

# Introduction to Georges Canguilhem

*Mike Shortland*

Take away Canguilhem and you will no longer understand much about Althusser, Althusserianism and a whole series of discussions which have taken place among French Marxists; you will no longer grasp what is specific to sociologists such as Bourdieu, Castel, Passeron and what marks them so strongly within sociology; you will miss an entire aspect of the theoretical work done by psychoanalysts, particularly by the followers of Lacan.<sup>1</sup>

Many readers will find Foucault's recent tribute to Canguilhem a little disconcerting. And what of Althusser's own remark that 'I feel bound to acknowledge the obvious or concealed debts which bind us to our masters in reading learned works, once Gaston Bachelard and Jean Cavaillès and now Georges Canguilhem'?<sup>2</sup> Disconcerting ... and perhaps even obtrusive. For, ten years after Althusser, Bourdieu, Lacan (and Foucault himself) first appeared in translation, we are all now in some senses party to the industry of criticism they inaugurated; indeed, now that we are moving into the calmer waters of 'post-structuralism', it *is* disturbing to be reminded of an almost neglected part of our intellectual ancestry. All we have of Canguilhem's work in English is his 1943 doctoral thesis on pathological structures with some 'New Reflections' dated 1963-66, an essay on models and analogies in biology, a couple of brief articles on the history of animal physiology, and his short lectures on psychology.<sup>3</sup> This, and a small harvest of criticism.<sup>4</sup> The major writings of his predecessor at the Institut d'Histoire des Sciences et des Techniques de l'Université de Paris, Gaston Bachelard, remain untranslated and will be known only through Lecourt's study and the articles by Gaukroger and Bhaskar.<sup>5</sup> Cavaillès' pioneering work on axiomatic methodology, set theory and the logic of science has been even more poorly served.<sup>6</sup> The article we publish below remains the most general and accessible statement of Canguilhem's views on the nature of the history of the sciences. Apart from the intrinsic interest the article may have as a contribution to the 'deontology' of that discipline, we hope that its publication will prove to be the first step in illuminating an area of work which has hitherto escaped notice in this country.

Three questions are commonly asked about the practice of the history of the sciences: *Who? Why? and How?* As Canguilhem elsewhere suggests,<sup>7</sup> the answers to the questions *Who?* (which clearly implies the question *Where?*) and *Why?* reveal an ominous ambiguity in the siting of the discipline between philosophy, history and science. The philosopher

appears to need it because any theory of knowledge disassociated from epistemology would be splintered into timeless (and spaceless) fragments, and because without a history of the sciences epistemology would simply involve repeating *ad nauseam* the statements of the science it proposed to deal with. This should not, however, be taken to suggest that the history of the sciences can be a testing ground for epistemology; to do so would involve us in equating the object of that history with the object of science itself. That is: to advance the epistemological thesis that there exists an eternal, unitary scientific method. Far from assuming that science, so to speak, comprehends its history within itself, Canguilhem wants epistemological judgements to be made on the basis of the past of knowledge and knowledge of the past.

Epistemology, then, serves to provide the history of the sciences with judgements founded on the current discourses of science, whereby the historian can retrace the steps any particular science takes until the point is reached at which that science becomes epistemologically unintelligible. In other words, epistemology can be deployed to discern two types of history: the history of 'lapsed' knowledges and the history of knowledges which are 'sanctioned' by the current activities of scientific research.<sup>8</sup> Present-day sciences do not furnish us with ready-made 'truths' which can be used to form a teleology, but can instead be the instruments to help us understand and explain how, and in what ways, notions and attitudes which have been displaced by progress are truly part of their own pasts, rather than simply being labelled 'non-scientific' or 'ideological', or, as Canguilhem suggests below, being characterised as scientific ideologies.

Thus, the history of the sciences has a historical object: the *historicity* of scientific discourse. (Canguilhem's position here links up firmly with Bachelard's historical epistemology.<sup>9</sup>) Though the full import of this approach to the practice of the history of the sciences can only be gauged from Canguilhem's case studies,<sup>10</sup> it is possible to indicate some of the consequences it will entail for the theory of that history.

Since each science has its own properly constituted object, its own norms, rhythms and temporalities, the history of the sciences cannot be written in the language of continuity or evolution - as the measure of a uniform drift towards the better. Science proceeds instead through a process of re-organisation (in respect to its object and norms) and a process of rupture and mutation (in relation to ideologies and

pre-scientific knowledges and so on). Each of these steps, or breaks, or progressions has the effect of recasting the history of a science and of reformulating its epistemological foundations.

The practical consequences of a theory of history which accords relative autonomy to the space in which theoretical problems generated by scientific practice can be studied, is twofold: to purge what Canguilhem terms the 'virus of the precursor', and to eliminate the 'epidemic of accidents'.<sup>11</sup> The search for accidents and precursors - or even their 'unexpected discovery' - requires the historian to isolate any explanatory system from its cultural context and allow theoretical problems and concepts free play in a wholly disengaged, uncontaminated intellectual space. Moreover, it denies the specificity and effect of the social formation in which scientific practices are inscribed and transforms historical relations into a logic of scientific development. As the article shows, Maupertuis can only be viewed as a precursor to Mendel at the cost of reducing the theory of heredity to a meditation on an eternal theme - a theme derived *of course* and *always* from 'common-sense' observations, and held together by the ambiguity of metaphors or linguistic terms. Much the same might be said of the 'accidental' discovery of Mendel's paper on plant hybridization after it had gathered dust for 34 years.

In summary, then, Canguilhem is saying that a philosophy of history which inspires the narrative of chronicles and contingencies simply degenerates into the specifically epistemological annihilation of the actual historicity of knowledge. His project is an attempt to reinstate this historicity by reuniting two disciplines: the history of the sciences - which

'takes ideas as facts' - and epistemology, which 'takes facts as ideas by inserting them into a conceptual system'.<sup>12</sup> Until now we have had to gauge the success of this venture by examining the work of those influenced by Canguilhem's method and approach. The publication of *What is a Scientific Ideology?* will, we hope, stimulate a more informed discussion of the nature and practice of epistemological history and a more profound understanding of the work of Foucault, Althusser, Serres, Dagognet and Lecourt. We invite readers to contribute to that discussion with comments, notes or articles.

## Footnotes

- 1 Michel Foucault, 'Introduction' to G. Canguilhem, *On the Normal and the Pathological*, Dordrecht, Holland, 1978, p.ix. See also Foucault's 'Georges Canguilhem: Philosopher of Error', *Ideology & Consciousness*, 7, Autumn 1980, p.51.
- 2 Louis Althusser, *Reading Capital*, London, 1970, p.16n.
- 3 'The Role of Analogies and Models in Biological Discovery', in *Scientific Change*, ed. A.C. Crombie, London, 1963; *A General History of the Sciences*, ed. René Taton, London, 1963-6, Vol.II, pp.527-39 and Vol.III, pp.414-21, 'What is Psychology?', *Ideology & Consciousness*, 7, Autumn 1980.
- 4 See D. Lecourt, *Marxism and Epistemology*, London, 1975; Colin Gordon, 'The Normal and the Biological: a Note on Georges Canguilhem', *Ideology & Consciousness*, 7; Mike Shortland, 'Disease as a Way of Life', *Ideology & Consciousness*, 9, Autumn 1981.
- 5 D. Lecourt, *op.cit.*; S.W. Gaukroger, 'Bachelard and the Problem of Epistemological Analysis', *Studies in the History and Philosophy of Science*, 7, 1976; R. Bhaskar, 'Feyerabend and Bachelard', *New Left Review*, 94, Nov-Dec 1975.
- 6 Jean Cavaillès, *Sur la logique et la théorie de la science*, Paris, 1947 and *Philosophie mathématique*, Paris, 1962.
- 7 i.e. in 'L'Objet de l'histoire des sciences', in *Etudes d'histoire et de philosophie des sciences*
- 8 See 'L'Histoire des sciences dans l'oeuvre épistémologique de Gaston Bachelard' and 'Gaston Bachelard et les Philosophes' in *Etudes*.
- 9 See Canguilhem's 'Sur une Epistémologie Concordataire', in *Hommage à Gaston Bachelard*, ed. G. Bouligand et al., Paris, 1957.
- 10 e.g. *La Formation du Concept de Réflexe aux XVIIe et XVIIIe Siècles*, Paris, 1955 and *La Connaissance de la Vie*, Paris, 1965.
- 11 *Etudes*, p.20.
- 12 Gaston Bachelard quoted in *Etudes*, p.177.

# What is Scientific Ideology ?

**Georges Canguilhem**

Translated by Mike Shortland

What is a scientific ideology? The question, which seems to me to be posed by the *practice* of the history of the sciences, is one whose solution would have an important bearing on the theory of that history. After all, is not the first thing we need to determine *of what* the history of the sciences claims to be a history? The easy answer seems to be that it is a history of those forms of culture that are sciences. But it still remains to specify by what criteria any practice or discipline, in any historical period, can be judged to merit its self-imposed title of science; for it *is* a matter of title, that is, of a claim to privileged status. And then we are bound to ask whether the history of what is genuine science must exclude or tolerate, or else claim and incorporate a

history of the evictions of the fraudulent by the genuine. *Eviction* is used intentionally here, to indicate the juridical dispossession of a title acquired in good faith. Unlike Voltaire, we have long ceased to lay superstitions and false sciences to the charge of frauds and intrigues cynically invented by crafty dervishes and perpetuated by ignorant nursemaids.<sup>1</sup>

What we are dealing with is clearly more than a technical problem, or a problem of historical methodology concerned with the past of scientific knowledges such as it can be reconstituted from documents and archives. The problem is really an epistemological one concerning the permanent mode in which scientific knowledges are constituted in history.

In his Report to the First Session of the XIIth International Congress of the History of the Sciences, *Les Facteurs du développement de l'histoire des sciences*, Professor Suchodolski posed a similar question in these terms:

If the whole of the history of science to the present day were instead the history of 'anti-science', this would no doubt prove things could not have been otherwise and would in all likelihood remain so in the future.... The history of science as a history of truth is quite unrealisable. The conception itself is internally contradictory.<sup>2</sup>

We shall need to return to this notion of *antisience* a little later to examine the extent to which it can be reconciled with our conception of ideology.

In fact, our question really arises in the *practice* of the history of the sciences. For up to the present day, few historians of science have applied themselves to this question, and, amongst those who have, we cannot help being struck by the astonishing absence of criteria. Few historians of mathematics allow for a study of the magical and mystical properties of numbers in their work. If historians of astronomy continue to devote some space to astrology - in spite of the fact that Copernicus overturned the chimerical foundations of the horoscope in 1543<sup>3</sup> - this is only because positional astronomy is indebted to astrology for several centuries of observations. But a good number of historians of chemistry take account of the history of alchemy and incorporate it into the succession of 'stages' in chemical thought. Historians of the social sciences (of psychology for example) seem more embarrassed about the problem. Two-thirds of Brett's history of psychology<sup>4</sup> is given over to a critique of theories of the soul, mind and consciousness, most of which predate the appearance of the very term 'psychology' and *a fortiori* the formulation of the modern idea of psychology.

## II

Is it relevant to speak of a scientific ideology? Can the term adequately designate and delimit all the discursive formations that claim to be theoretical, the more or less coherent representations of relations between phenomena, the relatively stable parameters of lived experience? In short, can it delineate those pseudo-knowledges whose unreality derives solely from the fact that a science is essentially established through a critique of them?

The present-day standing of the notion of *ideology* has an ancestry beyond question. It derives from the vulgarisation of the thought of Marx. Ideology is an epistemological concept with a polemical function, applied to those systems of representations which are expressed in the language of politics, morality, religion or metaphysics. These languages assume the task of expressing the truth about things, whereas they are in fact the means to protect and defend a situation, that is, a system of relationships between men and things. Marx denounces ideologies in the name of the science he claims to inaugurate: the science of men who make their own history (but not under conditions of their own choosing).

It has been asked how the term ideology, borrowed from 18th-century French philosophy, was charged by Marx with the significance it bears today. According to Cabanis and Destutt de Tracy, ideology was the science of the genesis of ideas.<sup>5</sup> Its aim was to treat ideas as natural phenomena expressing the relation of man - conceived as a living, sensual organism - to his natural environment. Despite their premature positivism, the ideologists were anti-theological and

anti-metaphysical liberals. As such, they were at first deceived by Bonaparte's political actions and thought him the rightful executor of the French Revolution. But once these Bonapartists turned anti-Napoleonic, they were scorned and persecuted by Napoleon I,<sup>6</sup> and it was he who reversed the image they had wanted to project. Ideology was denounced as a metaphysics and as a branch of sterile speculation in the name of a political realism which formulates legislation on the basis of a knowledge of human feelings and the lessons of history.

So we can see that, in the sense he gave to the term ideology, Marx preserved the idea of an inversion of the relation between knowledge and things. Ideology, which began by designating a natural science of man's acquisition of ideas modelled on reality, designated henceforth any system of ideas produced as the effect of a situation condemned from the outset to misunderstand its relation to the real. Ideology consists of the displacement of the focus of a study.

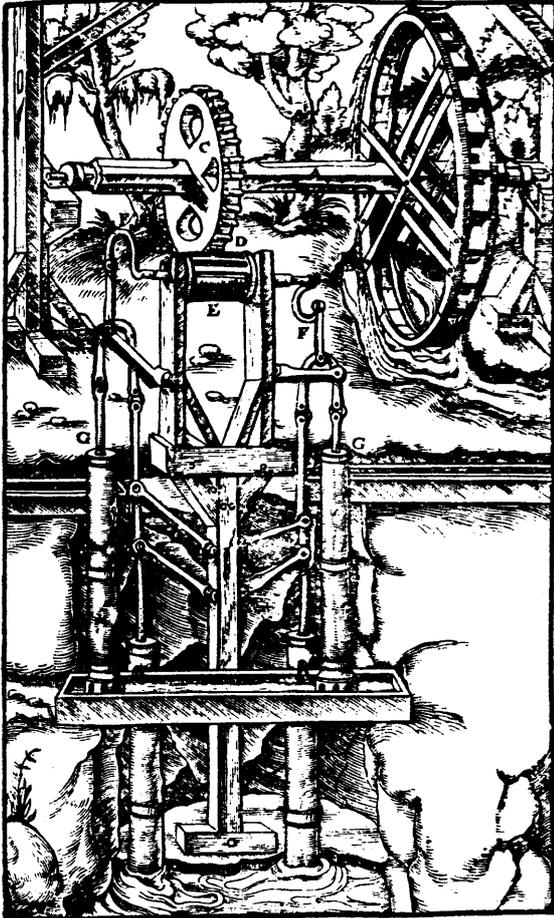
But can the notion of a scientific ideology be included within the general Marxist notions of ideology without distortion? Apparently not. In *The German Ideology*, Marx categorically counterposes political, economic, legal and religious ideologies to the science of economics, that is, to the science he intends to constitute. Science authenticates itself by tearing aside the veil that is ideology's whole and only reality. This seems to turn scientific ideology into a logical monstrosity. By definition, all ideology is a movement, in the double sense of distance and decentering: distance from reality and decentering relative to the point of investigation from which it imagines itself to set off. Marx is determined to show that, confronted by the Marxist science of economics, all politico-economic ideologies appear as products of a class situation which prevents bourgeois intellectuals from seeing, in what they believe to be a mirror, that is to say a science indicating real things, anything other than the inverted image of the relationship of man to man and of man to nature. None of these ideologies state the truth, even though some are less distant from it than others. All are illusory. And by illusion, we must no doubt understand a mistake, a misapprehension, but also a reassuring fabrication: an unconscious willingness to accept judgements governed by interest.<sup>8</sup> In short, Marx seems to me to have assigned a compensating function to ideology. Bourgeois ideologies are reactions symptomatic of the existence of conflicting social situations, that is, of class struggles, and at the same time they deny in theory the concrete problem whose existence brings these struggles about.

It might be thought strange that in *The German Ideology* Marx does not number science amongst the ideologies. This is indeed remarkable. Certainly, in criticising Feuerbach, Marx accuses him of failing to understand that the so-called 'pure' science of nature receives its means and its objectives from industry and commerce; that is, from man's material activity. But does this authorise no difference of epistemological status between that type of ideological discourse Marx terms liberal economics and sanctioned discourses such as electro-magnetism and celestial mechanics? Certainly, it is true that the development of astronomy during the 18th and 19th centuries was dependant upon the manufacture of optical and chronometrical instruments. The determination of longitude at sea during the 18th century was a theoretical problem which made an appeal to the technologies of clock-making for commercial ends.

And yet, by virtue of the convergence of efforts supported by technologies and economies quite unlike it in terms of their associated ideologies, is not Newtonian celestial mechanics today receiving immense

experimental verification in the technologies of artificial satellite research and astronautics? To say that the science of nature is not independent of successive modes of the exploitation of nature and of the production of wealth is not to deny the autonomy of its problematic and the specificity of its method. Nor is it to render that science relative, in the sense that economics and politics are, to the ruling ideology of the ruling class at any given moment in social relations. In the *Contribution to the Critique of Political Economy*, Marx met what he called a 'difficulty', namely, the fact that art, which is related by its productions to the social world, could retain a permanent value outside its historical conditions and after these had disappeared. Can Marxists deny Greek geometry what Marx allowed for Greek art?

But, for all that, are we forbidden to give any meaning to the concept of scientific ideology because we cannot number scientific knowledges amongst ideologies? We must distinguish form and content in the realm of ideology. Marx explicitly declares that ideologies will come to an end once the class that assumes through necessity the task of ending all class relations actually fulfills its dialectical obligations. Ideological, political, religious and moral illusions will then, quite literally, have served their turn. But history will nonetheless continue. Or, to be more precise, it will begin. This is a history concerned with certain relations to nature. So we need to ask whether some new relations to nature can be established in advance by virtue of the clarity and foresight of science in its historical development. On the other hand, for men to relate confidently to nature in new ways, can we not maintain the view that the production of scientific knowledges will demand, in the future as in the past, a priority for intellectual adventure over rationalisation, a certain presumptuousness in going beyond what should already be known with prudence and cau-



tion? Were this the case, scientific ideology would be both the obstacle to and sometimes also the condition of possibility for the formation of science. In this event the history of the sciences should include a history of scientific ideologies, recognised as such. What would be the advantage of elaborating the epistemological status of this concept? Let us see.

### III

In contrast to political class ideologies, a scientific ideology is not false consciousness. Neither is it false science. The characteristic of a false science is that it never has to face falsity; it does not need to renounce anything and never has to change its terminology. For a false science, pre-scientific states do not exist. Its discourse cannot accept contradiction. In short, false science has no history. Scientific ideology, as we shall come to see when we examine the case of atomism, does have a history. It comes to an end when the position it occupied in the annals of learning is supplanted by a discipline which can demonstrate operationally the validity of its own norms of scientificity. When this occurs, a certain domain of non-science is determined by exclusion. We are using the term non-science here, rather than M. Suchodolski's 'antiscience', simply to allow for the fact that within scientific ideology there exists the explicit intention of being a science, of imitating some model of an already existing science. This seems to me essential. The existence of scientific ideologies implies that parallel scientific discourses already exist and, in consequence, that the separation of science and religion has already been enacted. In the case of atomism, for example, Democritus, Epicurus and Lucretius all claimed the status of science for their physics and psychology. In opposition to antiscience, that is, religion, they proposed antireligion; that is, their science. Scientific ideology is obviously the misunderstanding of the methodological requirements and of the operational possibilities of science in that area of experience in which it seeks to lay its hold - but it is not a scorning, ignorance or denial of the function of science. Thus, ideology must certainly not be confused with superstition since it occupies a place - albeit by usurpation - in the realm of knowledge and not in that of religious belief. Besides which, we cannot call scientific ideology superstition if we adhere to the latter's strict etymological sense. Superstition is the maintenance of a representation from ancient religion which has not been eliminated by the interdicts of the new religion. Scientific ideology is really over-positioned in relation to the site science will come to occupy. But in addition scientific ideology is displaced. When a science comes to occupy the site that ideology seemed to indicate for it, it does not do so in the anticipated position. When 19th-century chemistry and physics constituted scientific knowledge of the atom, the latter did not appear in the space assigned to it by atomist ideology, that is to say, in the space of the indivisible. What science discovers is not what ideology led us to look for. When the context of orientations and methods is so different that the technique of crushing matter and the theory of the convergence of measurements can be distinguished, the persistence of words has no bearing on the issue. Indeed, what ideology announced as simple finds a scientific reality in a coherent mass of complications.

I hope to be able to find in the Mendelian theory of heredity another example of the process whereby an ideology is dethroned. Historians of biology seldom

fail to look to Maupertuis in their search for precursors of genetics. In his *Vénus physique* Maupertuis was concerned with the mechanism of the transmission of normal and anomalous morphological traits: he invoked the calculation of probabilities to decide if the frequencies of similar anomalies were, or were not, fortuitous, and he explained the phenomena of hybridization by supposing the existence of seminal atoms - that is, of hereditary elements combined at the moment of conception.<sup>9</sup> Yet merely putting the texts of Mendel and Maupertuis side by side is enough to establish clearly the difference between a science and the ideology it expels. Mendel does not study the facts caught by a phenomenology of first impressions, but those determined by research. And this research, in turn, is determined by a problem, a problem without precedent in pre-Mendelian literature. Mendel invented the concept of *character* to signify the element of what is transmitted hereditarily and not the elementary agent of that transmission. This Mendelian character could combine with *n* other characters, the frequency of its reappearance in different generations being measurable. Mendel had no interest in structure, fecundation, or development. As far as he was concerned, hybridization was not a means to establish the constancy or inconstancy of any global type, but the means to break it up - it was an instrument of analysis, and, given access to a large sample of cases, a tool to disassociate characters. He is only interested in hybrids in order to break with the hundred-year-old tradition of interest in hybridization. He has no interest in sexuality, nor in the debate over the innate and the acquired, nor even in that over preformation and epigenesis. Mendel's sole interest is in verifying, mainly through calculations of combinations, the consequences of his hypothesis.<sup>10</sup> Whatever Mendel neglects is, conversely, whatever interested those who were not in reality his predecessors. The 18th-century ideology of hereditary transmission was desperate for observations, for accounts of the production of animal or vegetable hybrids, and for the appearance of monstrosities. This avid curiosity had several purposes: to decide between preformation and epigenesis, between ovism and animalculism, and thence to resolve juridical problems concerning the subordination of the sexes, paternity, the purity of genealogical lines and the legitimacy of aristocracy. These preoccupations overlap the problems of the inheritance of psycho-physiological traits, that is, the debate between innatism and sensualism. The technique of hybridization found as much support in the agronomist's interest in discovering improved varieties as it did in the botanist's interest in determining relations between species. Maupertuis's *Vénus physique* cannot be uprooted from its historical context and superimposed on the *Versuche über Pflanzenhybriden* to effect a partial correspondence. Mendelian science was not situated within the parameters of the ideology it supplanted for the simple reason that this ideology had many different parameters, and none of these were ever set out by those who worked within them. They were inherited from traditions of various ages. Ovism and animalculism do not arise from the same epoch as the empirical or mythological arguments in favour of aristocracy. For the science of heredity, the ideology of heredity (here the term ascends from science to ideology; in the case of atomism it descended from ideology to science) is an exorbitant aim, a naive desire to resolve a whole range of problems of theoretical and practico-juridical importance without criticising their foundations. Here ideology disappears either by reduction, or by being cut down to size. But it is in vanishing as a badly founded science that it

appears as ideology. A collection of observations and deductions is qualified as ideology once it has been disqualified as science by a discourse which delimits its own area of validity and which proves itself by the coherence and integration of its own results.



If, in assigning a status to scientific ideologies, it is instructive to study how they disappear, it is, I believe, even more instructive to examine how they come into being. I propose to examine briefly the genesis of a 19th-century scientific ideology: evolutionism. The work of Herbert Spencer offers us an interesting case for analysis. Spencer believes that he can formulate a mechanical law of universal progress via the evolution from simple to complex across successive differentiations. The movement from more to less homogeneity and from less to more individuality is the universal law of the formation of the solar system, the animal organism, living species, man, human societies, the products of man's activity and thought and, first and foremost, the universal law of the formation of language. Spencer explicitly declares that his law of evolution was obtained by generalising the embryological principles of Karl-Ernst von Baer (*Über Entwicklungsgeschichte der Tiere*, 1828).<sup>11</sup> The publication of *The Origin of Species* (1859) confirmed Spencer's own conviction that his system of general evolution was ranged at the same level of scientific validity as Darwinian biology. But to bring the guarantees of a more apodeictic science than the new biology to his law of evolution, Spencer flatters himself that he has deduced the phenomenon of evolution from the law of the conservation of energy by referring to the instability of the homogeneous. To anyone who follows Spencer's thought through the progressive elaboration of his work, it appears that first von Baer's biology, and then Darwin's, seem to furnish him with a scientific guarantee, something like an engineer's project in 19th-century English industrial society: to legitimize free enterprise, the corresponding political individualism, and competition. The law of differentiation ends in the support given to the individual pitted against the state. But, if this is where it explicitly ends, perhaps this is because this is also where it implicitly began.

The extension of mechanics, or epigenetist embryology, and of transformist biology beyond their authorised field, cannot be justified by any of those sciences. When theoretical conclusions are detached from their own premisses, freed from their context and extended to the totality of human and, more particularly, social experience, we need to ask what the purpose of such a transfusion of scientificity can be. The purpose is practical. Evolutionist ideology functions as the self-justification of the interests of a particular type of society - industrial society - when it is in conflict with traditional society on the one hand, and the demands of workers on the other. It is both anti-theological and anti-socialist. Here we return to the Marxist concept of ideology as a representation of natural or social reality whose truth lies not in what it says, but in what it silences. Of course, 19th-century evolutionism cannot be reduced to Spencerian ideology. But this ideology did have a more or less permanent effect in colouring linguistic and ethnological researches, in giving a lasting sense to the concept of the primitive and in easing the conscience of colonising races. We can still see active traces of it in the behaviour of advanced societies towards societies deemed to be 'on the road to development'. And this even after cultural ethnology has recognised the plurality of cultures and has apparently proscribed any society setting itself up as a norm of assessment and a yardstick of the levels of attainment of others. In disposing of their evolutionist origins, contemporary ethnology, linguistics and sociology have brought a sort of proof of the fact that an ideology disappears when the conditions that make it a historical possibility have also disappeared. The scientific theory of evolution has not remained exactly what Darwinism was; for Darwinism is only a moment integral to the history of the constitution of the science of evolution. By contrast, evolutionist ideology has remained a sterile residue within the history of 19th-century social science.

#### IV

By analysing a few examples, I hope that I have delimited the area in which scientific ideologies appear as well as the manner in which they are constituted. In characterising them, I have to insist again that they must not be confused with the *ideologies of scientists*, that is, the ideologies scientists themselves generate in the discourses they use to situate science within culture and in relation to other cultural forms. The ideologies of scientists are philosophical ideologies. Scientific ideologies would more properly be the ideologies of philosophers - discourses aiming to be scientific which are upheld by those that are still, in the given field, only presumptive, or presumptuous scientists. During the 18th century, the concepts of Nature and Experience were part of the ideology of scientists, whereas the concepts of 'organic molecule' (Buffon) and 'chain of being' (Bonnet) were concepts of a scientific ideology within natural history.<sup>12</sup>

I would, therefore, propose the following conclusions:

(a) Scientific ideologies are explanatory systems whose object is exaggerated in comparison to the borrowed scientific norm that is applied to it.

(b) A scientific ideology always pre-exists a science, and does so in the area that the science will come to occupy. There is always a science prior to an ideology, but in an area to one side which the ideology cross-cuts at an angle.

(c) Scientific ideology must not be confused with

false sciences, with magic, or religion. Like them, scientific ideology is indeed driven by an unconscious need for access to totality, but it differs in being a belief which languishes alongside (*louche*) an already instituted science whose prestige it recognises and whose style it attempts to emulate.

This being so, I must finish where I began, and propose a theory of the history of the sciences which would throw light on its practice.

A history of the sciences which treats a science in its historical development as an articulated succession of *factual truths* has no need to occupy itself with ideologies. So, it is appropriate that historians of this school leave ideology to historians of ideas or, what is worse, to philosophers.

A history of the sciences which treats a science in its historical development as an elaborated purification of *norms of verification* cannot concern itself with scientific ideologies. What Bachelard distinguished as the history of lapsed sciences and the history of ratified sciences needs to be separated and, at the same time, interwoven.<sup>13</sup> The ratification of truth or objectivity also entails a condemnation of the lapsed. But if what will at a later stage come to lapse does not first of all present itself for ratification, then verification has no grounds for making truth appear.

So the separation of science and ideology should prevent us placing some elements of an apparently preserved ideology and the scientific construction which unseated it in a continuous sequence within a history of the sciences. It should prevent us looking in the *Rêve d'Alembert* for a foretaste of *The Origin of Species*.<sup>14</sup>

But the interweaving of science and ideology should also prevent us from reducing the history of the sciences to the platitudes of a chronology, that is, to a kind of featureless painting with neither shadow nor relief.

The historian of the sciences needs to work, and present his work, on two levels. If this is not done, if the specificity of scientific ideology is neither recognised nor given a place - a place on a level distinct from the various levels of scientificity - then the history of the sciences risks being no more than an ideology. An ideology in the sense, this time, of a false consciousness of its object: a knowledge as far from its given object as it thinks itself bound to it. Here ideology would be the misunderstanding of the fact that any knowledge with a critical grasp of its project and its problem knows from the start that it is at some distance away from its operationally constituted object.

In attempting to write only a history of truth, we make an illusory history. M. Suchodolski is quite right on this score: the history of the truth alone is a contradictory notion.

#### Translator's Note

This is a translation of 'Qu'est-ce qu'une idéologie scientifique?' which appears in Georges Canguilhem's *Idéologie et rationalité dans l'histoire des sciences de la vie*, Paris, 1977, pp.33-45. Though Canguilhem's prose is for the most part clear and consistent, I have broken up some of the longer sentences and amalgamated others to render it into unambiguous English. I should like to thank Clare Fischer for her help.

## Footnotes

- 1 Cf. the article 'Préjugés' in Voltaire's *Dictionnaire philosophique*.
- 2 *XIIe Congrès International d'Histoire des Sciences, Colloques, Textes des Rapports, Actes, Tome IA*, Paris, 1970, p.34.
- 3 T.S. Kuhn, *The Copernican Revolution*, Cambridge, Mass., 1957; Angus Armitage, *The World of Copernicus*, Wakefield, 1972; Alexandre Koyré, *From the Closed World to the Infinite Universe*, Baltimore, 1957, and *The Astronomical Revolution*, London, 1964 (trans.).
- 4 George Sidney Brett, *A History of Psychology* (3 vols.), London, 1912 (trans.).
- 5 The word *ideology* was first used by the French philosopher Destutt de Tracy (1755-1836) in his *Eléments d'idéologie* (4 vols.), Paris, 1801-15. See Raymond Williams, *Keywords*, London, 1976, under 'Ideology' (trans.).
- 6 'His [Napoleon's] scorn of industrial *hommes d'affaires* was the complement to his scorn of *ideologists*.' K. Marx, 'The Holy Family', in Karl Marx and Frederick Engels, *Collected Works*, Vol.4, London, 1975, p.123.
- 7 According to Marx, the political ideologies of the French and English during the 18th century were less far from their real foundations than the religious ideology of the Germans.
- 8 In the *Communist Manifesto* the illusion which consists, for the bourgeoisie, in believing those social relations in which it is dominant to be eternal is qualified by the notion of an 'interest-governed conception'.
- 9 See, for example, Bentley Glass, 'Maupertuis, Pioneer of Genetics and Evolution', in *Forerunners of Darwin 1745-1859*, B. Glass, O. Temkin, W.L.

- Straus (eds.), Baltimore, 1959. A discussion of Maupertuis' *Venus physique* more in line with Canguilhem's can be found in Jacques Roger, *Les Sciences de la vie dans la pensée française du XVIIIe siècle*, Paris, 1971 (2nd edition), pp.468-487 and *passim* (trans.).
- 10 Cf. Jacques Piquemal, *Aspects de la pensée de Mendel*, Paris, 1965.
  - 11 Von Baer (1792-1876) was the greatest of the early 19th-century comparative embryologists and the most able proponent of epigenetic thinking. The best survey of his work remains untranslated: Boris Raikov, *Karl Ernst von Baer 1792-1876. Sein Leben und sein Werk*, Leipzig, 1968, but Jane M. Oppenheimer, *Essays in the History of Embryology and Biology*, Cambridge, Massachusetts, 1967, is reliable. J. Arthur Thomson still offers the fullest account of Spencer's biological views: *Herbert Spencer*, London, 1906. Also useful is J.W. Burrow, *Evolution and Society. A Study in Victorian Social Theory*, Cambridge, 1966 (trans.).
  - 12 On Buffon (1707-88) see Jacques Roger, *op.cit.*, pp.527-84; R. Wohl, 'Buffon and his Project for a New Science', *ISIS*, 51, 1960, and P.L. Farber, 'Buffon and the Concept of Species', *Journal for the History of Biology*, 5, 1972. For Bonnet (1720-93) see B. Glass et al., *op.cit.*, p.164ff, and Arthur O. Lovejoy, *The Great Chain of Being*, Cambridge, Massachusetts, 1936, pp.283-7 and *passim* (trans.).
  - 13 See G. Bachelard, *L'Activité rationaliste de la physique contemporaine*, Paris, 1951, pp.35-40 and 'L'Actualité de l'histoire des sciences' in *L'Engagement rationaliste*, Paris, 1972, pp.137-52 (trans.).
  - 14 For an example of this approach, see Lester G. Crocker, 'Diderot and Eighteenth Century French Transformism', in B. Glass et al., *op.cit.*, pp.114-44 (trans.).

# REVIEWS

## Women and Political Thought

Susan Okin, *Women in Western Political Thought*, Virago, 1980, £4.50

One of the central tasks of classical political theory has been to consider what kinds of social organization are necessary, or possible, in the light of human nature. A particular conception of human nature (e.g. that humans are naturally aggressive) carries with it a host of implications about the kinds of institutions which are required to regulate social life. This book explores the way in which four political philosophers - Plato, Aristotle, Rousseau and Mill - have characterised the nature of both men and women. It also shows how some of their ideas live on in the works of modern thinkers (Talcott Parsons and Erikson) and in sexually discriminatory US court decisions.

Okin's central thesis is that, to arrive at a conception of human nature, two quite different kinds of questions have been asked, depending on whether men or women are at issue. To establish the nature of the male, the question 'What are men like?' has been posed. Typically, men are seen as having a more or less limitless potential, as individualistic, assertive, rational and creative. To establish the nature of the female, however, the question asked has

been 'What are women for?'. Women's nature has been defined (by men) in terms of her perceived function, in particular, her function as child-bearer and child-rearer. The social institution in which these functions are to be enacted is the family. It is as mother and wife that woman fulfils her essence, caring for her children and her husband, to whose authority she is subject.

Both of these conceptions are prescriptive, though in somewhat contradictory ways. Rousseau, for example, condemned slavery as degrading to man's essence. It was, he said, both offensive to nature and to reason 'for a man to give up his life, freedom, and right to himself, to another' (quoted p.143). Here a conception of man's essence is used to condemn an oppressive social institution. When it comes to women, however, the situation is reversed. The married woman is *expected* to renounce her freedom. She is subject to her husband's will, even to the extent that 'if he blames her, she is blameworthy; and if she has acted innocently, she is guilty as soon as she is suspected; for even preserving appearances is part of her duty' (quoted p.165). Rousseau is not (generally speaking) critical of this situation. He is critical of women who refuse to accept their 'natural' role and to act in accordance with it. Loosely, it is wrong for a man, but right for a woman,