# 3 Is mathematics socially shaped?

## The 'strong programme'

### 1 The planet that could only be seen from France

The most important advance in nineteenth-century astronomy was the discovery of a new element in the solar system. Since 1781, when Laplace had hypothesized that this new element was a planet called Uranus, astronomers had observed deviations by the planet from its predicted orbit. In the early decades of the next century, a number of scientists suspected that these deviations might be due to another, hitherto undiscovered, planet. In 1845, a student at Cambridge, John Adams, calculated the orbit of this hypothetical planet and reported his findings to the Greenwich Observatory, which was nevertheless unable to detect it by telescope. In the meantime, the director of the Astronomical Observatory of Paris, Urban Jean Le Verrier, had independently reached the same conclusions and in 1846 announced the discovery of a new planet, to which the name of Neptune was given. The discovery was hailed as a triumph by the French scientific community, which used it as a watchword in its struggle against the Church for the monopoly of knowledge about nature. Then, however, the American astronomer Walker calculated a new orbit for Neptune which was entirely different from the one worked out by Adams and Le Verrier. Was this the orbit of the same planet or of a different one? For the American astronomers it was a different one; for the French astronomers, who had made massive investments in terms of their public image and scientific authority in Le Verrier's discovery, it could only be Neptune, and the different orbits could only be due to errors of calculation (Shapin, 1982).

The controversy over Neptune's orbit is typical of the cases examined by the tradition of science studies carried forward by the so-called 'Edinburgh School'. After its foundation in 1966 by the astronomer David Edge, the Science Studies Unit of Edinburgh moved rapidly to the forefront in the social studies of science. Since then, Barry Barnes, David Bloor, Donald MacKenzie, Steven Shapin and Andrew Pickering are some of the scholars who have worked at the Unit. When first developing their approach to the sociology of science, the firm intention of these scholars was to oppose the institutional sociology of science that had become established in the US since the Second World War. The punctilious definition given to their subject of study as the 'sociology of scientific knowledge' (SSK), rather than simply as 'sociology of science', was an explicit declaration of intent to open the 'black box' of science which, in the opinion of the Unit's members, the institutionalized approach had left largely intact, doing no more than examine its external features.

Whereas the approach of Merton and his followers belonged largely within the sociological mainstream, the approach of the Edinburgh School has been clearly interdisciplinary from the outset. It makes extensive use of materials from the history of science (as well as conducting original case studies, although almost always from a historical perspective) and it engages in constant dialogue – albeit often critically – with the philosophy of science.

It should be emphasized that the SSK theorized at Edinburgh is based on case studies, and that it has simultaneously stimulated a large body of work by sociologists and historians of science. A valuable essay by Steven Shapin has organized this mass of studies into four broad areas on the basis of the analytical aims and significance of each of them.

The first area comprises studies that highlight the *contingent* nature of the production and evaluation of scientific findings. In other words, these are studies which reveal the existence of a 'grey area' between what nature offers to researchers and their accounts of it, and that this grey area may, in principle, comprise factors of a social nature.

For example, in 1860 the English biologist T.H. Huxley announced the discovery of a primitive form of protoplasm which he called *Bathybius Haeckelii*. His discovery was soon confirmed by other scholars, and the *Bathybius* was, for a long time, considered to be a 'fact', being cited in support of the nebular hypothesis of planetary evolution by numerous Darwinians, as well as by Huxley and Haeckel themselves. The *Bathybius* was taken to constitute proof of the continuity between non-living forms and living beings. Only subsequently did certain biologists begin to argue that the *Bathybius* was an artefact bred from a combination of 'observers' imagination and the precipitating effect of alcohol on ooze' (Shapin, 1982: 160).

In entirely similar manner, cellular meiosis was observed or denied by various groups of researchers until – following 'rediscovery' of Mendel's theories in the early twentieth century – chromosomic theory came up with an interpretative grid able to accommodate cytological observations. Golgi's corpuscle is another fact/artefact that has long made cyclical appearances and disappearances in observations by cellular biologists (Dröscher, 1998).

Shapin himself, however, admits that these studies

> open the way to a sociology of scientific knowledge [but] they do not by themselves constitute such a sociology. An empirical sociology of knowledge has to do more than demonstrate the *underdetermination* of scientific accounts and judgements; it has to go on to show *why* particular accounts were produced . . . and it has to do this by displaying the historically contingent connections between knowledge and the concerns of various social groups in their intellectual and social settings.
>
> (Shapin, 1982: 164, my italics)

This goal is achieved, according to Shapin, by the studies belonging to the second area – the one which uses professional interests as an element in sociological explanation. In the already cited case of the *Gilia inconspicua* (see Chapter 2), the criteria used by both sides to argue for the superiority of its own classification of the plant can be related to the desire of each to protect its conspicuous investments in learning, publications and reputation. The hypothesis that there exist tumour-provoking viruses – which subsequently won Temin, Baltimore and Dulbecco the Nobel prize for their discovery of the reverse transcriptase enzyme – inevitably provoked the scepticism of scientists who had spent lifetimes working under the 'dogma' that RNA could never generate DNA (Kevles, 1999). It is not rare for such conflicts to arise among scientists of different scientific affiliations. English biologists, unlike geologists, had been inclined to abandon a teleological view of natural history already before publication of Darwin's *Origin of the Species* (1859).

A theory that the adaptation of living beings was governed by biological laws, and not by a divine plan or by simple environmental determinism, enabled biology to free itself from the sway of geology; for geologists, by contrast, a teleological account enabled them to treat geological change as primary and that of living beings as its consequence (Ospovat, 1978, cf. Shapin, 1982). When the dispute erupted over the alleged discovery of cold fusion by Pons and

Fleischmann in 1989, chemists and physicists were not only in conflict over their respective purviews (who should study the phenomenon) but also over which signals constituted 'proof' that fusion had occurred: the production of heat according to the chemists, the emission of neutrons according to the physicists (Lewenstein, 1992a; Bucchi, 1996). During the already-mentioned controversy over zymase,[1] industrial mycologists were uninterested in detailed analysis of the cell's inner functions, which were of little relevance to their work; while those who had publicly supported the protoplasm theory were strenuously opposed to any recognition at all of zymase. From a theoretical point of view, the new results could be reinterpreted in the light of the old protoplasm theory, adapted so that a role could be given to enzymes. Yet, in the social domain the debate had by now polarized between two irreconcilable camps, with zymase being brandished by the biochemists as the symbol of a new era and of the struggle against the old establishment (Kohler, 1972).

According to SSK, what scientists 'see' and the explanations they give for it relate more generally to the role of science and scientists at a given historical moment, and to the level of professionalization and separation between experts and non-experts. This is the theme of the third area of studies singled out by Shapin. In the seventeenth century, French academics were reluctant to accept that meteorites came from the sky because accounts of their fall very often originated from peasants, or at any rate from 'non-professionals'. They were consequently deemed unreliable. Following the Revolution and, consequently, the change in attitude among intellectuals towards the common people, scientists began seriously to consider the connection between meteor showers and the fall of rocky objects in the countryside.

The fourth group of studies cited by Shapin enable him to argue that the role of social factors does not stop when scientific activity has been professionalized. In fact, it is possible to show that scientists make much use of images, models and metaphors from the more general culture at large. The source of these images may be for instance technological (an example being the mechanical pumps to which Harvey compared the heart) or political culture. The great biologist and political activist Virchow, for example, presented his conception of the organism made up of cells through analogy with his solidarist conception of a society in which individual citizens cooperate in the collective interest (Mazzolini, 1988). Better known and more widely studied is the influence exerted by Malthus' theory of social competition and individualism – ideas which pervaded Victorian society – on Darwin's development of his evolutionary theory (Gale, 1972; Young, 1973). George Poulett Scrope, one of the first geologists to hypothesize constant and long-period geological processes – thereby helping to discredit 'diluvial' explanations – also studied and wrote about political economy. His use in geology of the concept of time as an explanatory factor – 'neutral' with respect to other events, and potentially infinite – derived from his view of money as a means of circulation and exchange bereft of any intrinsic value (Rudwick, 1974).

Evelyn Fox Keller (1995) has described the history of biology in the twentieth century as the shift between two paradigmatic 'metaphors': a transition, that is, from a metaphor centred on the embryo and the organism's gradual development to one attributing to the gene – equivalent to the atom in physics – the capacity to 'construct' the organism on a predefined template. The former metaphor has been dominated by embryology; the latter has been characterized by the rise to predominance of genetics. This transition can be interpreted at various levels. One of them is specifically technical and has radically transformed the conditions and potential of biological research; the other is political and concerns the opposition and subsequent reconciliation between Germany – where the embryological paradigm held sway – and the US, where the genetic paradigm rapidly rose to dominance. At the cultural level, the genetic paradigm owes a great deal to the concept of information developed in cybernetics. And at an even broader cultural level, the waning of genetic determinism and the rediscovered importance of the 'cytoplasm' – the female part of the cell – owe a great deal to the feminist movement of the second half of the twentieth century.

The process also operates in reverse: images and concepts from science may be transferred into the political and social spheres. According to the SSK approach, the theories or explanations selected for such transfer depend on the specific circumstances of certain social groups, and on the specific strategies pursued by them.

An example is provided by phrenology. Developed during the nineteenth century from the work of the German doctor Franz Joseph Gall, this doctrine maintained that a person's psychological characteristics are located in specific zones of the brain, to which correspond bumps on the cranium. In the years around 1820, the theory provoked heated debate at Edinburgh University between phrenologists and anatomy lecturers. The dispute centred on different conceptions of the brain. This the university anatomists viewed as a unitary whole, whereas the phrenologists believed that it was an assembly of parts corresponding to different intellectual faculties. Both groups were made up of distinguished anatomists, and both groups performed careful dissections and examinations of the brain. For Shapin, phrenology gave the mercantile class the ideal means with which to challenge the academic elites. By turning phrenology into a dynamic theory of heredity, they could use it to highlight, besides the existence of certain traits inherent to the individual, also the possibility of altering or changing those traits by means of social reform. Not coincidentally, this view of heredity grew more entrenched as the bourgeoisie found itself having to cope more and more with the working class's demands for reform, and shifted its favour to eugenic theories in consequence (MacKenzie, 1976).

Thus, what Shapin calls *full circle* is achieved: 'connecting interests in the wider society to judgements of the adequacy and validity of esoteric mathematical formulations' (Shapin, 1982: 191). It is wrong, Shapin maintains, to yield to the temptation of separating the strictly technical component of a controversy from its 'cosmopolitan and methodological' ones.

> Anti-phrenologists' insistence that cranial bones in the region of the frontal sinuses were not parallel was explicitly connected to their claim that phrenological character diagnosis was impossible; phrenologists' assertion that the cerebral convolutions might show standard pattern and morphological differentiation was explicitly related to their view that mental faculties were subserved by distinct cerebral areas.
>
> (Shapin, 1982: 193–194)

We may likewise read the controversy on heredity that broke out in the early twentieth century between the biometrics school and the Mendelians. While the former propounded a rigid Darwinism, whereby evolution was the constant selection of minuscule differences, the latter embraced Mendel's recently rediscovered theories and their underlying hypothesis of more abrupt and discontinuous changes. According to Barnes and MacKenzie, this contrast reflected not only different technical competences and resources – for example, the biometricians made much use of mathematical-statistical tools – but also more general political and social attitudes. The biometric approach was compatible with the eugenic convictions and social reformism of the middle class, which pressed for political measures capable of shaping the development of society. The Mendelian approach instead reflected the conservative and non-interventionist views of the more reactionary classes (Barnes and MacKenzie, 1979).

These dynamics have also been used to analyse the controversy in statistics between Pearson – the leader of the biometrics school – and Yule. The dispute centred on the most appropriate correlation indicator for nominal statistical variables like ‘living/dead’ or ‘high/low’. The index proposed by Pearson – *rt* – was based on the hypothesis that such variables can be considered products of a bivariate normal distribution. Yule instead developed another index – Q – which dispensed with that assumption. In this case, too, the incompatible positions taken up (and backed by opposing ‘networks’ in the British academic community) can be linked with the different goals that Pearson and Yule believed that statistical theory should pursue. What was assumed to be ‘normality’, however, depended on the scientist’s broader vision of society – which in Pearson’s case was centred on eugenics and Fabian socialism (MacKenzie, 1978).

A further example is provided by the history of Italian mathematics and concerns one of the last of Italy’s mathematical ‘duels’, which was held in Naples in 1839. The tradition of mathematical duels dated back to the Renaissance, when they were frequently used to settle scholarly disputes. Originally watched by a crowd of spectators as two or more mathematicians strove to solve the same problems, with time these duels came to be conducted by correspondence or in the columns of learned journals. The duel in Naples resulted from a challenge issued by the mathematician Vincenzo Flauti against members of the ‘analytic’ school, whom he invited to solve three problems of geometry. A professor at the University of Naples and secretary to the Royal Academy of Science, Flauti was the leading exponent of the ‘synthetic’ school, whose teaching centred on pure geometry and the methods of classical mathematics. The founder of the school, Vincenzo Fergola, a fervent Catholic and the author *inter alia* of essays which asserted the effectively miraculous nature of the liquefaction of Saint Januarius’ blood, considered mathematics to be a ‘spiritual science’, on the grounds that it was pure, and consequently insisted that it should not be contaminated with practical applications. The analytic school was institutionally associated with the *Scuola di Applicazione del Corpo di Ingegneri di Ponti e Strade*, which trained bridge and road engineers, and was therefore more concerned with geometrical analysis and the application of calculus to empirical problems. The two schools had been at loggerheads since the beginning of the century, with the ‘analyticists’ accusing the ‘syntheticists’ of anti-scientific behaviour because they had ignored the algebraic revolution in French mathematics; while the syntheticists responded in kind, going even so far as to accuse their rivals of moral depravity.

In the end, the mathematics section of the Royal Academy, which was given the task of adjudicating the duel and awarding a monetary prize to the winner, pronounced against the analytic school: a judgement prompted, according to several scholars, by the closer compatibility of the synthetic school with the counter-revolutionary policy of the Bourbons and the Catholic Church (Mazzotti, 1998).

What conclusions can we draw from these various examples? Shapin warns against adopting the unsatisfactory and caricatured version of the sociology of knowledge which he calls the 'coercive model'. This model, in fact:

a claims that sociology asserts that all individuals in a certain social situation will adopt a certain intellectual belief;
b treats the social as a mere aggregation of individuals;
c establishes a deterministic relationship between social situation and beliefs;
d views sociological explanation as concerned with 'external' macrosociological factors;
e opposes sociological explanation to the assertion that scientific knowledge is empirically grounded on sensory inputs from natural reality.

None of these statements reflects the SSK approach and its thesis that 'people produce knowledge against the background of their culture's inherited knowledge, their collectively situated purposes, and the information they receive from natural reality'. In this regard, the exponents of the SSK have taken especial pains – and here again they depart sharply from the Mertonian tradition – to reconstruct in detail the activities, methods and concrete experimental practices of scientists. Many of the members of the Science Studies Unit, moreover, had scientific backgrounds: Edge came to it from astronomy, Barnes from physics and Bloor from cognitive science. 'The role of the social' concludes Shapin 'is to prestructure [scientist's] choice, not to preclude choice' (Shapin, 1982: 196, 198).

## 2 Is even mathematics 'social'?

The proponents of the SSK have examined the relationship between science and society from various points of view. Yet the Edinburgh school has often been identified – by its critics especially – with the so-called 'strong programme', the classic formulation of which was set out by David Bloor in his *Knowledge and Social Imagery*

(1976). Although Bloor and his book have been regarded – again by critics especially – as epitomizing the sociology of science, it should be borne in mind that Bloor developed his interest in the philosophical and sociological analysis of science after earning a doctorate in psychology. His main intention, as he recalls today, 'was to show to philosophers of science that in the light of a wide range of studies, mainly carried out in the history of science, it was not possible anymore to hold a vision of science as exempt from social influences'.[2]

The core of the 'strong programme' consists of a set of methodological principles for the sociological analysis of scientific knowledge. According to Bloor, such analysis should be:

(i) *Causal*, i.e. concerned with the conditions which bring about beliefs or states of knowledge.
(ii) *Impartial* with respect to truth or falsity, rationality or irrationality, success or failure. Both sides of these dichotomies require explanation.
(iii) *Symmetrical* in its explanation. The same types of cause should explain true beliefs and false ones.
(iv) *Reflexive*. In principle its patterns of explanation should be applicable to sociology itself, which obviously cannot claim to be exempt from sociological analysis.

(Bloor, 1976: 4–5)

Bloor obviously does not deny that there exist 'other types of causes apart from social ones which will cooperate in bringing about belief', but his intention is to give greater dignity and pervasiveness to sociological explanation. Social factors like interests, political ideologies and cultural features, he maintains, should not be brought to bear solely when knowledge jumps the rails of rationality or lapses into error. This attitude – which Bloor views as characterizing most of the preconceived objections made against the sociological approach to the study of science – sees 'logic, rationality and truth' as 'their own explanation . . . it makes successful and conventional activity appear self-explanatory and self-propelling' (Bloor, 1976: 6). On this view, sociological explanation should only intervene when some anomaly (which cannot but be 'social') deviates rationality and progress towards the truth from their automatic course. Sociology could thus explain – by invoking religious or political or more generally cultural factors – Kepler's mystical beliefs about the sun, or the astronomer Schiaparelli's conviction that Mars was populated by human beings organized into some sort of socialist collective. It could also explain the ‘Lysenko case’ – that of the Stalinist biologist who for many decades suppressed the Mendelian theory of genetically transmitted traits, arguing in obeisance to communist ideology that they instead depended on environmental conditions. But it could not explain the factors responsible for the success of Darwinism or of Virchow’s cellular theory. It is this ‘weak programme’ that Bloor’s theoretical proposal opposes.

In order to illustrate the symmetry principle, Bloor refers to a comparison made by Morell between two schools of chemistry research in the early 1800s: Liebig’s school at Giessen, and Thomson’s school in Glasgow. According to Bloor, the radically different fortunes of these two schools (international success for Liebig’s, oblivion for Thomson’s) cannot be explained solely on the basis of the experimental results achieved by the two great scientists. Also responsible were factors such as the personalities of the scientists who headed the schools; their status and relative abilities to obtain funding for their laboratories; and their choice of sector in which to conduct their research. For example, Thomson was working in a political context where it was impossible to obtain public funding, which was instead amply available to Liebig. In his dealings with his pupils, Thomson tended more to exploit their labour than to set value on it. Finally, Thomson chose to work in a mature sector, that of inorganic chemistry, where experts like Berzelius and Gay-Lussac had already made glittering reputations, and where it was difficult to come up with innovative and significant results. The sector of organic chemistry chosen by Liebig was of more recent development, less structured and less dominated by other researchers, and it was characterized by simpler experimental procedures, easier to teach to pupils.

A possible objection against the strong programme is the so-called ‘argument from empiricism’, which runs as follows: ‘social influences produce distortions in our beliefs whilst the uninhibited use of our faculties of perception and our sensory-motor apparatus produce true beliefs’ (Bloor, 1976: 10). Bloor meets this objection by pointing out that an increasingly negligible part of knowledge – and scientific knowledge in particular – derives directly from the senses. The perception of scientists themselves – not to speak of non-scientists – is mediated by complex instruments and by elaborate intermediation apparatus (publications, experimental equipment, the mass media).

It is therefore impossible to distinguish sharply between ‘truth = individual experience’ and ‘error = social influence’. Indeed, it is precisely the social dimension (the sharing of standardized experimental practices, agreement on criteria and procedures, repeatability and controls) that guarantees the functioning of science despite distortions in the individual perceptions of researchers. It is not brute experience or observation that stands at the centre of scientific activity but socialized activity, 'repeatable, public and impersonal' (Bloor, 1976: 26).[3]

To illustrate the point more thoroughly, Bloor recounts the well-known story of Blondlot's N-rays. Blondlot, a French physicist and member of the Academy of Science, announced in 1903 that he had discovered a new type of radiation similar to X-rays. One of the properties of his N-rays was that they were polychromatic: when passed through an aluminium prism, Blondlot claimed, they could be shown to comprise elements with different indices of refraction. During a visit to Blondlot's laboratory, the American physicist Robert Wood surreptitiously removed the prism; even so, Blondlot continued to see signals emitted by the N-rays. Wood wrote an article about his visit for the journal *Nature* in which he concluded that N-rays did not exist: they had simply been produced by Blondlot's desire to discover another type of radiation.[4]

'Sociologists', Bloor comments,

> would be walking into a trap if they accumulated cases like Blondlot's and made them the centre of their vision of science. They would be underestimating the reliability and repeatability of its empirical base; it would be to remember only the beginning of the Blondlot story and to forget how and why it ended. The sociologist would be putting himself where his critics would, no doubt, like to see him – lurking amongst the discarded refuse in science's back yard.
>
> (ibid.: 25)

The point for Bloor is not that observation or data from experience are valueless; rather, the point is that they do not suffice in themselves to bring about change in beliefs. Bloor depicts the relationship between experience and beliefs as in Figure 3.1.

Scientific theories and results are often 'under-determined' by observational data. In this regard Bloor furnishes a series of examples of how the same perceptive or observed data can be interpreted in completely different ways. He cites the elementary case of the apparent diurnal movement of the sun, which has been interpreted in different epochs and observational contexts as demonstrating the sun's rotation around the earth, but also the other way round. Another example is the 'parallel roads' along the sides of Scottish valleys;

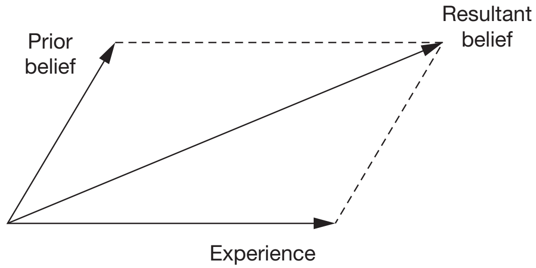


*Figure 3.1* The relationship between experience and beliefs
*Source:* Bloor (1976: 27)

these are geological phenomena even though they look like man-made paths. On the basis of his observations of similar 'roads' during his travels in South America, Darwin thought that they were due to the erosive action of the sea; Agassiz, a geologist who had studied the Swiss glaciers, offered the entirely different explanation that they resulted from lakes imprisoned during the Ice Age.

The geologist Alexander Du Toit – among the first to endorse Wegener's hypothesis of continental drift when it was still being dismissed as absurd by a large part of the scientific community – lived in South Africa, and there the evidence of the break-up of the continents was more obvious than elsewhere. His contribution to the theory was to replace Wegener's Pangaea with two original continents, Luarasia and Gondwana, with the centre of the latter located precisely in what is today's South Africa.

Whereas Priestley, on placing a gas flask in a water bath on which a small pot of minium was being heated, saw the red lead absorb phlogiston and change into lead, we, today, see the oxygen separate itself from the lead oxide and leave the lead as a deposit.

Bloor even goes so far as to apply the strong programme to the scientific discipline usually considered most impermeable to the influence of social factors: mathematics. His concern in this case is to show that even formulas, proofs and elementary results do not have an intrinsic meaning but depend on a set of presuppositions. The proof that the square root of two is an irrational number may lose significance in a mathematical system in which the concept of even and odd do not exist; or it may be interpreted (as it was by the Greek mathematicians) as proof that the square root of two is not a number at all. To different institutional and cultural contexts may correspond different logics or mathematics. Even the solution of a mathematical problem may be the result of a complex negotiation. In this regard, Bloor takes from Lakatos (1976) the example of Euler's well-known theorem on polyhedra which relates their number of vertices, edges and faces thus:

$$V - E + F = 2$$

To this theorem, which was formulated inductively by Euler in 1752 and demonstrated by Cauchy in 1813, Lhuiler and Hessel found an exception: the polyhedron shown in Figure 3.2, which satisfied the standard definition (a solid whose faces are polygons) but not Euler's theorem. It was therefore necessary to reformulate the definition of a polyhedron as a 'surface composed of polygonal faces'. Shortly afterwards, however, further exceptions were discovered, like that shown in Figure 3.3. This time it was the proof that had to be reformulated as being valid only in the case of simple polyhedra – ones, that is, whose faces could be flattened. But Figure 3.4 shows a simple polyhedron for which Euler's theorem does not hold.

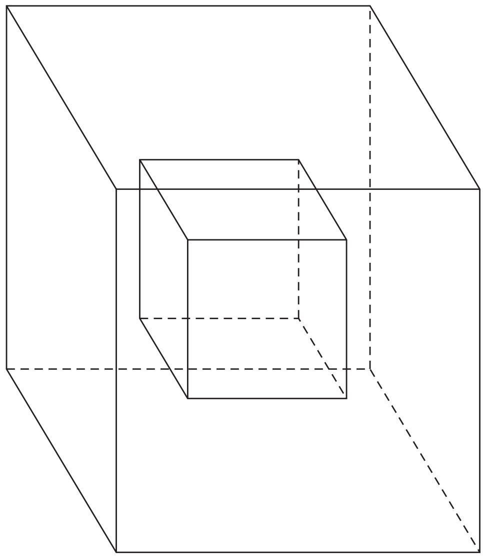

*Figure 3.2* Lhuiler and Hessel's polyhedron

*Source:* Bloor (1976: 133)

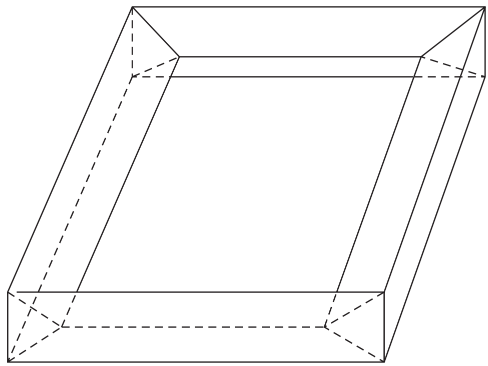

*Figure 3.3* An example leading to the reformulation of Euler's theorem
*Source:* Bloor (1976: 134)

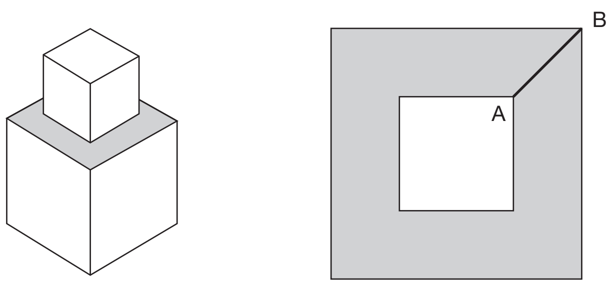


*Figure 3.4* A further exception to Euler's theorem
*Source:* Bloor (1976: 135)

According to Bloor, this example shows that not even in mathematics are there immutable definitions and postulates from which proofs and theorems 'automatically' and invariably derive. Instead, constant negotiations take place over the definitions themselves; negotiations which, in the specific case of Euler's theorem, concerned what a polyhedron actually is and whether exceptions should be incorporated into the theorem by modifying it, whether they should be rejected as 'non-polyhedra' (perhaps by restricting the definition), or whether they should be deemed to confute the theorem. The choice of one or other of these options can be related to the social and institutional context in which the researcher is working. For example, a closed and strongly cohesive scientific community based on loyalty to a specific theory or result, and where greatest value is set on obedience to tradition, may see any counter-example as a threat to its existence, and therefore tend to expel exceptions to the Euler/Cauchy theorem, calling them – as Mathiessen did, for example – 'recalcitrant cases' (cited in Bloor, 1982: 200). In a more differentiated context, where diverse groups of mathematicians work in diverse institutional settings (academies, universities, journals), an anomaly can live together with the rule: the theorem can be retained with certain restrictions or deemed valid under certain conditions; 'no formula has indeterminate validity', was Cauchy's riposte to the counter-examples brought against his theorem. Finally, a highly competitive and individualistic context, in which originality and innovation are rewarded, will opt for a 'revolutionary' response and therefore abandon the theorem (see Chapter 2).

## 3 The weaknesses of the strong programme

Though generally recognized as ambitious, Bloor's endeavour has been considered by several critics as not entirely successful. Some of them have argued that if the declared objective of his work, and that of the Edinburgh school in general, has been to delve into the 'black box' of science – at whose exterior Merton came to a halt – it has not been completely achieved.

Perhaps with excessive over-simplification, a philosopher of science particularly critical of the sociological approach has singled out four versions of what he calls 'externalism' (the view that the context is able to determine the content of scientific research) (Bunge, 1991):

(a) Moderate or weak externalism: knowledge is socially conditioned.
(a1, local) The scientific community influences the work of its members.
(a2, global) Society as a whole influences the work of individual scientists.

(b) Radical or strong externalism: knowledge is social.
(b1, local) The scientific community constructs scientific ideas.
(b2, global) Society as a whole constructs scientific ideas.

Bloor's approach seems, at times, to restrict itself to conceptions little different from those of Merton and his school, lying midway between (a1) and (a2) – especially when it analyses the influence of factors like the style of the leaders of the Liebig and Thomson schools, and more generally of the economic-social context, on their differing fortunes. Elsewhere, Bloor appears to adopt a perspective close to Kuhn's, or especially Fleck's, when he argues that it is theoretical predispositions or proto-ideas that guide observation or the conduct of experiments, not the other way round.

It is not that these various gradations are mutually incompatible. Indeed, Bloor sometimes seems to theorize a kind of sociological opportunism whereby the role of the social component may vary from a minimum to a maximum according to the type of scientific case under examination. 'When the signal noise ratio is as unfavourable as this' – the reference being to Blondlot, but also to Huxley and his *Bathybius* or Golgi's corpuscle – 'then subjective experience is at the mercy of expectation and hope' (Bloor, 1976: 25).

But the danger of this attitude is that it may push sociology back into the residual role of dealing with the 'rejects' of science (gross errors, cases of deviance) – a role which Bloor explicitly opposed, and to do so formulated the symmetry principle.

Numerous critics have pointed out the ambiguity of this principle. According to Ben-David, for instance, the examples furnished by Bloor do not satisfy the criteria of covariance and causality. If a specific interest or cultural orientation determines the adoption of a particular scientific perspective, then a change in the former should necessarily give rise to a change in the latter. But this obviously does not always happen: numerous theories or approaches may succeed one another in the same political or cultural context. Bloor responds to this objection by restating the claims of his approach: '[This point] would be fatal only to the claim that knowledge depends *exclusively* on social variables such as interests' (Bloor, 1991: 166, italics in the original).

> Doesn't the strong programme say that knowledge is purely social? . . . No. The strong programme says that the social component is always present and always constitutive of knowledge. It does not say that it is the *only* component, or that it is the component that must necessarily be located as the trigger of any and every change: it can be a background condition. Apparent exceptions of covariance and causality may be merely the result of the operation of other natural causes apart from social ones.
>
> (Bloor, 1991: 166, italics in the original)

A more sophisticated criticism has been brought against the relationship between social and 'natural' factors. Consider again the example of phrenology. According to Shapin, the two sides in the controversy differed in their views because they came from different social backgrounds. While the anatomy lecturers were an elite characterized by an esoteric notion of knowledge, most of the phrenologists were amateur scientists, often tradesmen or members of the middle class, who espoused a more 'accessible' conception of science.

The objection by scholars like Brown is that the under-determination of theories with respect to data does not automatically entail that interests play a decisive role. 'In fact,' Brown objects, 'just as there are infinitely many different theories which will do equal justice to any finite set of empirical data, so also are there infinitely many theories which will do equal justice to a scientist's interests' (Brown, 1989: 55).

In other words, if it was the intention of the Edinburgh middle classes to undermine the cultural hegemony of the aristocracy, why did they choose precisely phrenology for the purpose? Was the synthetic school in Naples the only mathematical approach compatible with the political and religious concerns of the Bourbon and religious authorities? What is it that makes social factors and scientific theories overlap?

Bloor's answer is plausible, as he says that there was no necessary reason for the opponents of the university elite to choose phrenology rather than any other theory for their purposes. 'Perhaps anything materialistic, empiricist and non-esoteric would have served as the not-X to the elite X' (Bloor, 1991: 172). 'Once chance favours one of the many possible candidates,' concludes Bloor, 'it can rapidly become the favoured vehicle', thus flanking the causality principle with a randomness principle. I shall return to this point later, because I believe it to be of considerable importance, though perhaps in a sense slightly different from that envisaged by Bloor.

Another weakness pointed out in the approach is its tendency to identify the social with interests, even though its proponents often use the latter term in a broader sense than mere material interests. The linkage between the cognitive dimension (the interpretative flexibility of which Bloor provides numerous examples at the micro-sociological level) and the macrosociological one of interests and social circumstances have sometimes been regarded as not made fully explicit. Paradoxically, two opposite critical reactions have been put forward on this point. On the one hand, the Edinburgh authors have been accused of transforming scientists into 'interest dopes'[5] or 'flat, puppet-like characters who were shaped by exogenous interests rather than a complex set of contingencies and motivations' (Hess, 1997: 92). On the other hand, it is possible to discern at the basis of the strong programme an equally idealized image of the omniscient scientific actor, perfectly rational, and able to choose consciously between one theory or method and another on the basis of his or her interests and those of the group to which s/he belongs.

What is certain is that the charge of radical relativism and constructivism brought against Bloor is largely unwarranted. And not only because he himself considers his mission to have been a 'positivist' attempt to apply a scientific method to the study of the relationship between science and society.[6] This is borne out by the consideration that over the years the strong programme has also been subjected to fierce 'internal' criticisms by sociologists of science themselves, and with regard to two aspects in particular. The first is the just-discussed one of causality. The SSK, the argument runs, does not greatly differ from Merton's model and that of the institutional sociology of science, for it does no more than replace norms with interests as the factors explaining how scientists behave. A large part of the studies discussed in the following chapters have been prompted by the more or less explicit intent to find alternatives to Bloor's allegedly too rigid model.

The second set of criticisms centres on the final 'commandment' of the strong programme: reflexivity. Some sociologists of science have emphasized the scant ability of the SSK theorists to apply the tools developed by themselves to the sociological analysis of scientific knowledge. The alternative proposed is that new narrative forms – dialogue, multi-voice or first-person narrative – should be used to bring out the nature as constructs of their own texts (Woolgar, 1988) or to make the 'social positioning' of their own observations explicit, as has been later attempted by feminist strands of science studies (Haraway, 1997).

## Notes

1 See Chapter 2.
2 Personal communication, 4 June 1999.
3 Studies like those by Shapin and Schaffer on the controversy between Hobbes and Boyle have shown in more detail how the adoption of the 'empirical style' by science results from a complex historical-social process (Shapin and Schaffer, 1985). Today known only for his political theories, in seventeenth-century England Thomas Hobbes was also an active proponent of natural philosophy. His search for stability in natural philosophy based on logical argument, and according to which the very concept of vacuum was to be repudiated, found rebuttal by Boyle with an instrument that settled the matter: a machine able to 'produce facts',

namely the air pump used in his experiments on the vacuum at the Royal Society. A 'local' experiment witnessed by a restricted number of gentlemen – and who were therefore trustworthy – and then written up in detail was transformed into the 'matter of fact' able to bring everyone to agreement (see also Chapter 7).

4 Ashmore (1993) has analysed Wood's report in detail, showing that a 'trick' – surreptitiously removing Blondlot's prism – non-repeatable and more of an experiment in social psychology than physics, has been unproblematically incorporated into the literature and celebrated as epitomizing the scientific method, even by philosophers and sociologists of science.

5 The expression is used by analogy with that of 'cultural dope' coined by the founder of ethnomethodology, Harold Garfinkel, with reference to the way in which traditional sociological theories, especially Parsons', view the individual (Garfinkel, 1967).

6 Personal communication, 4 June 1999.