them. For our present purposes, there is no need to think about what motivates individuals to hold one or another of these opinions, nor the criteria by which they might perhaps be judged to be 'correct' or true'.

SSK thus takes interpersonal communication for granted as a fundamental component of any process of knowledge construction. Even in the case of academic science, this goes far beyond the elaborate machinery of refereed publications and official archives. Elementary common sense suggests that the social, and indeed sociable, processes of exchanging meanings which we have already described would play an important part in the way that individual scientists go about their business of 'constructing science'. At conferences, just as over cups of coffee in the laboratory canteen, they will be found to converse and persuade by all the logically disreputable but surprisingly compelling life-world methods (Latour & Woolgar 1979). Even that dry-as-dust piece of writing, the scientific paper, has a kind of rhetoric of its own, as authors strive to add conviction to the formality of their reporting (Medawar 1963, Ziman 1968, Bazerman 1988).

A scientific community at work

Knowledge claiming to be scientific requires to be presented as a more or less systematic, more or less codified, body of 'facts' and 'theories' generated by organised research. In the course of construction, it tends to become more rather than less elaborate, and less rather than more intelligible. The SSK principle can be applied to this at many levels, according to the scope of the social group being considered. For some purposes, the whole 'scientific community' can be regarded as a single group, shoring up its position of power in society at large by constructing a supposedly coherent, ubiquitous, conceptual scheme whose unchallengeable 'truth' makes it preferable to all others. Or one can consider the adepts of a particular scientific sub-discipline, such as elementary particle physics, constructing the ultimate Grand Universal Theory designed to underlie and explain the research results of all the rest of physics, and thus of supposedly less 'fundamental' disciplines such as biology.

Our concern here is more modest. We are interested in the process by which the members of an 'invisible college' (Price 1963) - a quite small group of scientists working in a narrowly specialised field of research - actually construct a small piece of what counts as 'established knowledge' in that field. Detailed sociological research has concentrated mainly on their life-world exchanges within the laboratory (e.g. Latour & Woolgar 1979, Knorr-Cetina 1981), the forms of discourse used in informal exchanges between groups centred in different laboratories (e.g. Gilbert & Mulkay 1984), and the 'political' factors that shape controversies over credibility and intellectual authority (e.g. Latour 1987). These modes of interpersonal communication and inter-group negotiation are common, however, to almost all organised social groups. It would prove nothing to demonstrate that they are also to be found in the school classroom, or in other contexts where science is being learned.

The key point is that scientists are differentiated from members of other social groups by the institutional practices that they are constrained to observe, the socially-structured roles that they are induced to play, and the behavioural norms they are expected to follow, in the process of justifying their research claims and getting them accepted as new science. Even though they inevitably perform these duties imperfectly, and often fall sadly short of their own communal ideals, professional scientists construct their knowledge inside a social framework that is as distinctive and formative as the classroom, teaching laboratory, lecture theatre or examination hall.

The norms of science

This framework can be encapsulated theoretically in the set of norms proposed by Robert Merton (1973). These are not to be interpreted as empirically descriptive nor as an explicit ethical code. They are too abstract and idealized to be practised perfectly, but they do delineate a moral map on which to locate a number of the procedures that are characteristic of the scientific world (e.g. Harré 1986). In other words, they are similar in principle to the unwritten norms underlying the behaviour of teachers and students in their school roles, such as: disciplined attention and respect for the authority of the teacher; concern for the intellectual development of the student; the right of the student to seek information and explanations from the teacher; the right of the teacher to test students' understanding; and so on.

In the first place, the construction of scientific knowledge is a *communal* enterprise. What this means in practice is that the members of the relevant research community make public their individual observational results, theoretical interpretations and critical comments. It also means that they tacitly accept a very substantial body of *established knowledge*, such as experimental data, basic theories, criteria of proof, technical methods, etc., already shared by this community. In this they draw quite explicitly upon what they have learnt as students, or make clear, by citing previous research by themselves or others, at what points they agree with or differ from this communal resource.

It could be said that school science also ought to satisfy this norm. That is to say, best-practice teaching envisages the creation of a public forum for ideas within the classroom, where students are encouraged to contribute to, help construct, and eventually accept for themselves, a new level of shared understanding. But there are, of course, significant differences, such as the diversity and irrelevance of much that students are able to bring into the process, and the practical limits to their interactions amongst themselves, rather than collectively or individually with the teacher.

To say that the research process is *universal* is to indicate that the community being addressed is not closed or hierarchically ordered, but that scientific communications are supposed to be given weight according to their intrinsic merits, rather than according to the personal status of their authors. In principle, the process is open to any person who can make a useful contribution, whether or not they are accredited to the community in question - for example, by having passed an examination or presented a PhD dissertation in the subject. In practice, however, active membership of such a community is usually restricted to persons who clearly show that they have mastered and accept its communal knowledge base, and the weight attached to their communications depends very much on their standing within the group.

In reality, therefore, the scientific community is not so different from almost all other organised groups in having a pecking-order of esteem and authority. But something would be seriously wrong with a scientific 'invisible college' were this personal authority to be as concentrated as it normally is in the school teacher *vis-à-vis* her pupils. The norms of the classroom may prescribe equal opportunity and even-handed treatment as between one student and another, but they certainly do not invite 'universality' in the intellectual exchanges between teachers and students.

The norm of *disinterestedness* is often misunderstood. It does not mean that individual scientists should not have a personal interest in the success or failure of their research claims: on the contrary the whole process is driven by the strong commitment of every scientist to his or her own ideas and by individual zeal in presenting them to the community. What the norm means is that this presentation should be done in a form that suppresses the public expression of this personal interest - i.e., as if impersonally and objectively - so as to limit the social disruptions inevitably generated by intellectual controversy (Ziman 1968). Like other social institutions, such as parliament or the law, science has developed a characteristic dramaturgical medium for its rhetorical interactions (Harré 1979) .

This notion of 'disinterestedness' is out of place in the science classroom. It is true that teachers must insist on elementary decorum in their exchanges with students, and strive to inculcate the basic principles of scientific discourse, such as objectivity, factual accuracy, logical consistency, etc. But these traditional virtues of 'the scientific attitude' come perilously close to the sort of pedantry that disguises the personal interest of the teacher in the progress of her pupils and stifles their own expression of their enthusiasm to learn new ideas. It also inhibits the use of a variety of more openly rhetorical explanatory devices, such as anthropomorphic metaphor, reference to life-world experience, historical re-enactments, or detailed analysis of particular cases, on the grounds that these would be quite inappropriate in the 'scientific domain'.

The norms of *originality* and *scepticism* bring many features of the SSK process close to more familiar philosophical approaches to the construction of scientific knowledge. These are the norms that generate what the scientists define as 'progress'. Thus, some element of novelty, whether empirical or conceptual, is a prerequisite for a scientific communication claiming to make a useful contribution. The community requires a continuous supply of new bricks from which to construct or reconstruct its knowledge. The enormous pressure on scientists to come up with something genuinely new forces them to become highly expert and extremely specialised, which in turn limits the scientific scope of each research community.

But, as positively emphasized by Popper (1963) and effectively taken as given by most philosophers of science (see, e.g. Callebaut 1993), these bricks must be tested in a systematic way, by organized communal criticism, before they are incorporated into the structure. This is performed by a variety of practices, such as peer review of research papers before they are published, public discussions in scientific meetings, favourable or unfavourable citations in other papers, and authoritative assessments of the current state of knowledge in review articles and books. Indeed, one might say that the other norms hold in place the social framework of a stable arena for this process of communal testing (Ziman 1968).

These two norms, continually generating scientific knowledge by their creative tension, pose a serious dilemma for science education. It is natural for any scientist to insist that they ought to be explicitly respected in the classroom, for they lay down the grounds on which science is to be believed, the extent to which it can be accepted as reliable, and so on (Ziman 1978). On the other hand, the *systematic* exercise of these norms, as envisaged in the 'discovery' method of teaching science, can rarely be an efficaceous didactic technique for encouraging every student to explore personally the scientific domain around every new concept as it is being internalised. Much has been written about the possibility of a heuristic of 'discovery' within science education, but in practice its proclaimed goals have proved unattainable (Solomon 1980)

Teachers and students are not engaged in the construction of new knowledge from the ground up. They know very well that they are committed, in effect, to the task of assembling a pre-conceived structure from a kit of parts designed to fit together in a certain way - that is, in accordance to 'established' knowledge in that field. This task is much too difficult, and life is much too short, to permit the exercise of these norms in relation to everything that is to be learnt. Indeed, there would be no guarantee at all that the personal constructs of the students would then merge and converge into a social construct equivalent to the accepted scientific view of the matter. In practice, students are encouraged to be 'original' mainly to expose the diversity of these personal constructs, and then to be 'critical' in expunging those that deviate from what the teacher holds to be the 'correct' line. There is thus a significant contradiction between the open marketplace for ideas in professional science and the 'managed' supply system of the average class room.

Connecting the individual with the community

A normative analysis of social action is incomplete without an account of the role of the individual in the relevant social group. One must ask, for example, what motivates an individual to comply with these norms, and what are the sanctions against non-compliance. In the Mertonian scheme, the motivational element is artfully indicated by collapsing the list of norms into the acronym CUDOS. In other words, each member of a research community is supposed to have a strong personal interest in becoming recognised as such through the communication of acceptable research claims, and in being rewarded with increasing esteem in larger and larger communities if these claims are judged to be acceptable.

It would take us too far away from our main argument to discuss the validity of this rather idealised model, which many sociologists now reject on principle for its 'functionalism'. Questions may be asked, for example, about the existential status of the norms, and the effectiveness of the sanctions against deviance from them. In any case, this aspect of the Mertonian scheme is rapidly becoming obsolete, as the traditionally individualistic structures of academic science are being made more managerial, more systematically accountable, and more overtly competitive (Ziman 1994).

An instructive comparison might be made here with the way that the motivation of both students and teachers inside the classroom is being changed by external political pressures. In the UK, government insistence for more explicit testing of student progress, performance appraisal of teachers, and direct competition between schools is rapidly transforming the school science classroom from an open space for the enthusiastic exercise of intellectual imagination into an arena for the competitive demonstration of prescribed skills. The connection here between individual motivation and social ideology is particularly clear.

A much more delicate issue is the connection between the individual and social forms of knowledge. In spite of the current emphasis on 'social epistemology', philosophy and psychology have a great deal to say about the former, but almost nothing in depth about the latter (see, e.g. Fuller 1988, Callebaut 1993). It is surprisingly difficult to find a systematic analysis of *intersubjectivity* - that is to say, the processes by which 'subjective' facts, concepts, interpretations, beliefs, etc., are communicated, negotiated, revised, and eventually become the common property of two or more persons (Ziman 1978, Solomon 1994). Such processes are evidently much more dynamic and interactive than the empathic or hermeneutic facilities now seen to be essential for the meaningful study of human behaviour or social action (Taylor 1985).

In the case of life-world knowledge, as we have argued earlier, this is achieved by a fluid matching of meanings between individuals who strive to agree, in a process which has little or no commitment to coherence or to an epistemology of any kind. In a scientific group, intersubjectivity still involves the familiar process by which conscious individuals communicate with one another in order to establish that they are 'in the same world' (or, as a methodological solipsist would have to say, 'having the same dream'). This time, however, the meanings to be matched refer to a shared scientific world and the sameness of the dreams has to be negotiated according to rather more rigourous principles.

Here again, we enter an area of fierce contention. Few metascientists would now agree that it is possible to define a uniquely scientific 'method', or to formulate an overarching set of 'constitutive principles' for the construction of scientific knowledge (see, e.g. Ziman 1984). The most that can be said, perhaps, is that each scientific discipline develops its own foundation beliefs, standards of observational adequacy, classes of admissible evidence, criteria of proof, and so on. But these principles differ considerably from discipline to discipline, and evolve significantly with time. For example, the style of argumentation that would have been considered thoroughly scientific in early 19th century geology, would cut not much ice in late 20th century biochemistry.

In this paper we take the view (Ziman 1968, 1978) that although these principles cannot be formally codified, they are not arbitrary, since they are closely linked to the normative structure of the relevant scientific community. Some of them, at least, seem generic to the natural sciences as a whole. Thus, for example, it is difficult to see how a community dedicated simultaneously to universalism, originality *and* criticism could even survive, let alone make any progress in the construction of knowledge, without acceptance of the logical principle of non-contradiction as a criterion for observational consistency, sound inference and valid criticism. Indeed, this is the principle that underlies the basic conventions of scientific discourse that all science teachers are expected to instil into their students. But this principle is only a precept, which has to be somewhat softened in the behavioural sciences, where ambivalent hermeneutic interpretations are often admissible in the search for understanding.

In any particular discipline, however, these methodological principles are items of stage furniture which are so well known to the scientific actors that it guides not only the steps they make on the scientific stage but also how they orchestrate meanings between themselves when they communicate scientific messages. Without such a shared frame for this matching process, individuals would be hopelessly locked into their personal constructs, with no possible means of coming together to create a communal version. Or to put this the other way round: their roles as scientists in this particular field positively require them to construct just such a communal version within just such a methodological frame, almost regardless of the diversity of features of their personal constructs that they do not have in common, and are forced to suppress or jettison in the process.

It is thus easy to understand the growing emphasis on the epistemological role of *language* in mainstream philosophy (e.g. Wittgenstein 1958), as well as in social studies of science (e.g. Fuller 1988) and in many other academic disciplines such as theology (Cupitt 1990). Language is more than a medium of *subjective* cognition: it is the means by which *intersubjective* cognition is achieved (Vygotsky 1979). Scientific communities are notorious for the highly specialised languages, such as advanced mathematics (Ziman 1978) that they generate and use for knowledge construction in their particular disciplines. But philosophers and linguists now reject the old notion of a completely 'logical', precise and unambiguous language for the expression of scientific knowledge. The basic communal institution, both for a scientific community and for a class of school students, must be a *natural* language, replete with all its uncertain analogies, dead metaphors and multiple meanings. For good or ill, for a prestigious paper or for a faltering fallacy, it is the medium through which cognitive communalism is practised and the means by which each individual is enabled to link his or her personal knowledge with the knowledge constructed by the group.

Social knowledge

What, then, is social knowledge? Nobody would suggest that a social group can 'know' anything, except through the minds of its individual members, or in the abstract symbolism of a textbook or archive. But we are surely interested in a more coherent concept than the sum, or perhaps lowest common denominator, of all that these members happen to know (Moscovici 1976). In the case of a research community or a group of students, we can properly refer to the resource mentioned above - the *accepted knowledge* with its associated norms - which actually forms and binds the group. In spite of much opinion to the contrary (e.g. Latour 1987), no other social concomitant - liking of others, enjoyment of laboratory activity, wish for profitable employment, or thirst for esteem - has a comparable power to make the community hold together for 'doing science'. In terms of our basic metaphor, scientific activity can be described as the process of building on, extending, reconstructing and solidifying a body of knowledge within a framework of norms and regulative principles of the kind indicated above. It is the intersubjective quality of this knowledge which makes this construction process possible (Ziman 1978).

For many scientists and science teachers, this may seem a very unsatisfactory definition of what the world at large regards as peculiarly true and reliable. It takes little thought to conclude that this formulation is circular and self-sustaining: an isolated research community has no means of preventing the perpetuation of some deep initial misconception over long periods. Indeed, this is precisely the case. History demonstrates the striking stability of scientific 'paradigms' (Kuhn 1962) long after the appearance of evidence that would challenge them.

But the epistemological mission of SSK is not completed by announcing: 'Relativism rules: OK!' There are still many unexplored features of the particular type of social action that we call science. For example, little has been made of the fact that research communities overlap extensively, and scientific specialties are often multiply connected into networks that cover large areas of what many scientists imagine to be a potentially complete map of knowledge (Polanyi 1958). Needless to say, there is total disagreement amongst metascientists as to the epistemological status of such a 'scientific world picture' and its (metaphysical?) relationship to (physical?) 'reality'.

In spite of their adherence in principle to the doctrine of unified science, research communities are often deeply concerned about the demarcation of their boundaries, and sometimes divide into factions over the degree to which their specialised constructs are 'reducible' within more general frames of meaning. In the classroom, by contrast, the overall unity of science has to be emphasised - perhaps exaggerated - to correct the distortions induced by the traditional mode of constructing knowledge in disconnected fragments, field by field, discipline by discipline.

The disestablishment of 'established knowledge'

The research community and the classroom group are deemed to be engaged in the same task - the construction of a particular body of 'established knowledge'. But this is a concept requiring further analysis. According to the protocols of official scientific communication - that is, the conventions governing the style and content of scientific papers (Ziman 1968, Bazerman 1988) - science only 'knows' what can be cited from its archives. But one of the achievements of the SSK critique of positivist philosophies of science has been to show that even formally published 'scientific facts' (as the general public calls them) are often very uncertain and highly disputable (Collins & Pinch 1993). The process by which they are supposedly tested is very imperfect and incomplete, and the implicit communal goal of a freely accepted consensus is always far beyond reach.

It is impossible, moreover, to separate 'established scientific knowledge', from the 'tacit knowledge' that is taken for granted throughout a research community (Polanyi 1958). Some of this is clearly technical, such as experimental procedures , instrumental capabilities, criteria of proof, etc. But the regulative principles and methodological precepts of a discipline do not exhaust the cognitive context in which scientific knowledge is constructed. As we have already indicated, it is essential to include the everyday knowledge and common sense understanding of the 'life world' shared by scientists as ordinary human beings - knowledge that is incorporated invisibly in natural languages and brought into play quite unconsciously in intersubjective communications between the members of a language community. The impossibility of codifying this component of all 'social knowledge' has so far foiled attempts to build a computational device capable of all the complex intellectual tasks actually required in the scientific construction of knowledge (see, e.g., Boden 1988).

Even well-established, apparently 'objective' knowledge cannot therefore be considered independent of the social circumstances in which it is constructed or construed. Popper (1972) argued that hypothetical 'men from Mars' should be able to reconstruct earthly science from a careful reading of the contents of its archives. But these communications, now disembodied, have been put together under the influence of professional norms which are scarcely comprehensible to persons who have not already been socialised into their use. With only verbal or pictorial accounts to go by, our Martians would struggle hopelessly with notions of communality or operational disinterestedness. And without experience of our peculiarly fluid way of utilising language and meaning, they would not understand how students and scientists take these communications on board and build them into their personal conceptions of the world.

Accepting science 'for real'

The ultimate touchstone of knowledge is that it should be 'for real'. We use this term in the phenomenological sense (Schütz & Luckmann 1974) to indicate the mysterious process by which a particular body of knowledge becomes an unquestioned source or criterion of thought and action. This is, of course, the supreme characteristic of the life world. We know ourselves to be in and of it, not merely through the intellect and senses, but through our emotions and bodily actions. Some of this knowledge is apparently incorporated into 'mental models' or 'internal maps' (e.g. Johnson-Laird 1983) representing, for example, 'my house', 'our town', family relationships, etc. But as we have indicated it is not necessarily coherent or self-consistent except quite 'locally', and may include a great deal of essentially disconnected items of information whose 'reality' has never had to be put to the test. Indeed, much of it is entirely idiosyncratic, being a 'personal' rather than a publicly communicable construct.

Knowledge in the scientific domain is supposed to be entirely non-personal. Nevertheless, for an experienced scientist who is actively involved in constructing and using it, this knowledge acquires the same sense of being 'for real'. This does not necessarily mean that it is 'real' according to traditional philosophical criteria, such as correspondence with a world that is independent of human thought. History clearly shows that people can take 'for real' whole systems of supposed facts and theories which are just not the case - and act accordingly. All that we are saying is that scientists adopt what Fine (1991) has called the 'Natural Ontological Attitude' to what they regard as well-established scientific knowledge, and spend at least a part of their lives within that frame. This is, perhaps, the tacit feature of Kuhn's concept of a 'paradigm' which has occasioned so much dissent amongst philosophers (Callebaut 1993).

In principle, the scientific knowledge constructed in the school classroom should have this same characteristic. In practice, this is very difficult, for two reasons. The most obvious reason is that time is too short. A professional scientist has years of instruction, technical practice and research in which to become familiar with the scientific domain, to internalise its interconnected meanings, and link these with personally experienced events in the lifeworld. The other reason is that scientific knowledge is typically conceived of, and presented in the classroom, as an abstract map, or idealised model, very different in style from most forms of lifeworld knowledge. Its conventional, arbitrary, incoherent, contingent, tacit characteristics are systematically concealed from school students, who are encouraged to construct a highly formalised over-arching version, mentally disconnected from anything that actually happens in the lifeworld. To most students, indeed, it seems bizarre to think of adopting the natural ontological attitude to such an alien form of life.

Thinking scientifically

One of the goals of science education, much emphasized by all advocates of 'public understanding of science', is to break down the barriers between science and the public. In effect, everybody should be taught, or coaxed, or beguiled into 'thinking scientifically' when the occasion demands. As we have seen, Kelly's romantic notion of 'every man his own scientist' is not a sound basis for such a project, although it does remind us of the pervasive human propensity for curiosity and speculation about the nature of things.

The intellectual membrane between scientific and 'ordinary' thought is certainly not impermeable. Science itself contains a large element of tacit lifeworld knowledge, and is continually exploring the possibilities of extending its formal coverage of the natural world. Over a period of time, many elements of well-established scientific knowledge diffuse to a wider public, where they lose their original 'scientificity' and become part of the life world knowledge. Certain ideas that have come to be shared without question by the scientific community are adopted by the whole natural language community. 'Proteins' and 'viruses' are now treated as unremarkable constituents of ordinary life. To most people nowadays, metaphorical expressions such as 'feedback' and 'quantum leap' come more easily than, say, 'yoking up' or 'dead reckoning'. A word such as 'energy' can no longer be considered simply as referring to some particular body of 'scientific facts' in the notoriously difficult terrain of thermodynamics, or as a technical term used unselfconsciously amongst physicists. The scientific meaning has become a familiar element of life-world knowing, taking its place alongside the other meanings attached to the same word (see Solomon 1992) and subject to the commonsense logic of ordinary discourse. The final product of the social construction of scientific knowledge often turns out to be just another of those comfortable 'facts of the life-world'.

Unfortunately, this is a very slow and uncertain evolutionary process that cannot be greatly speeded up by even the most enlightened science education. It is also irrelevant to an improved public understanding of the nature of science as a human activity. Some science educators have argued (see, e.g., Woolnough & Allsop 1985) that it is possible for a school student to be taught in such a way as to become 'a scientist for a day', but in the social context of the classroom this rhetoric is all too obviously empty. As we have seen, school students do not really construct their knowledge of science as if they belonged to a research community whose product is as yet unknown: they are involved in a guided process of 'reconstruction' with a complex, pre-conceived end.

As a result, what most students learn - beyond a hotchpotch of partially memorable trivia - seems to belong in an entirely separate domain from their lifeworld knowledge. Even students who become competent at manipulating theoretical concepts in this scientific domain, and solving problems involving such concepts, revert all too easily to lifeworld ways of thought and hardly notice that they have switched back to the comfortable and unstructured lifeworld domain. In order to 'think scientifically' in the fullest sense, they would have to 'own' their scientific knowledge to the same cognitive and affective depth as they do its lifeworld counterparts, and to link the two domains intuitively in thought and action.

This exploration of the parallels between two modes of 'constructing scientific knowledge' was not designed as a path towards positive policy proposals. What it shows, rather, is the real difficulty of the task that faces thoughtful science teachers, and the shallowness of many of the slogans telling them how to do their job much better. It also shows how much more might be learnt about the nature of science itself by studying it metascientifically in the classroom context. There is a major research programme here for the science studies community - provided, of course, that they accept science educators as genuine intellectual colleagues, with their own expert knowledge of the peculiar corner of the social world that we call a school.

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