

Newton at the Crossroads

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'The label on a system of ideas is distinguished from that on other articles, amongst other things, by the fact that it deceives not only the buyer, but often the seller as well.'
(Marx, Capital, Volume II)

Boris Hessen and his audience

In this essay I attempt a re-evaluation of a remarkable text published in London in 1931: Boris Hessen's 'The social and economic roots of Newton's Principia' <1>. Hessen was a member of the high-powered Soviet delegation, headed by Nikolai Bukharin, which attended the International Congress of the History of Science and Technology held at the Science Museum in July 1931. Both the genesis and the reception of Hessen's paper, and the volume in which it was published, Science at the cross-roads, have been analysed elsewhere, notably by Gary Werskey <2>. Here I connect Hessen's arguments with the contexts of left critiques of science and of developed Newtonian scholarship in order to illuminate some important themes in the practice of history of science. Specifically, I concentrate on two central themes which Hessen raised in his own paper and which have played a major role in historians' understanding of scientific work since then: the celebration of the heroic genius in science, and the relation between scientific work and its social context.

Left scientists who responded to Hessen's essay report the considerable impact which it had upon them in the 1930s, transforming contemporary radical understanding of the interpretation of science and its history. Furthermore, even those writers who displayed little overt sympathy for Hessen's political intervention nevertheless seem to have been deeply affected by the direction of the programme he proposed: Clark's Science and social welfare in the age of Newton (1937) and Merton's Science, technology and society in seventeenth century England (1938) both emerged as detailed responses to Hessen's arguments. Clark was a speaker at the 1931 congress, and was attacked directly by members of the Soviet delegation for his views. Merton's essay used many of Hessen's arguments to display the socio-economic connections of seventeenth-century science. At the same time, Merton was encouraged to pursue this project by his patron, the Russian sociologist Pitirim Sorokin. Sorokin had worked in the Soviet Union in the 1920s, composing a favourable review of Bukharin's Historical Materialism (1922), on which much of Hessen's work was based. The works of Clark and of Merton may stand as representative of the patterns which orthodox history of science in Britain and America was to develop after 1945 <3>.

Some current historians, however, argue against any attempt to evaluate the status of science. They argue for a 'naturalism' which 'closes no evaluative or political options; it merely ejects them from historical practice'. They also contrast this naturalism with the 1930s context, when 'both

sides of the great debates ... recognised that sustaining their discourse were opposed methods of evaluating science and opposed policies towards it' <4>. Hessen and his audience were very clear on this point. Hessen displayed Newton's greatest achievement as a response to the technical needs of the bourgeoisie, and as conditioned by the ideological conflicts of the revolutions of the mid-seventeenth century. He went on to couple this analysis with enthusiastic advocacy of Soviet science policy, and of the promise offered by socialism for scientific development: 'only in socialist society will science become the genuine possession of all mankind'. Hessen's critics, such as the members of Michael Polanyi's Society for Freedom in Science, established during the Cold War, were equally convinced that 'the movement against pure science and against freedom in science was first brought to Great Britain by the Soviet delegation in 1931' <5>. Any assessment of Hessen's work, therefore, must also confront the claims of contemporary historiography, which now disavows any such concern for purity, freedom, or socialism.

Such a confrontation is ironic. Hessen's paper was seen as an argument for the supreme political importance of research in history of science. History of science has developed by denying any such political significance. Yet many of Hessen's claims turn out to be just those which 'naturalistic' historians also make now. Notably, Hessen criticised the use of genius and free enquiry as explanatory principles in the history of science. He also pointed out the way in which conflicts of ideological interest affect the form in which cosmologies are expressed. These are now familiar features of historiography: Hessen differs from our contemporaries, however, in giving such features a profoundly evaluative role in his own analysis.

Hessen's audience was aware of such concerns: he 'showed that a knowledge of the history of science was not only of entertaining antiquarian interest, but was essential for the solution of contemporary social problems, due to the unorganised growth of a technological society' <6>. Desmond Bernal, leading communist scientist, wrote in 1939 that 'we did not understand all they said, in fact, I now suspect they did not understand it entirely themselves, but we did recognise that there was something new and with immense possibilities of thought' <7>. The embryologist and historian Joseph Needham was amongst those influenced by Hessen. In 1938 he collaborated with Walter Pagel in the establishment of a history of science committee at Cambridge University. Needham and Pagel, following Hessen, argued that 'historians of science have tended too much to fall into mere antiquarianism'. History of science could only be reformed if 'it will but accept the support

now willingly offered by historians and scientists alike, and go forward to present the history of scientific thought always in relation to the social and economic background of the time' <8>. This programme linked fully contextual, and potentially materialist history of science with an explicit vision of the social function and policy implications of that study. This reading of Hessen's paper was profoundly influenced by the scientists' perceptions of the crisis of the 1930s and its causes. In that sense, evaluative history was a crucial resource in the political work of the period.

As I have indicated, two insights of importance for work in history of science are articulated in Hessen's analysis. Significantly, neither of these insights is recognised in the criticisms to which Hessen was subjected after 1931. As an analyst of historical change, Hessen did not appeal to the heroic biography of his subject, nor did he offer the so-called 'vulgar' Marxist account of the relation between economic base and scientific work which is so often ascribed to such arguments. Bernal pointed this out in 1953: the 'picture that the ideas arose entirely out of the operations of economic law is nowhere seriously maintained, though it still serves the professional anti-Marxists as a convenient Aunt Sally' <9>. The first sections of Hessen's paper chart the technical demands of the emergent capitalist mode of production in mining, navigation, warfare and trade, and correlate these demands with the topics treated in the *Principia* in mechanics and astronomy. Here Hessen draws on arguments of Engels and of Plekhanov: for example, in a letter to Starkenburg of 1894 Engels insisted that

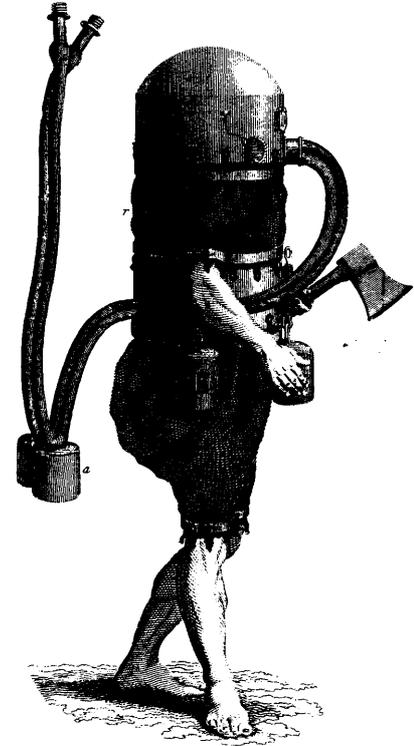
'science depends far more on the state and the requirements of technique... If society has a technical need, that helps science forward more than ten universities. The whole of hydrostatics (Torricelli etc.) was called forth by the necessity for regulating mountain streams of Italy in the 16th and 17th centuries.... But unfortunately it has become the custom ... to write the history of the sciences as if they had fallen from the skies.'

This letter is also cited by Merton in his own analysis of the relation between technique and science <10>. For Hessen, it provided a fundamental insight into the conditions of Newton's work. Nevertheless, he repeatedly denies that this analysis constitutes a complete account of that work and its genesis. 'It would be incorrect to limit the analysis of the contents of the *Principia* to determining its intrinsic connection with the economics and technology of the epoch which served the needs of the rising bourgeoisie'. Hessen claimed that 'it would be too greatly simplifying and even vulgarizing our object' to treat 'the economic factor' as 'the sole determining factor'. Hessen catalogued 'the various superstructures' which, he held, determined the content and form of Newton's utterances: 'political forms of class war, and the results of the reflection of these wars on the minds of the participants; political, juridical philosophic theories, religious beliefs and the subsequent development into dogmatic systems' <11>.

Hessen picked out two aspects of the form of Newton's *Principia* which acted as obstacles to a materialist analysis of its production. Firstly, Newton as author of the *Principia* wrote as a natural philosopher. Hessen recognised the constraints of this role. Newton's text inevitably concealed its own "'low" sources of inspiration'. A specific reading of the *Principia* would be determined by stipulations of authorial intention and of audience. Hessen drew an analogy here with Newton's own claim that the geometrical form of the *Principia* concealed the genuine analytic method by which the discoveries were made <12>. In fact Newton's claim is not substantiated: we must see this picture of a 'hidden' analytic core as a significant move in the priority dispute with Leibniz over the invention of calculus. Newton wrote in 1716-1718 that 'I was writing for scientists (ad Philosophos) steeped in geometry and putting down geometrically demonstrated bases for natural philosophy', while noting that 'the analytical method through

which we found these propositions shines out everywhere'. In the same way, Hessen argued, the 'earthy core' of Newton's programme was necessarily invisible in a text which set up a demarcation round the 'high' practice of natural philosophy. 'We should seek in vain for an exposition by Newton himself of the connection between the problems which he sets and solves, with the technical demands out of which they arose'. Thus, to locate the social and technical interests at work in the *Principia*, it was necessary to analyse the complex of social practices in which Newton was situated, rather than offer a simplistic exposition of the overt text itself <13>.

Secondly, the *Principia* was constructed as a coherent and potentially universal system, in which an 'encyclopaedic survey' integrated isolated technical problems into what appeared to be a manifesto for a new science. Hessen argued for the specific ideological function of such an image: the Newtonian project was 'equivalent to the creation of a harmonious structure of theoretical mechanics which would supply general methods of resolving the tasks of the mechanics of earth and sky' <14>. The perception of an apparently unrelated set of problems as components of a single dominant scientific programme handed control over those problems to the practitioners of that programme. Once again, therefore, the local connections between tech-



nique and theory were likely to be obscured. Simultaneously, those connections could only be displayed by concentrating on the political and economic structures within which seventeenth century natural philosophy worked. In both these cases, therefore, apparent obstacles to Hessen's materialist analysis were to be transformed into excellent instances of social determination of science.

The other major insight in Hessen's account was his critique of the appeal to heroic genius as the source of scientific change. Joseph Needham has pointed out that this critique made a considerable impact on the audience in 1931: the choice of Newton as target 'was a great inspiration to the younger socially conscious British scientists of the time', but 'to the older British historians of science it seemed almost like sacrilege or at least lèse-majesté'. In an essay of 1935, Needham insisted that 'the history of science is not a mere succession of inexplicable geniuses, direct Promethean ambassadors to man from heaven'. He hoped that 'further historical research will enable us to do for the great embryologists what has been well done by

Hessen for Isaac Newton' <15>. As in his account of the function of technical needs in scientific development, Hessen drew on Plekhanov's work, notably The role of the individual in history. Plekhanov had argued that 'a great man is great not because his personal qualities give individual features to great historical events, but because he possesses qualities which make him most capable of serving the great social needs of his time'. Plekhanov pointed to the solution of scientific problems as an instance of the primacy of social needs. In the same way, Hessen insisted that 'the historical development of productive forces and production relationships' rather than 'the kindness of divine providence' should provide the basis of an assessment of Newton's achievement <16>. Hessen rejected any account in which scientific work was portrayed as an autonomous theoretical enterprise. He also rejected the claims of historians such as G.N. Clark, for whom the only important social constraint on science was the establishment of such autonomous institutions. In principle, these institutions were identified with the universities. In 1931, Clark had argued for the integration of history of scientific ideas into historical study. Soviet delegates at the congress attacked this view, since it would lead to a reinforcement of the heroic mode of historiography.

In 1937, Clark amplified this view in his essay on Newton as a direct response to Hessen. Clark made some concessions to Hessen's argument: he agreed that it might be possible to show Newton, Locke and Boyle as 'bourgeois' thinkers; he acknowledged that experimental technique was drawn from mechanical crafts and that scientific style might be derived from the exigencies of merchant accounting. But Clark's main concern was the celebration of the universities as an isolated environment for free theoretical endeavour. 'The pursuit of knowledge in universities is a self-perpetuating tradition,' Clark insisted. 'It was the social function of the universities to set free from the pressure of other motives men who had the desire to know.' Just as 'Newton's mind was in close communication with many minds, of various classes and countries and centuries', so 'the universities had their own laws of growth. At their heart was the disinterested love of truth' <17>. It was this form of history which Hessen explicitly rejected. Echoing Engels, Hessen satirised the universities as bastions of obscurantism: such institutions 'struggled against the new science with a strength equal to that exerted by the dying feudal relationships against the new progressive methods of production'. Thus, the critique of pure idealism was intimately connected with a critique of typical institutions of idealist theory <18>. In fact, Clark's arguments came to typify much of post-war historiography. Insofar as he was prepared to countenance Hessen's 'provocative essay', Merton was soon branded with the label of 'externalism'. In his influential essay against Merton, A.R. Hall wrote in 1963 that Merton had used Hessen as 'his most uncompromising example of the "externalist" historiography', and that historians now realised that 'social forms do not dominate mind; rather, in the long run, mind determines social forms' <19>.

The response to Hessen amongst historians of science was dominated by the political conjuncture in which that paper appeared, and the perceptions of foul intent which historians detected behind Hessen's materialism. Exactly because Hessen gave such a high place to history of science in the provision of political policy, idealist historians retreated to an ever-increasing insistence on the insulation of scientific work. Thus in 1981 J.R. Ravetz and P.S. Westfall both commented on the failure of Hessen's intervention to stimulate more than Robert Merton's and Desmond Bernal's efforts in social history of science. For Ravetz, the villainous idealist was Alexandre Koyré, whose anti-Marxism led him to a 'totally superficial interpretation of the Scientific Revolution'. Ravetz wrote that Hessen's work was 'naive and simplistic' but at least it threw 'speculative bridges across the gap between science and society'.

Westfall defended Koyré against Ravetz's charges, and announced that 'much of the modern world appears to me as so many epiphenomena to the growth of science' <20>. Such views have explicit methodological and political implications. The appeal to genius closes down a wide range of historical problems, depriving them of interest. With such an idealism firmly established at the heart of history of science, certain terms come to acquire an important place in historians' explanatory schemes: the discovery and its author. Typically, such history searches for the underlying consistency and unity in a given scientist's thought, assuming axiomatically that the structure of that thought would hold the key to apparently diverse practices, and, at the same time, arguing that texts in the historical archive spoke unambiguously with the authorial voice. Thus, in his magisterial biography of Newton, published in 1980, Westfall found that Newton's genius remained untouched and inexplicable behind all possible historical analysis; 'he has become for me wholly other ... a man not finally reducible to the criteria by which we comprehend our fellow human beings' <21>.

Politically, too, the rejection of and hostility to any such programme as that proposed by Hessen produces an extreme reaction amongst historians of science. The history of science in Britain and America in the period 1950-1970 affirmed the isolation of scientific work from social pressure, giving that pressure at most a negative function. This reinforced a model of pure scientific inquiry, guiltless and progressive, which could be contrasted with the pathologies of planned science (Lysenkoism was often cited here) and with the unfortunate errors of free theory (which would always be attributed to the perversion of research by some extraneous social factor). Merton's own move from the investigation of the social factors at work in scientific development to the investigation of the open and liberal norms of scientific inquiry typified this change. In a recent attempt to defend Merton's original essay from its interpreters, Abraham has stipulated that Merton's intentions always referred to science as a value rather than as practice; that 'the theoretical tradition with which the Merton thesis should be identified' is explicitly opposed to any attempt to incorporate 'grosser or more tractable human needs such as the provision of economic want'. So for his contemporary apologists, Merton's work itself stands as a smoothly consistent attempt to distance sociological analysis of science from materialism, thus making the heroic pattern quite secure. 'Individual contribution' to scientific work for Abraham is emphatically 'an independent force' <22>.

In this way, even the mildly contextual approaches developed in the 1930s can be purged of any danger to the autonomy of science. Merton himself now denies that his essay should be read in a 'simplistic' manner as affirming the total economic determination of science. In the 1970 preface to a re-issue of this essay, Merton explained that 'during the Great Depression vulgar Marxism was just about the only variety of Marxism that was being expounded on the periphery of American academic circles'. He also pointed out 'the reversion to this practice among some American academic youth today' <23>. Much orthodox sociology of science which takes its inspiration from Merton's work since the 1940s has reinforced a boundary between the content of science, which is invulnerable to social analysis, and the extrinsic factors working on science, to which province sociology should be confined. Furthermore, his work of the 1930s is seen as some kind of anticipation of this great division. We are instructed against reading Merton's essay as a suggestion of a more profound sociology, and also against using Hessen's work as anything but an awful warning of the excesses of vulgar Marxism. Thus, mature social history of science now finds itself divorced from the valuable resources which the insights of Hessen presented, and which value-free sociology condemns. As I have indicated, some of Hessen's insights now re-emerge as commonplaces of contemporary social analysis. It is fruitful to con-

sider these commonplaces in their political and practical context. In so doing, it is hoped that the really evaluative structure of some contemporary historiography will become clearer.

The Newton industry and its social problems

Two recent developments in the study of science in history have made Hessen's essay an increasingly important resource. On the one hand, the period since the 1960s has seen the development of a 'Newton Industry': works of massive scholarship have brought considerable quantities of hitherto unavailable material into the public domain. The motives and interests at play in Newton's achievement have been debated in some detail. On the other hand, the same period has seen the emergence of a group of socialist writers concerned with the history of science and its social relations.

Monuments of this industry include editions of the mathematical papers and of the correspondence, a variorum edition of the *Principia*, and a re-assessment of the mass of alchemical and theological manuscripts in the Newtonian archive. Some attempts have also been made to broaden the explanatory base of historical analysis. The more spectacular examples include Frank Manuel's Freudian psychobiography and M.C. Jacob's series of studies which explore the reception of Newtonian cosmology by groups of Anglican churchmen <24>. Occasionally such studies glance at the work of the early 1930s, normally to contrast the new sophistication of the history of science with the alleged crudities of those early texts. Thus in the final volume of his edition of Newton's mathematical papers, D.T. Whiteside castigates a modern historian working in the DDR for the unsupportable claim that 'Newton was primarily a physicist', and notes that 'for all its patent absurdities and the much in it that is badly outdated' there is nevertheless 'a hard core in Boris Hessen's celebrated essay ... which we should at least consider with respect, even though we may not accept its past reality' <25>. This 'hard core' might well refer to the picture of practical technique which Hessen displayed. Here it is precisely the reductionist component of Hessen's which has most appeal.

This appeal is to be contrasted with the approach of social historians such as M.C. Jacob who have been more concerned to distance themselves from the economism which Merton and Hessen are held to have developed. J.R. Jacob and M.C. Jacob argue that 'Merton did not address himself to the connections between Puritanism and scientific theory, but only to those between Puritanism and scientific practice'. Inevitably, those who wish to display Merton's work as itself confined to the level of social values have argued that this reading represents a 'plain misunderstanding' of Merton's intentions. Nevertheless, Jacob has insisted that 'it is only on the level of the former connections between religious ideology and matter theory that we can trace the social genesis of the conceptual revolution that culminated in the Newtonian synthesis' <26>. A paradox: it would now be an 'internalist' strategy to show how Newton's science responded to the technical demands of the 17th century (since allegedly this would confirm the purely instrumental function of theory) and 'externalist' to concentrate on the purely theoretical implications of his cosmology (since this would show the deeply political import of such cosmology). The link which Hessen forged between practice and theory has come unstuck once again. It is not clear how historians who argue for a purely theoretical connection between political and natural philosophical cosmologies might treat the practical work of natural philosophers in context. However, we shall see that the vicissitudes of the Newton industry do suggest a way in which scientific practice might be recaptured for social history. But first we must examine the equally difficult career of current socialist analyses of such problems.

Socialist writers on science and its social relations

have recently returned to Hessen and his contemporaries in order to understand the historical career of materialist analyses of science. Hessen has not emerged unscathed from this process. Indeed, it has now become routine to interpret his essay as a classic of the vulgar Marxism from which contemporaries wish to distance themselves. I have already indicated that this is by no means the only reading available. But the construction of Hessen's argument as the ideal type of reductionist analysis is legitimated by the political contexts in which it was produced and the purposes to which attacks on Hessen are now put. Most importantly, perhaps, Hessen is now read as a precursor of Bernalism. Bernal moved from enthusiasm for Hessen's arguments to the claim that 'in the long run the constructive rather than the destructive use of science is bound to prevail', and that rational planning of scientific development, bolstered by a committed historiography of science, could guarantee this successful future. The tension which exists in Hessen's analysis of Newton, between the heroic solution of technical problems and the obstacles of ideological pressures, also exists in Bernal. This is nowhere better illustrated than in Bernal's account of the work of Pasteur, whom Bernal credits with the foundation of developed crystallography and also with the solution of problems of 'immediate economic interest'. Bernal saw the mixture of motives here as 'one of glory of science and benefit to society' <27>. These are by no means the common concerns of radical writers on science now. So most left interpretations of Hessen's paper have come to involve declarations of rival attitudes to the career of science policy and planning since the 1930s, and, more directly, to Soviet attitudes to science as either purely superstructural, or else as a neutral relation of production.

Two areas of interest emerge from these disputes: first, we are given rival chronologies of the emergence of Soviet attitudes to science, and thus rival versions of the role of the historian of science in successful or disastrous science planning. Secondly, we are given rival accounts of the style of historiography which Hessen typified, and thus differing recommendations for the goal of sophisticated historical work. It is clear, for example, that the date of the 1931 Congress coincided with massive changes in Soviet attitudes to science. The period of the late 1920s, initiating the 'Great Break', had direct consequences for the politics of science. These included increasing political investigation and, ultimately, suspicion of allegedly 'neutral' technical experts and expertise. Bukharin himself, although under severe political pressure from the CPSU Central Committee, became director of research of the Supreme Economic Council and dominated the ideological reconstruction of the Academy of Sciences <28>. As Werskey has pointed out, Bukharin had presided over the only meeting of the Conference on Science Planning in the April before the London meeting, and thus his own concerns with the relation between economic transformation and scientific change were acutely focused at this moment. At the same time, however, very few members of the Soviet delegation were committed supporters of the programme initiated at this point. So the image which British witnesses received of a coherent Soviet Marxist position was itself illusory. This is made very clear both by Marxist critiques of Hessen and Bukharin, and also by the efforts of the British CP to impose a standard version of the import of their 1931 contributions upon communist scientists in Britain <29>.

Such responses to *Science at the cross-roads* and its Soviet background show how any methodological prescriptions for historical work always imply political evaluations of the place of science in society. One of the first - and certainly one of the most considered - responses to this book was that of Gramsci. In his 'Critical Notes' on Bukharin's *Historical Materialism*, Gramsci cites the 1931 volume and argues for a mature history of 'experimental method' and scientific instrumentation which would avoid and also destroy the crudities of any attempt to display science as 'metamorphoses of the technical instrument'.

Gramsci shopped how economism, an 'infantile deviation' of Marxism, was 'generated by the baroque conviction that the more one goes back to "material" objects the more orthodox one must be'. Here Gramsci offered some direct recommendations for history of science: natural objects could only enter such a history when 'socially and historically organised for production'. A contrast was to be drawn between a history of science which reduced all scientific practice to the utilisation of instruments of 'production and work', and a totally different history in which 'the ensemble of social relations' was treated, and in which it could be shown that 'the principal instruments of science are of an intellectual (and even political) and methodological order'. This latter historiography also licensed a different politics of science, in which scientists took their place as mental workers within a comprehensible social totality, and not in the false position of technological administrators. Once again, Gramsci's original response to the arguments of Science at the cross-roads itself has come to be used as a resource for creative re-interpretation. Certainly, his attack on Bukharin and Hessen has profoundly



affected contemporary socialist suspicions of their own approach to the politics of science <30>. Socialist critiques of science now either make pure theory the sole site of political influence, or else rigorously separate the theoretical level from any such influence. In either case, the task of the self-respecting social (or socialist) historian is to concentrate on the theoretical construction of science alone. In the former case, this self-denying ordinance allows the historian to distance analysis from the crudities of economism. In the latter case, it allows the historian to present such analysis as a contribution to the theory of scientific socialism.

It is thought to be bad manners to concentrate in any detail on the social relations of scientific practice, as opposed to the representation of social relations in scientific theory. R.M. Young wrote in 1973 that Hessen was a notorious example of 'those who have attempted to account for findings in so-called pure science by claiming that they are direct, unmediated expressions of economic forces in the period'. Young declared that 'it is, frankly, difficult to recover the enthusiasm generated by Hessen's essay', and went on to express his sympathy for historians of science working during the Cold War who 'turned to the internal history of ideas as practised by Koyré and Meyerson with a sense of relief, excitement and liberation' <31>. A similar sentiment was expressed in 1978 from a totally contrasting perspective by A. Callinicos. Responding to arguments that Hessen's work represented a radical intervention in the history of the sciences, Callinicos wrote that a 'cavalier attitude to the history of science' was inevitable under Hessen's recommendations, since if 'the sciences passively reflect what goes on in the economy, then they have no specific existence of their own, and there is no need to bother with studying their actual history' <32>. It is important to treat such celebrations of 'actual history' with deep suspicion, since such actuality always pretends to a specious intellectual detachment on the part of the historian. The newer insistence that social history of science must explore the details of technical practice however is healthy just because it cannot pretend to any detachment

from the politics of science.

Hessen's political commitments to materialism were explicitly part of his account of Newtonian cosmology and Newtonian natural philosophical work. In a remarkable passage in his essay, Hessen displayed the tension in Newton's work between insistence on mechanistic causation and opposition to atheist materialism. Hessen saw Newton's work on the action of divine power as part of this problematic. In turn, he saw Newton's opposition to materialism in terms of the dual threat of aristocratic deism (identified with the disciplines of Hobbes and Toland) and of radical materialism (identified with the sectaries of the Civil War). Hessen went on to catalogue the instances at which these struggles became manifest in Newton's programme: the Boyle Lectures of the 1690s, the controversy between Clark and Leibniz, Newton's accusation of Hobbesian materialism levelled against Locke, and Newton's analysis of space as the 'sensorium of God' <33>. All this account has now become a rather celebrated part of the Newton industry, but few modern practitioners would recognise Hessen's original formulation of the argument. Instead, the industry has concentrated upon the construction of an acceptable inventory of Newton's true beliefs. As Westfall put it in his pugnacious review of the 'changing world of the Newtonian industry' in 1976, the image of Newton as 'mathematical physicist and solid citizen' is 'exactly what has come unglued during the last fifteen years' <34>.

This process has been accentuated by detailed explorations of theological and alchemical work, by sophisticated philosophical explorations of the metaphysics of space, time and matter, and by closer attention to the deployment of Newton's claims in the context of the 1690s and early 1700s. Nevertheless, the result of this impressive work has not been as dramatic as Westfall seemed to expect. On the contrary, the specific aim of the industry has almost completely saved Newton from decomposition. In particular, both theological and alchemical excesses have been normalised. We are now to accept that the alchemy which Newton pursued for a substantial part of his life can be recuperated as a preamble and resource for rational matter theory, and his Arian heterodoxy was held so privately that its influence was limited to some very close associates and no further. Indeed, the strength of his heresy can be used to portray Newton as completely isolated from all concern for contemporary struggles in church and state. Certainly the public Newton of Westfall's recent biography seems just as much a 'mathematical physicist and solid citizen' as we could desire, and, perhaps, as Hessen might have suspected. The result has been a sense of scepticism of the possibility that 'the depth of the overall vision of Newton's genius' could 'ever be completely fathomed' <35>.

Hessen's essay can be seen as an invitation to historians who wish to abandon just this goal. His work challenged the notion of genius as a functional part of historical concern, and it outlined some paths by which the social construction of science might be explored. We cannot ignore the valuative assumption inherent in this invitation, however. The heroic character of Newton's work did play a vital part at all levels of the Newton industry. It bolstered up an enterprise which sought a single and consistent set of beliefs which could be safely attributed to the great man. It dominated the interpretations historians were prepared to offer of Newtonian texts, and allowed them to argue that only if it could be shown that a particular item contributed to the Newtonian synthesis could that item be given any historical significance. Finally, it reinforced the notion that the key to such a synthesis must lie in some limited set of methodological and metaphysical propositions which generated the revolution in science. In this context it is understandable that some historians might display their own work as free of all evaluative options. They contrast such work with that which assumed the success of Newton's work, and then only used the historical context when it was necessary to explain failures to perceive that allegedly obvious triumph. They might also draw the contrast with a

tradition which used Newton's texts as direct expressions of authorial intention and then attributed varieties of interpretation to 'misunderstandings' of that intention. But the move away from heroic historiography does take up an evaluative position. This move involves a symmetrical analysis of 'true' and 'false' beliefs. It insists on the interested character of all interpretation. This model applies as much to the understanding of experimental practice as it does to written texts. Such practice organises and generates assent within the community and is related to the assent generated to a particular interpretation of some authoritative text. Such work seems peculiarly appropriate in Newton's case, since he was an extraordinarily obsessive analyst of interpretative dispute and the political 'corruption' of belief. Furthermore, there are already considerable resources available for such work. Studies of the replication of Newton's optical experiments, his presentation of ecclesiastical and prophetic history, disputes with rivals such as Leibniz, his domination of the Royal Society as president, and the origins of eighteenth century natural philosophical practice, all point towards an effective combination of political and technical analysis of the social construction of Newton's programmes <36>.

What evaluation, then, would such an analysis offer? In fact, it would challenge a powerful model of the scientist at work. It has often been pointed out that the orthodox (and idealist) model of such work is highly functional in the training of scientists and the defence of support for research. There is no doubt, therefore, that any historiography of science which concentrates on practices of persuasion and assent, and which therefore inevitably con-

fronts scientific culture as a scene of political struggle, would automatically subvert such functions. As Boris Hessen suggested in 1931, the history of the sciences is always a powerful resource for, and reflection of, models of science and social relations. In this respect, A.R. Hall's attack on value-free sociology of science was extremely accurate. Hall wrote that it was fatal 'to suppose that it is not worth while to take sides or that the determination of the historian's attitude to the issue is not significant' <37>. Hall's argument was that since the development of science was such a significant element of modern culture, it was vital to privilege that development, examine its effects on the derivative category of society, and grant it a highly positive assessment. For exactly the same reason, an alternative historiography inevitably criticises the so-called 'textbook tradition', and also criticises perceptions of the place of scientific culture.

Just because Hessen also saw the construction of science as an overwhelmingly important part of the historical development of Western society, he too shared these criticisms. He saw no reason why Newton's apparently self-evident triumph obviated the need for its historical explanation, and he saw an urgent need for a form of history which could provide such an explanation. So should we.

'The founder of this religion will be a man of great power. The faithful will give him the title of Captain of the Newtonian Guard.'
(Saint-Simon, Lettres d'un habitant de Genève à ses contemporains, 1802)

Footnotes

- B. Hessen, 'The social and economic roots of Newton's *Principia*', in N.I. Bukharin et al., *Science at the cross roads* (first published 1931, republished London, Frank Cass, 1971), pp. 150-212.
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