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• *ABSTRACT*

Recent sociological studies of scientific discovery have challenged the assumption that such discoveries are easily identifiable processes which take place in the mind of heroic discoverers. In this paper, four examples of discovery stories are chosen from the critical period of transition from natural philosophy to the nineteenth-century scientific disciplines. In each case it is impossible to find any criterion for discovery apart from the local practices of contemporary research communities. 'Discovery' is a retrospective label attributed to candidate events by these communities – a technique for marking technical practices which are prized by the community. Each discipline was sustained by the reproduction of these new techniques, with the aid of an ideologically loaded model of discovery and discoverers. Finally, it is suggested that the early nineteenth century was also marked by a change in the historiography, as well as the practice, of the sciences. Natural philosophers had often presented their histories as methods for training practitioners in discovery, but historians of the sciences from the early nineteenth century separated the disciplined training of scientists from the heroic discovery moment, for which no training was possible. The emergence of the disciplined sciences was thus the context in which discovery stories were constructed, and in which a historiography emerged which made those stories effective.

Scientific Discoveries and the End of Natural Philosophy

Simon Schaffer

> Questions of science are very frequently career questions. A single discovery can make a man famous and lay the foundations of his fortunes as a citizen. Every newly observed phenomenon is a discovery, every discovery is property. Touch a man's property and his passions are immediately aroused. (Goethe, in Eckermann, *Conversations with Goethe*, 1836).

Two Aspects of Scientific Discovery

Consider a set of examples which are routinely treated as scientific discoveries: Uranus, oxygen, the electrostatic inverse square law, photosynthesis. Each was apparently accomplished between 1765

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and 1790. Each was the object of enquiry by groups at the Royal Academy of Sciences in Paris and at the Philosophical Society at Bath. Each has coupled with it the names of heroic discoverers: Herschel, Lavoisier, Coulomb, Priestley. The local context and structure of these cases plays an important part in the analysis presented in this paper. I use these examples to illustrate two closely related aspects of the history of scientific discovery. First, I consider the status of discovery stories in the work of historians, philosophers and sociologists of science. Philosophers often help themselves to historical examples of discovery, and the examples I have cited have been among the commonest of these. Such examples are chosen with considerable ingenuity, and the philosophical project often involves the construction of a transcendent definition of the discovery event which captures the important features of these examples. Thus it has been common to take discoveries rather unproblematically, as single events of individual mental labour whose analysis requires the examination of logical or psychological manoeuvres. However, historians and sociologists have found it increasingly difficult to provide examples which can easily be subjected to such processing. Instead, they have displayed discoveries as artefacts constructed within research communities and as attributes granted to candidate events by the sanction of those communities. Investigations of 'textbook history' and of 'scientists' accounts' have re-opened this field to such exploration.[1] Historical analysis shows that none of my four cases can be located in space and time as a single, unambiguously authored event. Yet each of those cases was granted the status of a discovery by natural philosophers in the late eighteenth and early nineteenth centuries. So the focus of my analysis will shift to the local reasoning and practices of specific groups of natural philosophers at that period.

It is not unusual to confront the process by which events in the history of science become less easy to interpret under the categories of a simplistic philosophy. However, a very important consequence follows from this process. The second aspect of discovery to which I point is its historic relation with the author. There is an heroic model of discovery in which analysis concentrates on the inspired genius: hence the long debate on the creativity of the scientist. This model bolsters an account of how science changes. All four cases mentioned here have been used to mark the end of classical research programmes and the initiation of mature scientific work in astronomy, chemistry, electricity and biology. Such changes are used to make the end of the eighteenth century a moment of critical discontinuity in the history of science, a 'second scientific revolution'.[2] In the final section of this paper I shall return to this problem of the end of natural philosophy.

However, if historians and sociologists have challenged the received view of the relation between author and discovery, then the view of scientific change should also be transformed. This transformation can be illustrated reflexively, by examining accounts of change and discovery produced in the late eighteenth and early nineteenth century. The view of heroic authors of scientific discovery did become current in the histories of science of the early nineteenth century: examples include the *éloges* of French and Scottish natural philosophers, the histories of astronomy of Bailly and Delambre, of mathematics by Montucla, of chemistry by Thomson and by Kopp. Bellone and Cannon have written of the appearance of a science of physics at this conjuncture, while Laudan and Nickles have written of the novel separation of contexts of justification and of discovery at that period. In Foucault's account of Cuvier's role in the history of biology, a new model of authorship sustains the privileged role of the new science of the nineteenth century.[3] All these changes included a change in the account given by the scientists of their own work. A reassessment of the process by which authorship is attributed to matters of fact in science would involve a revision of the style of history which has dominated our understanding of science since 1800.

The Identity of Discoveries

Discovery is an area of dispute which philosophers of science have used to raise the problems of demarcation and of normative analysis in an acute form. Philosophers have found fruitful opportunities here to instruct scientists on good practice, and to indicate those rather limited areas to which sociological or psychological analysis of science should be restricted. This was the force of the distinction between contexts of discovery and justification which, as Nickles and Curd have indicated, was inaccurately attributed to Reichenbach by subsequent commentators.[4] Such distinctions allowed demarcations within the realm of science, between reasoned argument and psychological insight, and within the realm of science studies, between philosophical or logical gloss and sociological or psychological exposition. Such opportunities arise because the problem of discovery is presented as an issue of individual mental endeavour. This stricture applies just as much to those who defend the existence of a logic of scientific discovery ('the friends of discovery', as they have been called) as it does to those who deny that such a logic can be built, and who confine the problem of the discovery event to the province of the mind. Kordig tells us that 'the proper distinctions are three: initial thinking, plausibility and acceptability. Logic is not essential to initial thinking. Good reasons are not there *required.* Psychology, sociology etc. are thus relevant.' Fox Grmek and his colleagues, discovery is 'the specific moment of understanding' to be investigated using 'psychological and sociological factors' by historians, and using 'codified language and logic' by philosophers. 'Rational objectivity is what counts when vicissitudes come to an end.' Zahar, by contrast, has argued forcefully against any 'irrational flash of intuition' or 'sudden inspiration,' notably in the case of Einstein's relativity theories. But here again the discovery is individualized: 'Einstein worked through a series of deductive steps,' susceptible to a perfect logical reconstruction. Laudan sums up the situation in philosophy accurately: 'I shall construe discovery rather narrowly as concerned with "the *eureka* moment", i.e. the time when a new idea or conception first dawns'. He argues that '*only* on this construal can any sense be given to the current debate about the existence of a logic of discovery'.[5]

The terms of this debate, therefore, depend on specific appeals to historical and sociological discovery stories. But there is a very important contrast between the evidence now provided by historians and sociologists and the demands of philosophers. In 1977, Grmek and others wrote that

> the lack of very precise or even genuinely accurate historical analyses of individual scientific discoveries is one of the reasons why the history of science has not yet been able to become truly the 'laboratory of epistemology'.[6]

It emerges that historical analysis must confine itself to privileged cognitive factors. McMullin has been more sanguine than Grmek about historians' abilities. They 'seem to be able to make quite good sense of the discoveries they write about.' But this 'good sense' turns out to be rather restricted. Discovery is understood as 'the initial creative formulation,' and 'it is ordinarily possible to construct a fairly satisfactory explanatory account on the basis of cognitive factors only.' Ultimately, McMullin suggested, 'the historical singularities of the case' can be suppressed and the historians can 'focus on the idea-relations only'.[7] In 1982, Koertge argued in similar terms: internalist historical accounts were used to defend the possibility of a logic of discovery, but, again, on condition that these accounts avoided 'noncognitive factors.' Koertge defined 'discoveries' as true propositions and 'discoverers' as those who first hold these propositions and who have good reasons for doing so. It followed that *scientists* alone could 'explain the content of scientific advances' and that historians 'explain why a certain scientist was the first to conceive of and take seriously a given idea (which later turned out to be successful)'.[8] Yet it is precisely this set of tasks — the detection of priority, the definition of a given idea, and the notion of subsequent success — which proves so problematic for any historical or sociological study of discovery. In fact, sociologists and social historians have found a series of aspects of this individualist, mentalist model of discovery deeply troubled: (a) the isolation of discovery in time and space; (b) the authorship of discovery; (c) the preconditions of work which generates discovery; (d) the process by which discovery is recognized. Each aspect will be illustrated for the late eighteenth century and some consequences drawn for our models of the discovery process itself.

Identity is a key attribute for the conventional models of discovery. In principle, we should be able to identify the moment after which the discovery can be said to have occurred, and an individual who can be credited with the discovery. It is now well known that such decisions are difficult. In particular, Kuhn, Barnes and Brannigan have all used some of the cases considered here to show that attributions are matters of dispute both for contemporaries and for historians of science. Historians of science have used these insights to explore discovery stories without adopting an uncritical acceptance of individualist and mentalistic accounts. Such historical analysis of discovery always turns out to be an analysis of the criteria the relevant community was using at a particular period. It does not generate an account easily susceptible to the form of philosophical gloss which searches for an ahistorical and transcendent criterion of discovery.[9] This is exemplified in the case of Uranus. No reasonable but external rule for spotting the discovery seems to work here. At 10.30 pm on Tuesday 13 March 1781 William Herschel recorded in his observing book 'a curious either Nebulous Star or perhaps a Comet' in 'the quartile near Zeta Tauri', and the following morning entered in his volume on 'Fixt Stars' the comment that 'I discover'd a Comet'. Can we identify this as the moment at which Uranus was discovered? No, for to do so would be to allow an external criterion which picked out *any* astronomical sighting of the object. Eighteen such sightings are recorded, including six by Lemonnier within a space of nine nights in January 1769.

For Lemonnier the object was a star, and for Herschel it was a comet. These are both 'erroneous' observation claims. Indeed, Herschel continued to see the object as a comet until spring 1782, more than a year later. He sustained this claim with two subsidiary observation statements: that 'the Comet approaches to us' and that 'the Comet had a very visible daily parallax which was sufficient to prove it to be on our side of the Sun'. After spring 1782, when the object had come to be seen as a planet, both these subsidiary claims were rejected. The object was, at that period, receding from the Earth and further away than Saturn. It seems we cannot attribute the discovery to Herschel until after spring 1782. Yet by then several other astronomers had described the object as a planet and had rejected Herschel's claims about its character, motion and position. The very first to use the term 'planet' was Maskelyne, on 4 April 1781, and others followed during the year. Did Maskelyne discover Uranus? No, since he had not yet decided on its character in November 1781: 'no certainty can be obtained of its Species as yet'. We have to look beyond some transcendent criterion here to see how late-eighteenth-century astronomers agreed on the identity of the discovery of the planet, and on its discoverer.[10]

This problem arises for all models which seek to identify the individual work of the discoverer. The case of oxygen has received most attention here, especially since its use by Kuhn in his original analysis of the problem. He convincingly argued that 'there is no single moment or day which the historian, however complete his data, can identify as the point at which the discovery was made'. We can point to the impossibility of easy discrimination between Scheele, Bayen, Priestley, Hales or Lavoisier in the case of oxygen, or Bernoulli, Robison, Stanhope, Priestley, Cavendish or Coulomb in the case of the electrostatic inverse square law. Kuhn's initial formulation, however, was somewhat marred by his search for some means of guaranteeing the labelling of a discoverer in a way which does not depend on the local practices of the relevant community. The category of 'anomaly' took pride of place here, but, as Brannigan has suggested, the category is merely tautological. Kuhn said that a discoverer must see an anomaly to be a discoverer, yet we only know that such an anomaly was perceived because the natural philosopher in question came to acquire the status of discoverer. 'Anomaly', just as much as discovery, is an accomplishment which natural philosophers must achieve.[11] Consider the oxygen example. Here the 'anomaly' presumably centres on the weight gain some chemists detected after the calcination of metals, and, in particular, in the calcination of mercury in a sealed vessel. By 1775 the status of this red mercury calx was already a well-established puzzle for many chemists, as Perrin and Holmes have both shown: indeed, the character of this substance as a calx was a problem at least as old as

Renaissance alchemy.[12] On 26 April 1775, Lavoisier presented the Royal Academy of Sciences with an account of a set of experiments on the air generated from heating this calx. Holmes has shown that this presentation was 'an imaginative reconstruction', and Lavoisier said as much. His air was described both as 'the air itself entire' *and* as 'the purest *portion* of the air in which we live'.[13] Koertge offered a recipe for deciding on the discovery here:

> As soon as we employ sentences instead of nouns, it is easy to unravel old trick questions on history of science exams, such as 'Who discovered oxygen?' One simply points out that Priestley discovered that heating mercury calx *produces* a gas which supports combustion, while Lavoisier discovered that the calx decomposes and *releases* such a gas.[14]

However, it is utterly unclear what evidence there is to help us point out these 'discoveries.' This is just because of two difficulties: first, the process by which the natural philosophers present accounts of their work, and persistently revise those accounts; second, the intimate connection between the acceptance of a discovery account and allegiance to a very specific set of technical practices. Here, the use of 'ability to support combustion' was itself a highly problematic matter of dispute in contemporary chemistry. In the next section of this paper I shall explore this connection in detail; here I shall concentrate on the authoring of discovery stories by natural philosophers themselves.

The presentation of April 1775 was later to be labelled by Lavoisier's research group as a discovery story. This needed a long period of hard interpretative work. In late February 1775, less than one month before the presentation, Lavoisier recorded in his notebook that some form of 'matter of fire' was 'an emanation from metals in calcination' and then 'combined' with common air. He frankly acknowledged that 'all of this agrees very much with the system of Priestley', though he claimed 'there is nevertheless a very notable difference'. Holmes has pointed out that Lavoisier could not spell out what this difference was: at any rate, we do not yet have the recognizable formulation of the 'sentence' we seek. A more detailed analysis of the process by which Lavoisier established the existence of a definite problem, presented his work as an answer to that problem, and then claimed discoverer status, shows that the formulation of this sentence will always remain elusive.[15] On 13 February 1776, still using Priestley's detailed techniques for the assessment and identification of airs, Lavoisier wrote in his notebook that the air generated from mercury calx 'was found to be the dephlogisticated air of M. Prisley'. This was almost a year after his presentation at the Academy. In order to turn that presentation into a discovery account, Lavoisier had to rewrite the whole past history of pneumatic chemistry.[16] In a memoir of April 1776, he began referring to 'a principle of fire much more ancient' than the phlogiston of Priestley or that of Macquer or that of Stahl. Finally, as Wilda Anderson demonstrates, Lavoisier was to publish a paper in 1783 in which an ingeniously reconstructed discovery story was set in spring 1775. Lavoisier now declared that his alleged opponents, 'who seek to persuade a public that everything that is new is not true', had 'managed to find, in an ancient author [Stahl], the first germ of this discovery'. Referring to his notebooks of 1774–75, Lavoisier wrote that 'the impartial public' had now judged that Lavoisier himself should be 'considered as the *author of the discovery* of the cause of the increase in weight of the metallic calxes'.[17] Such stipulations *make use* of the status of discovery and discoverer; they are not evidence which the historian can use to grant that status. The contests of the early 1780s allowed Lavoisier to make the question of mercury calx an 'anomaly' for chemists of the early 1770s, to make his work of 1775 an answer to that anomaly, and then to make that answer into a discovery. Finding the first enunciation of some sentence would mean acting antihistorically: accepting stories Lavoisier told in 1783. Anderson has very usefully drawn our attention to the problems of authorship and of technique here, and these are the central problems which sociology and history of discovery should address. There is no pre-given criterion of 'appropriate discovery' above and beyond the behaviour of these French chemists.[18]

Two further aspects of the quest for identity need to be stressed. First, we are considering cases where we must search for the definition of the object discovered. The inverse square law illustrates this well. Second, we must consider the problem of the point from which the discovery can be spotted. If we assume discoveries have a nature which can be written out for all history, then we will have to use hindsight, since there is no means to assess this identity before the work we examine has happened. In the case of electrostatics, Heilbron has drawn a very useful contrast between 'force' as macroscopic interaction, and '*force*' as the microscopic particle–particle interaction whose distance law was sought. Then it proves very difficult to find a discovery. Coulomb's work in Paris in 1777–85 was posited on the assumption of an inverse square law for *forces*, and it was by no means recognized by his community as an unexpected discovery.[19] In fact, the publications of 1785 were presented as exemplifications of the virtues of the new techniques of the torsion balance and the torsion pendulum Coulomb had developed, and as a firm rejoinder to claims of Aepinus and others about the possible repellent force between normal matter. Terms such as 'electrical mass' were used without qualification in Coulomb's memoirs, and this 'mass' was given as an effect of arrangements of electrical fluids. Issues of public knowledge also played a role, as in the contemporary oxygen case. Robison and Cavendish, in many ways decisive if we seek marks of an identifiable discovery, did work which remained unknown for many years after Coulomb. His contemporary natural philosophers, such as Cavallo, De Luc or Volta, all disqualified the work of Priestley or of Coulomb, since they were held to have displayed only the special properties of the particular equipment they used. The electrified can Priestley used lacked any detectable ponderomotive forces inside it, and the torsion balance or pendulum of Coulomb measured macroscopic effects. They were both constructed on the assumption of an identity between weight relations and charge relations, and, it was held, could not be said to reveal anything of the 'true' microscopic *forces*.[20] Furthermore, historians can place each claimant in widely differing practices in which the meaning of any force law would be extremely different. Only from a perspective placed *after* the work of Poisson might we see the 'forces' investigated here as being identical. They could not unproblematically be identified as such in the eighteenth century. Historians must have recourse to what Robert Westman has called the 'local rationality of the battlefield' and not to the identity of the discovery and the discoverer.[21]

The photosynthesis case highlights the problem of hindsight in historical explanation. Thus, in considering the contemporary problem of heat theory in the late eighteenth century, Richard Burian observes that even though Davy and Rumford allegedly discredited caloric theory by 1800, the caloric theory was not abandoned until the 1830s, and he follows Brush in suggesting that this was as a result of Fresnel's work of the 1820s on a wave theory of *light*. Burian comments that 'the historical order can be strongly defended as reasonable'. Perhaps — however, what concerns us here is the effect on the presentation of the work of Davy and Rumford of the career of wave theories in the 1820s and 1830s.[22] Such retrospection was endemic at this period. Discovery accounts such as those of oxygen, photosynthesis and caloric were obviously closely linked. Delaporte has suggested, with little evidence, that photosynthesis emerged 'on the substitution of Lavoisier's system of chemistry for phlogistic chemistry, as well as the introduction of quantitative methods'. But the two leading candidate discoverers — Priestley and Ingenhousz — were neither adherents of Lavoisier nor of his quantitative methods.

The only way of granting the discovery of photosynthesis an identity would be to use modern biology, or else to report how late eighteenth-century natural philosophers behaved. Modern biology would demand reference to the action of light, oxygen and chlorophyll. Such demands, of course, would exclude all eighteenth-century claimants. We could relax these constraints, and accept 'dephlogisticated air' and 'green matter' in late eighteenth-century reports. But this would destroy the label 'photosynthesis'.[23] Holmes's recent lengthy study shows the painful process by which Lavoisier and his colleagues in Paris constructed a general model of respiration and how plant respiration was placed within this structure of novel techniques. Anderson has fully demonstrated how Lavoisier's and Priestley's contrasting vocabularies are key sites of dispute and revision in the construction of this model:

> Only when the language has been sufficiently retranslated so that Lavoisier's experimenters perceive what they are asking can an experiment to test the question generate 'facts'.[24]

In view of this central role of linguistic usage in the very discovery stories being investigated, it is damaging to gloss eighteenth-century language in the way we would need if we were to seek identifiable discoveries. Language and rhetoric helped make the discovery claim a routine performance for rival versions of eighteenth-century pneumatics. In Bath and Calne, the question was not the 'discovery' that vegetation interacted with light and restored the atmosphere. That was the precondition of the pneumatic programme. The strength of that precondition, as McEvoy and Holmes have shown, is exemplified in terms such as 'vitiation', the label given to processes by which air was rendered less respirable. For Priestley, 'vitiation' just was phlogistication, and between April and November 1776 Lavoisier retained Priestley's term in his own work on the spoiling of air.[25] As he transformed the practices he used to test the identity of airs, he changed the connotation of the word. In contrast, Priestley retained it: he recorded that 'I fully satisfied myself that the green matter which I had discovered to produce dephlogisticated air was a vegetable' and that 'nothing turns green or consequently yields dephlogisticated air but in the light'. The accomplishment he reported to correspondents in this way was focused on the generation of and character of his prized 'green matter'. Ingenhousz, for example, an erstwhile colleague at Calne, was seen nevertheless as breaking the theological rules of the pneumatic programme, since he argued for the spontaneous generation of such matter. Similarly, Priestley attacked Scheele, another possible candidate discoverer, just because he denied the 'purification of respirable air by vegetation', and not as any kind of rival priority claimant. The processes of photosynthesis were invisible at least until the work of Saussure in 1804. We can say that the research of the early nineteenth century *produced* the discovery of photosynthesis in the late 1770s.[26] Without some form of teleology, there is no reconciliation available. It seems simultaneously unnecessary, ill-mannered and impossible to find a mark for discovery separate from and superior to the locally generated rules of communities of natural philosophers.

Discovery and the Fixing of Scientific Practices

Discoveries matter in scientific practice because they offer a model of the relationship between the individual worker and the scientific community. When a candidate event is given the status of a discovery, it is 'fixed', just as Fleck suggested that scientific facts are fixed.[27] This 'fixing' is linked with assent to the matter of fact and to the identity of the discovery. Two key processes mark this assent: a new fact is *replicated* and it is given an *author*. Recent historical and sociological research has shown that these two processes involve complex negotiations inside the scientific community. Collins and others have shown that replication is an accomplishment. No given piece of work has the inherent quality of a competent copy. Collins writes that 'scientists' actions may . . . be seen as negotiations about which sets of experiments in the field should be counted as the set of competent experiments' and so 'different sets of criteria [of adequacy] can lead to the discovery of different phenomena (facts)'.[28] This suggestion has important implications for the understanding of scientific discovery. If replication and authorship are matters of negotiation, then there is no event which corresponds to an automatic or instant discovery. A complex enterprise, accessible to historical and sociological understanding, generates objects which are then labelled as discoveries. Subsequently, the story of that process is rewritten. The lengthy enterprise is telescoped into an individual moment with an individual author. In his analysis of the case of Mendel, Brannigan has seen this as a process of reification. To treat discoveries as individual events with obvious and transcendent signs of identity is to treat this rewritten story as the basis of our analysis — that is, to commit a 'secondary reification'.[29] On the other hand, sociologists have shown how this rewritten story is itself produced along with the production of discoveries, and cannot be used as an explanatory basis for these discoveries. This process of rewriting has the associated results of defining the set of practices which produce the fact, and of sustaining the character of the fact. In his analysis of Hewish's Nobel Lecture on the pulsar discovery, Woolgar summarizes this point succinctly:

> The work of establishing the facticity of the phenomenon does not end with the settling of controversy. On each and every occasion that participants refer to a fact they do so in such a way that the facticity of the phenomenon is re-established.[30]

My suggestion here is that this re-establishment is closely connected with communal and retrospective decisions about discovery stories.

These considerations suggest that the cases I have cited from the late eighteenth century should be examined as processes of replication and of the attribution of authorship. We should seek to show historically how contemporary natural philosophers negotiated the identity and the reality of such events. In each case, the identification of the discovery took place by picking out a single move within a complex research programme. The isolation of the discovery served the interest of members of the research community in a variety of ways. By formalizing claims to replication, prized techniques of the research programme were picked out, and by attributing authorship, exemplary techniques of the programme were then fixed and celebrated. To accept a discovery was to declare allegiance to the work of the programme. Above all, the emphasis is on the practical technologies of scientific work. For example, we have suggested that historical interpretation of the cases of oxygen or of photosynthesis is frustrated when it seeks to isolate a genuine discovery moment and a true author. The production of dephlogisticated air would only be recognized as a discovery by those who accepted the specific interpretation and the techniques in pneumatic chemistry which natural philosophers such as Priestley were using. Discovery is used to reproduce the knowledge and the techniques of a specific practice: in the widest sense, discovery is a tool which helps scientists enter and remain in a defined scientific culture.[31]

This conception illuminates several aspects of the historical behaviour of scientists. First, it allows the analysis of boundary disputes between different research programmes. William Herschel's work with his telescope at Bath in spring 1781 was *not* part of conventional late eighteenth-century planetary astronomy. His work was, instead, part of the 'natural history of the heavens', in which the stars would be classified in species to gain access to the inner construction of the stellar universe. Contemporaries recognized this crucial contrast: on the one hand, Herschel was later at pains to emphasize that the discovery was *inevitable* just because he was not an astronomer in the received sense, while on the other hand Herschel's contemporaries often denied that he was any kind of astronomer, and occasionally treated him as eccentric or insane. Herschel told the mathematician Charles Hutton that it was not 'a lucky accident that brought this star to my view' but that 'it was that night its turn to be discovered'. When the events of March 1781 were then reified as the *discovery* of a *planet*, Herschel then exploited the status of the discovery to exemplify his new techniques. No other worker found it easy to replicate Herschel's observations, so the establishment of the claim that a discovery had occurred was seen by astronomers as a direct challenge to their practice. The acceptance of the discovery demanded allegiance to a new form of work. Hence Herschel found considerable resistance to the publication of his observation reports and to the claim that these reports were any kind of discovery. He told the Secretary of the Royal Society in May 1781 that it was necessary to provide

> a short account of the manner in which I found out the Comet, as I have been asked that question by several Astronomers who could hardly imagine what inducement I could have to look with such high powers in a place so little promising of any new discovery.

Discovery was an accomplishment and a prize: those who began the work which fixed the discovery of the planet necessarily negotiated the boundaries between Herschel's extraordinary practice and positional astronomy.[32]

This story could be extended to the further career of the discovery of Uranus and its author. Three aspects of that career are illuminated by seeing discovery as an issue of fixing technical practices. First, the discovery of Uranus was accepted in the 1790s by incorporating the object within a prized section of planetary astronomy. This section was that associated with Bode's Law, which described the spacing of the planets round the Sun. By incorporating Uranus in this manner, a process made even more evident in contests about its proper name, Herschel's work became part of planetary astronomy, despite the persistent problems of replication. Second, further work on Uranus took place within this alternative programme. This enabled astronomers to keep hold on Uranus even though its orbit refused to conform to their standard models of planetary motion: it was very difficult to reconcile data from before and from after 1781, and by 1841 the best calculated orbit deviated from standard observations by 70″ of arc. Astronomers suggested that a comet had struck the planet very close to the moment they now counted as its discovery in 1781, or that astronomers before Herschel had committed gross errors in their observation reports. Whichever recourse the astronomers chose, the status of Herschel as discoverer and Uranus as an unchallengeable discovery functioned to keep the planet's motion as a problem of the utmost importance within planetary astronomy, and defined the means by which that problem should be solved. For example, even though both Airy and Bessel contemplated abandoning the inverse square law of gravity to save Uranus's phenomena, this strategy was swiftly suppressed by the rules of celestial mechanics.[33]

Finally, the work on Neptune in the 1840s exemplifies the way in which discovery gains its meaning through practical techniques. Historians have shown that different courses of action were followed in the 1840s among different groups of astronomers in Cambridge, London and Paris. Different meanings given to the Neptune discovery in France and Britain were due to differences in astronomical practice and status in the two countries. The American mathematician Peirce claimed that 'Neptune' was in fact a different planet from that 'predicted' by Leverrier and Adams. Similarly, evidence has been produced that the distribution of these predictions, and thus the significance of the discovery, was dominated by the structure of the research networks centred on Cambridge and Greenwich observatories. Thus interests in contrasting research practices profoundly affect the judgements of identity — in this case, even the judgement of the identity of the planets Uranus and Neptune. They also affect the judgement of authorship — in this case, the roles and positions of Herschel, Leverrier and Adams.[34] This is of extreme significance, since identity and authorship emerge as matters in dispute, and as intimately connected with the interests and practices of different research groups. For example, the discovery of Neptune is often presented as a good case of a missed 'multiple discovery', since, 'but for the fact that he was diverted by other work', the Cambridge astronomer Challis would have observed the planet at much the same time as the observers at Berlin. Yet the work of Challis and Airy was precisely posited on techniques of planetary observation very different from those used at Berlin, but very similar to those used in the discoveries of asteroids in the early nineteenth century. To accept the Berlin work and Leverrier's prediction as tantamount to the unique discovery of Neptune was to endorse a very specific local set of astronomical and theoretical techniques, quite different from those endorsed at Cambridge. Supporters of the Cambridge network worked hard to make their techniques secure by making Adams the true predictor and Challis the unfortunately late observer of the planet.[35] Such contests show again how the isolation of a discovery is the endorsement of a complete research programme.

The struggles round oxygen and photosynthesis also show how discovery acquires its status as part of rival technical programmes in science, and how discovery is made into a category which fortifies members' interests. The work which Priestley conducted when making dephlogisticated air was dominated by two key technical assumptions quite peculiar to his own programme: first, Priestley's techniques picked out processes which involved the vitiation and restoration of common air, since this was part of his theologically based pneumatic system; second, restoration and vitiation were to be linked to the principle of phlogistication through the 'nitrous air test', in which volumes of the air whose virtue was being assessed were shaken with measured volumes of nitrous air. Priestley wrote:

> It is not peculiar to nitrous air to be a test of the fitness of air for respiration. Any other process by which air is diminished and made noxious answers the same purpose, but the application of them is not so easy or elegant and the effect is not so soon perceived. In fact, it is phlogiston that is the test . . . it is wholesome in proportion to the quantity of phlogiston that it is able to take.

The use of this nitrous air test was only meaningful for those who shared Priestley's gloss on its significance. Since the character of dephlogisticated air was identified through the use of that test between summer 1774 and spring 1775, the new air which Priestley made was necessarily interpreted in conformity with his technical practice.[36] On 31 March 1775, Lavoisier demonstrated this test on the air left over heated mercury calx in the presence of Macquer and other witnesses. He initially recorded the result that 'according to this operation, one could judge that this air is more perfect than common air'. He *then* reported that the test result showed the air to be only equally good as common air, basing this revised opinion on results sent from Priestley in January 1774. The ambivalent result of late March 1775 is reflected in the ambivalent presentation of the paper at the Royal Academy the next month. Holmes comments that

> in these operations Lavoisier was simultaneously learning how to perform the nitrous air test . . . and utilizing it to characterize the air derived from the reduction of mercury calces. He was assimilating into his own repertoire of concepts and practices Priestley's discovery of the new air.

In England, the test distinguished between this new air and phlogisticated nitrous air, and also gave dephlogisticated air its place in Priestley's pneumatics as 'eminently respirable air'. If Priestley's work of 1774–75 were presented as a discovery, rather than 'adventitious' or 'mistaken,' then that would constitute a claim to the superiority of his whole scheme of chemical technique, and his system of pneumatics.[37]

This emerged very clearly in the contest with the French chemists. Priestley consistently rejected Lavoisier's glosses of Priestley's published texts. Lavoisier wrote in 1783 that

> it could happen that Mr. Priestley, when reducing the minium [lead calx] using the inflammable air, given that his object was not to determine either the quantities, nor the increase or decrease of weight, would not have sought to use great precision in his results.

Anderson rightly draws attention to 'Lavoisier's transcription of Priestley's experiment': she adds that, for Lavoisier, 'Priestley's blindness is a function of the nature of his language'.[38] In the same way, Priestley held that it was not possible to translate Priestley's terms without the proper use of Priestley's experimental technique of identification and analysis. Thus he complained that Lavoisier had attributed to him the view that 'inflammable air long agitated in water appears to differ in nothing from common air', while Priestley himself wrote that 'a candle burnt in this air as in common air, only more faintly; but that, by the test of nitrous air, it did not appear to be so good as common air'. That is, by suppressing references to Priestley's prized nitrous air test in the transcriptions the French produced in the 1770s and 1780s, it was argued that they had distorted Priestley's discovery claim.[39]

Priestley also denied that the French chemists had in fact produced any successful experimental exemplification of their new views. Replication was the key issue here, and success in replication was assessed as part of rival schemes of practice. The French trials required 'so difficult and expensive an apparatus, and so many precautions in the use of it, that the frequent repetition of the experiment cannot be expected'. Priestley suggested that the French chemists should 'make the experiment in a manner less operose and expensive, requiring fewer precautions'. As McEvoy has shown, the rival discovery claims which Priestley made were part of Priestley's picture of French chemistry as tyrannical, since only those who had adopted French techniques and nomenclature were to be counted as proper chemists. The systematization of the French regime in a new language and new techniques formalized this contrast, and so fixed the rival descriptions of discovery made by Priestley and his critics.[40] Finally, as several historians have shown, the presentation of Lavoisier's work as the 'discovery of oxygen', followed by 'the discovery of the composition of water', was itself a polemical move made after Lavoisier's work. In fact, Lavoisier's researches of 1772–83 are more easily seen as the replacement of the matter of fire by an acidic principle as the source of weight changes during calcination. Hence, for example, the difficulty historians have in seeing the difference between phlogiston and matter of fire in Lavoisier's notes of 1775; hence, too, the intriguingly tortuous redrafting Lavoisier indulged in when composing key papers such as that on respiration and combustion of autumn 1776. Holmes shows Lavoisier referring to 'the opinions of Mr Priestley with which I am imbued', and then hesitating between terms such as 'experiments', 'results', and 'opinions', or between describing his attitude to Priestley in terms of 'discussion' or 'refutation'. Holmes suggests that Lavoisier had to 'credit his colleagues with what is due them, and . . . point out their shortcomings, so that they will accept his work as an advance on their own'. The same problems arise over the discovery of the composition of water in the early 1780s: experiments due to Priestley and his allies, such as Watt, Cavendish and Fontana, were processed by the French into new terms and set within new practical techniques. Thus Lavoisier replaced the term 'phlogiston' by the term '*caloric*', and then used the concepts of 'discoveries' such as those of *oxygen* as weapons in the enforcement of his system.[41]

The nitrous air test was also central in the rival discovery accounts in the case of photosynthesis. Priestley, Ingenhousz and Fontana all used crucially different equipment in performing this test: different methods of mixing nitrous air with the air under test depended upon different models of the character of air and its virtues. In Priestley's case, as we have seen, the nitrous air test was dependent on his concept of the melioration of the atmosphere. After 1771, Priestley set up a series of analogies between the processes of combustion, respiration, putrefaction and vegetation, and it was the apparent breakdown of that analogy in the case of vegetation, which thrived in common air and restored the air above it, which led to the work now labelled as the discovery of photosynthesis. Both the system of analogy and the assessment of atmospheric restoration were part of the practice which Priestley's programme demanded, and so his attacks on other workers, such as Ingenhousz or Fontana, concentrated on their illegitimate use of rival techniques in assessing atmospheric restoration.[42] Priestley attacked the physician Thomas Percival, who claimed that fixed air was food for plants, because, according to Priestley, 'one clear instance of the melioration of air in these circumstances would weigh against a hundred cases in which the air is made much worse by it'. Percival had not replicated Priestley's trials, because Percival had not sought the evidence for atmospheric restoration hidden in those trials. Furthermore, replicators of Priestley's experiments on vegetable growth in water were instructed that these trials were more 'natural': 'the plants that grow in water are in as perfect health in my jars . . . as in the open air, and therefore perform all their natural functions in perfection'. This claim disqualified all those experimenters who grew plants in the open, risking violation of those 'natural' conditions Priestley had produced.[43]

It was in the course of these routine exchanges about the proper means to be followed in producing competent versions of plant growth trials that the issue of discovery was raised. Collins has argued that 'Where there is disagreement about what counts as a competently performed experiment, the ensuing debate is co-extensive with the debate about what the proper outcome of the experiment is'. His conclusion that 'the closure of the debate about the meaning of competence is the "discovery" or "nondiscovery" of a new phenomenon' is amply demonstrated in the photosynthesis case.[44] The region in which Priestley was regarded as the performer of competent experiments on the restoration of the air was the same as the region in which he was regarded as the discoverer of a process by which that restoration occurred. That region was established by personal contacts in Wiltshire and in Birmingham, by the despatch of instruments and data which embodied this competent pattern of practice, and by the creation of a new instrument specifically based on the nitrous air test, called the 'eudiometer'. The discovery which Priestley authored was obviously connected by these means with his own account of vegetable action. So rivals were attacked because failures to replicate Priestley's work amounted to failures to use the same arsenal of tests and of language: Priestley wrote in 1780 that

> it is altogether without reason that the Abbé Fontana . . . pretends that the measure of *good* and *bad* nitrous air comes to the same thing in his method of applying the test. I am astonished and provoked by the little care with which some persons make experiments, and the confidence with which they report them.

Since the category of 'restoration of the atmosphere' was meaningful only for proficient workers in Priestley's pneumatics, only those workers could legitimately make Priestley the author of a discovery, and only those workers interpreted the discovery as a demonstration of the relation of light, vegetation, dephlogisticated air, and divine benevolence.[45]

The career of the inverse square law in electrostatics has also been interpreted as a product of contests between rival research traditions and practices. For example, Coulomb's work of the 1780s was successfully displayed as the demonstration of the force law, but this success was confined to the tradition of mathematical physics and engineering from which his construction of the torsion balance emerged. In his two memoirs of 1785, Coulomb claimed to have shown that the ponderomotive force between two electrified bodies varied as the inverse square of the distance between them. Only the tacit and sanctified assumption of the analogy with Newtonian gravity allowed Coulomb to go on to claim that the proportion between this force and the expression in distance was due to the 'electrical masses' of the two bodies: King writes that neither logic nor experiment licensed this second claim.[46] But it convinced those members of Coulomb's community for whom such an analogy was an integral part of their practice. Highly contrasting practices co-existed in the late eighteenth century, however. There were important differences between the direct measurements of macroscopic forces, such as those of Robison or Coulomb, and the tests of the claim that electric fluids which obey such a law would display no force within a spherical shell, such as those of Priestley and Cavendish. A mathematical approach drawn from Aepinus, for example, was often explicitly rejected in Britain. Benjamin Wilson rejected the 'introducing of algebra in experimental philosophy', and George Adams claimed that the 'mathematical theory of electricity has closed the door on all our researches into the nature and operation of this fluid'. The rejection of Aepinus's practice included the rejection of certain forms of the discovery of an electrostatic force law.[47]

Coulomb's work, too, was ignored in Britain and Germany until after 1800, as Home and Heilbron have both observed. Citing the failures of the German physicists P.L. Simon and G.F. Parrot to replicate Coulomb's claims, Heilbron writes that 'taken alone the results of the torsion experiments were not compelling'. The acceptance of the torsion experiments as a discovery authored by Coulomb was in fact an acceptance of French physics in its Laplacian form. It was difficult to replicate Coulomb's torsion experiments because of charge leakage, the twisting of the wire, and many other factors. The success the practitioners of French physics achieved in making Coulomb a discoverer, despite the enormous problems in the technical presentation or replication of his experiments, was a powerful weapon in the campaign which gave new French physics its dominant place by 1800. The status of the torsion experiments as a *discovery* and Coulomb as their heroic *author* made those difficulties less damaging. Ultimately, Poisson and Laplace expelled them from the official history of physics.[48] The status of such official histories is now a familiar part of sociological understanding of disciplinary formation in science. Thus, in his account of black-body theory at the end of the nineteenth century, Kuhn has drawn attention to the strenuous resistance with which scientists confront the recognition that 'their discoveries were the products of beliefs and practices incompatible with those to which the discoveries themselves gave rise'. In his analysis of the myths surrounding the discovery of X-ray diffraction, Forman labels this process 'inversion': 'the mythical event or discovery' is accounted as the cause of the overthrow of views actually held only by the 'decidedly unorthodox'. In his examination of the career of the weak neutral current in the 1970s, Pickering argues similarly against 'scientists' history' and thus in favour of the inextricable relationship between phenomena and practice.[49] In each case examined here, too, discovery stories were deliberately constructed and assent to those stories was then gained as assent to a complex of practices. Such reification takes its place, therefore, as a key technique in the accomplishment of closure in scientific work. In the final section of this paper, I will offer some conjectures about the genealogy of this process and its connection with the historical changes in the structure of the sciences at the end of the eighteenth century.

The End of Natural Philosophy

I have argued that the four late eighteenth-century cases treated in this paper all show how discovery was a label given to a set of events and to the work of a specific author. The label was granted retrospectively by a research community, and the process of labelling was co-extensive with the fixing of a group of practices pursued by that community. I now wish to place this process in its own historical context — that of the unprecedented emergence of disciplined research schools in the natural sciences at the start of the nineteenth century. This is an unfamiliar aim in sociology. We have yet to achieve a social history of the terms used in the models natural philosophers and scientists deployed when portraying their own work and explaining its changes. Some philosophers have pointed out the marked change in discovery accounts given in epistemologies of the nineteenth century. Laudan has written of the 'abandonment of a logic of discovery', while Nickles refers to 'the great logical inversion' in British philosophy, in which authorities such as John Herschel and William Whewell began to replace prescriptions for discovery by a concern with the principles of discoverability — that is, with the techniques of justification.[50] Historians of early nineteenth-century science have drawn attention to the construction of research schools at this period. German reforms at Giessen, Berlin and Göttingen, the career of Laplacian physics in Paris, and the Cambridge programme of mathematical physics, have been used to show the link between the social change in new scientific disciplines and the content of those disciplines. Thus analytical chemistry under Liebig at Giessen, the new sciences of electromagnetism in Germany and Britain, and physics education in Scotland, Cambridge and Paris, all necessarily involved new modes of training and new presentations of how science should be pursued and had emerged in history.[51] In his seminal *History of the Inductive Sciences from the Earliest to the Present Time* (1837), Whewell argued that the end of natural philosophy was marked by the emergence of disciplined, trained cadres of research scientists. In his presentation of the work of eighteenth-century electrical philosophers, he argued that

> a large and popular circle of spectators and amateurs feel themselves nearly upon a level, in the value of their trials and speculations, with more profound thinkers: at a later period, when the subject is become a science, that is a study in which all must be left far behind who do not come to it with disciplined, informed and logical minds, the cultivators are far more few, and the shout of applause less tumultuous and less loud . . . The experiments, which are the most striking to the senses, lose much of their impressiveness with their novelty.

I have suggested elsewhere that eighteenth-century natural philosophy was distinguished by the audience relation Whewell describes here.[52] The end of natural philosophy was accompanied by the appearance of models of discovery which appealed to *discipline* and to *genius*, and which have dominated theories of science ever since.

Each of the discoveries discussed in this paper figured in the histories of natural philosophy which were produced in the late eighteenth century. Priestley's work is exemplary in this respect. McEvoy has shown that Priestley's successive *Histories* of optics, electricity, and the one he planned for natural philosophy, were all designed to recapitulate for the reader the progress of the branch of natural philosophy in question. In his *History of Electricity* (1767), Priestley argued that the synthetic mode of presentation of discovery, typified in the texts of Isaac Newton, was disastrous for proper instruction in science. The synthetic mode presented discovery as a set of logically inevitable moves, and the achievement of discovery as an heroic act. Instead, Priestley argued for a more accurate account, above all, for a truly philosophical history:

> Were it possible to trace the succession of ideas in the mind of Sir Isaac Newton, during the time he made his greatest discoveries, I make no doubt but our amazement at the extent of his genius would a little subside.

Priestley argued that

> the interests of science have suffered by the excessive admiration and wonder with which several first rate philosophers are considered; and . . . an opinion of the greater equality of mankind in point of genius would be of real service in the present age.

Priestley was stressing the problematic character of discovery stories, and he was connecting the production of philosophical histories with the way natural philosophers should be trained.[53] This concern was present, too, in the histories of astronomy of Adam Smith in the 1750s and of J.S. Bailly in the 1780s. Smith made the escape from wonder and admiration a motor for philosophical advance, and he deplored the over-valuation of the individual, heroic, discoverer. Bailly argued that 'reading the history of the sciences does not demand that one be a savant, but it is a means to become one'. This set of texts linked histories of natural philosophy to the problem of disciplinary formation. After 1800, the organization of training and research and the structure of natural philosophy were transformed, and histories of the sciences changed too. Historians now transferred the wonder of nature and of nature's divine author to natural science and its heroic authors. The problems raised in late-eighteenth-century discovery stories were also transformed. Scientific change was now to be referred to the individual discoverer, or else to a more generalized communal mind.[54]

There was an intimate connection between the appearance of these new discovery stories in the form of disciplinary histories, and the appearance of the new institutions in which those disciplines were fixed and disseminated. The career of chemistry is representative of this link. Hufbauer has shown in great detail how the formation of the German chemical community by 1795 was dominated by the specifically novel social forms in German universities and communication networks. These institutions grew during the battles fought in the 1790s around the discoveries of French chemistry. Proponents linked these battles with the political and cultural transformation of Europe in the same decade: 'revolutions are universal chemical movements', wrote Schlegel. Battle-scarred participants produced new ways of training chemists, and new histories to accompany this training.[55] J.B. Trommsdorf, director of a very influential new 'chemical–physical–pharmaceutical boarding school' at Erfurt, and just such a veteran of the chemical debates, composed his *Versuch einer allgemeinen Geschichte der Chemie* in 1806. He linked his new history with the successes of the philosophy of Kant and Schelling (provided they did not 'threaten to devour science and dictate the laws of nature from the lecturing desk'), and he laid down the rules for future disciplinary histories, which would be soundly based upon series of discoveries and would treat those discoveries as expressions of 'the related spirit of the age . . . That is the purpose of a general history of the science as science'.[56] In his similar *Lectures on the Method of University Studies* (1803), Schelling himself reported that 'to study the history of the sciences has become a kind of religion. In it philosophers discern . . . the intentions of the world spirit'. These *Lectures* were crucial statements of the argument for state endorsement of philosophically guided disciplinary formation, notably within the new natural sciences. As such, they were profoundly important resources for contemporary English reformers of disciplinary training and for English analysts of the right relation between history of science and science teaching.[57]

In Britain, the debate on the relation between discovery and genius in an account of how science grew was part of the debate in which new roles such as 'scientist' and 'physicist' were established. The paragon of Romantic chemistry, Humphry Davy, made glorious discovery both the ideal prize and the fundamental mechanism of scientific change. Davy argued in his *Consolations in Travel, or the Last Days of a Philosopher* (1830) that the function of the analyst of scientific change was 'perpetuating thought in imperishable words, rendering immortal the exertions of genius and presenting them as common property to all awakening minds'. Discovery accounts produced by the heroes now acquired their central place: the texts of Joseph Black's work on fixed air, hagiographically edited by his disciple John Robison in 1803, were widely read as evidence that Black was 'the historian of his own discoveries'. Such assessments of the status of the scientists as heroic founders of research traditions were aimed at making them into 'sages', heads of philosophical schools.[58] Coleridge himself, who inspired much of this manoeuvre, was keen to reserve the title 'philosopher' to the specifically elevated élite, not to the swarms of disciplined researchers. Levere has pointed out that Coleridge attacked Davy's use of the term 'philosopher': 'I have met with several genuine Philologists, Philonoists, Physiophilists, keen hunters after knowledge and Science', Coleridge conceded. However, he insisted, 'Truth and Wisdom are higher names than these — and *revering* Davy, I am half angry with him . . . for prostituting the name of Philosopher . . . to every Fellow who has made a lucky experiment'.

And it was Coleridge's ban on the term 'philosopher' at the meeting of the British Association in Cambridge in 1833 which prompted Whewell to endorse the new and outlandish term 'scientist' for these 'Fellows'.[59]

Coleridge's key contributions to these innovations included his argument for a national clerisy of chosen intellectuals, and an image of the contrast between the mental life of those intellectuals and that of the cultivators of science. Ideologues as contrasted as David Brewster and William Whewell shared this aim and welded models of reform of the institutions of science teaching and accounts of the history of scientific discovery. In his extraordinary *Life of Newton* (1835), Brewster wrote that 'nothing even in mathematical science can be more certain than that a collection of scientific facts are of themselves incapable of leading to discovery'. Newton had displayed 'the impatience of genius' which 'never will submit to the plodding drudgery of inductive discipline'. In his consideration of Kepler, in the aptly titled *Martyrs of Science*, Brewster commented that 'the influence of imagination as an instrument of research has, we think, been much overlooked by those who have ventured to give laws to philosophy'. Coleridge, like Brewster, made Kepler's work an example of 'the inventive, generative, constitutive mind', a 'glorious achievement of scientific genius'.[60] In considering precisely the same cases, the work of Kepler and Newton, Whewell was also keen to show the role the mind of his heroes played antecedent to and separate from any 'laws of philosophy'. Kepler and Newton added a binding conception not given by data — hence the error of Bacon and his ilk who supposed

> that to be done by method which must be done by mind; . . . that to be done by rule which must be done by a flight beyond rule; . . . that to be a work of mere labour which must also be a work of genius.[61]

Whewell's *History* made use of a model of the development of the sciences which charted their course as the progress of discoveries, typically attributed these key discoveries to the genius and inspiration of a few superior minds, and then showed the dissemination of these insights by painstaking education through the agency of research schools. Such a picture perfectly fitted the interests of Whewell's own reform campaigns within Cambridge schemes of disciplinary education. His survey covered all the discoveries we have discussed in this paper. Herschel's discovery of Uranus was carefully positioned as both a heroic individual discovery and also as the moment when the 'planet of Herschel now conforms to the laws of attraction'. To this account Whewell was compelled to add ever lengthier glosses on the contests for priority and status around the discovery of Neptune, and here again Whewell wrote of heroic intuition by such as Adams and Leverrier, together with the disciplined process by which

> the theory of gravitation predicted and produced the discovery ... the lives of many of the most acute, clear-sighted and laborious of mankind had been employed for generations in solving the problem.[62]

Lavoisier's chemistry was closely linked with his moral stature and profound intellectual insight, but the triumph of his views were also dependent on a 'school' in which 'the new chemistry was gradually formed'.[63] Coulomb's work, as we have seen, did not so rapidly grip the proponents of the new science of electromagnetism in Britain, and while Whewell allowed Coulomb heroic status, he demanded 'experiments more numerous and more varied', which 'would ... be a task of labour and difficulty'. With a glance at Faraday, Whewell suggested that 'the person who shall execute it will deserve to be considered as one of the real founders of the true doctrine of electricity'.[64] Analyses such as those of Whewell made it increasingly clear that the process of science was highly stratified, and that this stratification was part of the formation of the properly trained scientific mind.

Whewell spelt out the link between this stratification and his model of discovery throughout his career as an exegete of the sciences. In optics, his favourite obsession, the role of discovery and its link with discipline formation was always important. He wrote to John Herschel in 1818 that the new science of optics was obviously a 'rich field of discoveries': Herschel was

> treading close on the heels of Brewster, and, so far as I can make out, Brewster has got a long way ahead of Biot in the race of discovery which has been going on for some time.[65]

David Gooding has shown convincingly how the work of Herschel, Faraday and their colleagues on optics and electromagnetism in this period was explicitly seen in terms of emergent philosophies of discovery, where (in Herschel's terms) 'he who proves, discovers'. Gooding also argues that the interpretation of the meaning of discoveries, notably in the context of optical and electromagnetic work presented at the British Association, was itself part of the problem of disciplinary specialization in the 1830s and 1840s.[66] Whewell hammered home this point in his own address to the British Association in 1833. This address was principally aimed at a partisan account of the merits of the undulatory theory of light, based on its superior record of discovery and programmatic success. But Whewell also showed the connection between the necessarily *humble* role to be played by the Association's members and the necessarily *superior* attributes of discoverers. Stratification did its work in defining the scope of this new organization.

> If the Discoverers . . . the great men of the present and the past — if THEY might be elate and confident in the exercises of their intellectual powers, who are *we* that we should ape their mental attitudes?

Since, according to Whewell, 'we cannot create, we cannot even direct, the powers of discovery', it followed that 'we may take care that those who come ready and willing for the road shall start from the proper point and in the proper direction'. The contrast between the élite and the mere cultivators was the same as the contrast between discovery and disciplined training.[67]

The account of science which gave discovery and disciplinary training these associated roles was used by Whewell and his allies both in their account of history and in their account of teaching. In his *History*, Whewell gave a laudatory account of the undulatory theory, and re-emphasized the contrasting but necessary functions of discoverers and disciples. Members of the Cambridge network were hailed as a 'younger race of undulationists'. This 'body of men . . . trained in the British universities' in the techniques of mathematical physics 'incalculably benefited' the cause of scientific progress. When 'an abstruse and sublime theory comes before the world' these men could make it a disciplinary practice, and

> convert into a portion of the permanent treasure and inheritance of the civilized world, discoveries which might otherwise expire with the great geniuses who produced them, and be lost for ages, as, in former times, great scientific discoveries have sometimes been.[68]

He wrote in the same terms in his contemporary proposals for excellent university teaching. Students would become 'truly men rather than men of genius, which no education can make them'. Such 'true men' would learn history of science alongside mathematical physics. They would be able to make discoveries into disciplines, but not learn how to make discoveries. In the new nineteenth-century sciences, they would

> feel themselves called upon to sympathize with the struggles and successes, the hopes and the anticipations of the great men of their time, whose names and discoveries would be an inheritance to later generations.[69]

The distinction between discovery and justification made here had a social context in the construction of these new disciplines. I have argued that the end of natural philosophy was marked by the reification of heroic discoverers and prized techniques by these new research schools. I have also argued, however, that the deeply influential *historiography* of science, in which discoveries are viewed as unproblematic mental events with obvious marks of identity, was created at the same time, and thus accompanied the end of natural philosophy and the invention of modern science.

• NOTES

This is an extended and greatly revised version of my paper 'Scoperte scientifiche alla fine del XVIII secolo', *Materiali filosofici*, Vol. 12 (1984), 97–114. I am grateful for comments by Thomas Nickles, Steven Shapin, and an anonymous referee.

1. On discovery and textbook history, see T.S. Kuhn, 'The Historical Structure of Scientific Discovery', *Science*, Vol. 136 (1962), 760–64 reprinted in his *The Essential Tension* (Chicago: The University of Chicago Press, 1977), 165–77. A. Brannigan, 'The Reification of Mendel', *Social Studies of Science*, Vol. 9 (1979), 423–54; S. Woolgar, 'Writing an Intellectual History of Scientific Developments: the Use of Discovery Accounts', ibid., Vol. 6 (1976), 395–422; A. Pickering, 'Against Putting the Phenomena First: the Discovery of the Weak Neutral Current', *Studies in History and Philosophy of Science*, Vol. 15 (1984), 85–117.

2. For the Second Scientific Revolution, see Gaston Bachelard, *Le matérialisme rationnel* (Paris: PUF, 1953), Chapter 2; T.S. Kuhn, 'Mathematical versus Experimental Traditions in the Development of Physical Science', *Journal of Interdisciplinary History*, Vol. 7, (1976), 1–31; S.F. Cannon, *Science in Culture: the Early Victorian Period* (New York: Science History, 1978); E. Bellone, *A World on Paper: Studies on the Second Scientific Revolution* (Cambridge, Mass: MIT Press, 1980).

3. For the cultural meaning of *éloges*, see D. Outram, 'The Language of Natural Power: the *Eloges* of Georges Cuvier and the Public Language of Nineteenth Century Science', *History of Science*, Vol. 16 (1978), 153–78; C.B. Paul, *Science and Immortality: the Eloges of the Paris Academy of Sciences 1699–1791* (Berkeley, Calif.: University of California Press, 1981); J.R.R. Christie, 'Joseph Black and John Robison', in A. Simpson (ed.), *Joseph Black: a Commemorative Symposium* (Edinburgh: Royal Scottish Museum, 1982), 47–52; P.B. Wood, 'The Hagiography of Common Sense: Dugald Stewart's Account of the Life and Writings of Thomas Reid', in A.J. Holland (ed.), *Philosophy: its History and Historiography* (Dordrecht: Reidel, 1985), 305–22. For disciplinary histories, see J.S. Bailly, *Histoire de l'astronomie moderne*, 3 Volumes (Paris, 1779–1782); J.B.J. Delambre, *Histoire de l'astronomie moderne*, 2 Volumes (Paris, 1821); J.E. Montucla, *Histoire des mathématiques*, 4 Volumes (Paris, 1799–1802); T. Thomson, *History of Chemistry*, 2 Volumes (London, 1830–1831); H. Kopp, *Geschichte der Chimie*, 4 Volumes (Braunschweig, 1843–1847). For changes in philosophy, see L. Laudan, 'Why was the Logic of Discovery Abandoned?', in T. Nickles (ed.), *Scientific Discovery, Logic and Rationality* (Dordrecht: Reidel, 1980), 173–84; T. Nickles, 'From Natural Philosophy to Metaphilosophy of Science', in P. Achinstein and R. Kargon (eds), *Theoretical Physics in the 100 years since Lord Kelvin's*

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4. H. Reichenbach, *Experience and Prediction* (Chicago: The University of Chicago Press, 1938); comments in H. Siegel, 'Justification, Discovery and the Naturalizing of Epistemology', *Philosophy of Science*, Vol. 47 (1980), 297-321, on 309-13; T. Nickles, 'Scientific Discovery and the Future of the Philosophy of Science', in Nickles, op.cit. note 3, 1-59; M. Curd, 'The Logic of Discovery: an Analysis of Three Approaches', ibid., 201-19, on 209-12.

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6. Grmek et al., op.cit. note 5, 6.

7. E. McMullin et al., 'The Rational Explanation of Scientific Discoveries', in T. Nickles (ed.), *Scientific Discovery: Case Studies* (Dordrecht: Reidel, 1980), 21-49, on 28-33. For a more extended account of McMullin's views on the role of cognitive factors in history of science, see his 'The Rational and the Social in the History of Science', in J.R. Brown (ed.), *Scientific Rationality: the Sociological Turn* (Dordrecht: Reidel, 1984), 127-63.

8. N. Koertge, 'Explaining Scientific Discovery', in P.D. Asquith and T. Nickles (eds), *PSA 1982: Proceedings of the 1982 Biennial Meeting of the Philosophy of Science Association* (East Lansing, Mich.: Philosophy of Science Association, 1982), Vol. I, 14-28, on 14, 20, 26.

9. Kuhn, op.cit. note 1; B. Barnes, *T.S. Kuhn and Social Science* (London: Macmillan, 1982); A. Brannigan, *The Social Basis of Scientific Discoveries* (Cambridge: Cambridge University Press, 1981). For a comparison of historical analyses of discovery in the period covered by this paper, see J.B. Delair and W.A.S. Sarjeant, 'The Earliest Discoveries of Dinosaurs', *Isis*, Vol. 66 (1975), 5-25:

> Though a number of dinosaur bones had been discovered before Buckland and Mantell commenced their work, all had been misinterpreted and none contributed to the growth of scientific knowledge. These two scientists, therefore, must be considered the true originators of the study of dinosaurs.

Compare with Adrian Desmond, 'Designing the Dinosaur: Richard Owen's Response to Robert Edmond Grant', *Isis*, Vol. 70 (1979), 224-34:

> Talk of 'missed opportunities' tacitly assumes that science is a search for transcendental truths . . . which being everpresent, passively await the man perspicacious enough to recognize them. It is more profitable, however, to view science as a culture-bound, inherently creative activity'.

10. S. Schaffer, 'Uranus and the Establishment of Herschel's Astronomy', *Journal for the History of Astronomy*, Vol. 12 (1981), 11-26; R.H. Austin, 'Uranus Observed', *British Journal for the History of Science*, Vol. 3 (1967), 275-84.

11. Kuhn, op.cit. note 1; Brannigan, op.cit. note 9, 22-26, 129-33; Barnes, op.cit. note 9, 42-43.

12. C.E. Perrin, 'Prelude to Lavoisier's Theory of Calcination: Some Observations on *mercurius calcinatus per se*', *Ambix*, Vol. 16 (1969), 140–51; R.E. Kohler, 'Lavoisier's Rediscovery of the Air from Mercury Calx', ibid., Vol. 22 (1975), 52-57; F.L. Holmes, *Lavoisier and the Chemistry of Life: an Exploration of Scientific Creativity* (Madison, Wis: University of Wisconsin Press, 1985), 41–51.

13. Antoine Laurent Lavoisier, 'La nature du principe qui se combine avec les métaux pendant leur calcination et qui en augmente le poids', *Observations sur la Physique*, Vol. 5 (1775), 429–33, cited in Holmes, op.cit. note 12, 48–49. For traditional accounts of this episode, see J.B. Conant, 'The Overthrow of the Phlogiston Theory', *Harvard Case Histories in Experimental Science* (Cambridge, Mass.: Harvard University Press, 1954), Vol. I, 67–115; A. Musgrave, 'Why did Oxygen Supplant Phlogiston?: Research Programmes and the Chemical Revolution', in C. Howson (ed.), *Method and Appraisal in the Physical Sciences* (Cambridge: Cambridge University Press, 1976), 181–209.

14. Koertge, op.cit. note 8, 19.

15. J.B. Gough, 'The Origins of Lavoisier's Theory of the Gaseous State', in H. Woolf (ed.), *The Analytic Spirit: Essays in History of Science in Honor of Henry Guerlac* (Ithaca, NY: Cornell University Press, 1981), 15–39; Holmes, op.cit. note 12, 31–33.

16. Holmes, op.cit. note 12, 53–54.

17. Antoine Laurent Lavoisier, 'Mémoire sur l'éxistence de l'air dans l'acide nitreux, et sur les moyens de décomposer et de recomposer cet acide', *Mémoires de l'Académie Royale des Sciences* (1776, published 1779), 671–80, on 679; Holmes, op.cit. note 12, 58; W.C. Anderson, *Between the Library and the Laboratory: the Language of Chemistry in Eighteenth Century France* (Baltimore, Md: Johns Hopkins University Press, 1984), 89, 170. For a comparative account of chemical language, see J.R.R. Christie and J.V. Golinski, 'The Spreading of the Word: New Directions in the Historiography of Chemistry, 1600–1800', *History of Science*, Vol. 20 (1982), 235–66. Lavoisier's reconstructed discovery story is in his 'Refléxions sur le phlogistique, pour servir de développement à la théorie de la combustion et de la calcination', *Mémoires de l'Académie Royale des Sciences* (1783, published 1786), 505–38, on 511.

18. Anderson, op.cit. note 17, 149–51.

19. J.L. Heilbron, *Electricity in the Seventeenth and Eighteenth Centuries* (Berkeley, Calif.: University of California Press, 1979), 462, 469–73.

20. D. and D.H.D. Roller, 'Development of the Concept of Electric Charge', in Conant, op.cit. note 13, Vol.II, 543–69, and W.J. King, 'Quantification of the Concepts of Electric Charge and Electric Current', *The Natural Philosopher*, Vol. 2 (1963), 107–27, are revised in Heilbron, op.cit. note 19, 473–77; J. Dorling, 'Cavendish's Deduction of the Inverse Square Law from the Result of a Single Experiment', *Studies in History and Philosophy of Science*, Vol. 4 (1974), 327–48; C.S. Gillmor, *Coulomb and the Evolution of Physics and Engineering in France* (Princeton, NJ: Princeton University Press, 1971), Chapter 6. Coulomb uses the concept of 'electric mass' in 'Mémoire où l'on détermine suivant quelles lois le fluide magnétique ainsi que le fluide électrique agissent,' *Mémoires de l'Académie Royale des Sciences* (1784, published 1785), 578–611, on 610–11.

21. The 'local rationality of the battlefield' is mentioned by R.S. Westman in 'The Rational Explanation of Scientific Discoveries', op.cit. note 7, 44.

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J.G. McEvoy, 'Joseph Priestley: Aerial Philosopher', Part 3, *Ambix*, Vol. 25 (1978), 153–75.

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> It is possible to lay down accurate rules, as Bacon has done, for the performing of that part of the inductive process which all men perform alike; but. . .these rules, though accurate, are not wanted, because in truth they only tell us to do what we are all doing. We think that it is impossible to lay down any precise rule for the performing of that part of the inductive process which a great experimental philosopher performs in one way, and a superstitious old woman in another.

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63. Ibid., Vol. III, 114–23, on 116, 119.

64. Ibid., Vol. III, 24–33, on 29. Whewell used W. Snow Harris, 'On Some Elementary Laws of Electricity', *Philosophical Transactions*, Vol. 124 (1834), 213–45, and 'Inquiries Concerning the Elementary Laws of Electricity,' ibid., Vol. 126 (1836), 417–52. For the use of Coulomb against Faraday, see G.B. Airy, in H. Bence Jones (ed.), *Life and Letters of Michael Faraday* (London, 1870), Vol. II, 353.

65. Whewell to John Herschel, 1 November 1818, in I. Todhunter, *William Whewell, DD* (London, 1876), Vol. II, 28–29; see G.N. Cantor, *Optics after Newton: Theories of Light in Britain and Ireland 1704–1840* (Manchester: Manchester University Press, 1983), 159–66, 173–77.

66. D. Gooding, ' "He Who Proves, Discovers": John Herschel, William Pepys and the Faraday Effect', *Notes and Records of the Royal Society*, Vol. 39 (1985), 229-44. Compare F.A.J.L. James, ' "The Optical Mode of Investigation:" Light and Matter in Faraday's Natural Philosophy', in Gooding and James (eds), *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday* (London: Macmillan, 1985), 137-61.

67. William Whewell, 'Address', *Report of the Third Annual Meeting of the British Association for the Advancement of Science* (London, 1834), xxiv, xii; Yeo, op.cit. note 50, 68; J.B. Morrell and A. Thackray, *Gentlemen of Science: the Early Years of the British Association for the Advancement of Science* (Oxford: Oxford University Press, 1981), 270-75; there was an important and revealing dispute at the BAAS in 1840, involving Harcourt, Babbage, Murchison and Arago, on the priority in the discovery of the composition of water, for which see Morrell and Thackray, op.cit., 215.

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69. William Whewell, *Thoughts on the Study of Mathematics as a Part of a Liberal Education* (Cambridge, 1835), 45-46; idem, *On the Principles of English University Education* (Cambridge, 1837), 14-15; idem, *Of a Liberal Education in General* (London, 1845), 107. For a comparative perspective on educational strategies in Cambridge and Scotland, see D.B. Wilson, 'The Educational Matrix: Physics Education at Early Victorian Cambridge, Edinburgh and Glasgow Universities', in Harman, op.cit. note 51, 12-48.

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