Brannigan creates interest in his subject from the outset by illustrating how scientific discovery has been associated with the bizarre and irrational. The myths of Daedalus, Midas, the novels of Huxley, Butler and Zamiatin, the Frankenstein, Jekyll and Hyde and Faustus stories as well as the 'mystical' spurs to discovery reported by Einstein, Kekule and Wallace, not to mention Newton and the apple, all serve to generate a view of discovery as irrational. A view firmly confronted by twentieth century positivism. Reichenbach, and later Popper, clearly drew a line of demarcation between the context of discovery and the context of justification. A positivist doctrine has evolved that dumps the context of discovery in the dustbin of empirical psychology and concentrates on the rational reconstructions of the context of justification. For Popper, there is 'no such thing as a logical method of having new ideas', nor a logical reconstruction of this process. A position reflected by Braithwaite (1960) who argued that the psychology and sociology of discovery were no business of the historian or philosopher of science whose concern should be with the objective arguments in the justification of change in science. Popper's notion of 'conjectures and refutations' became an injunction about how scientific thinking ought to be done. Ironically, Brannigan argues, the positivistic concentration on the justification process reinforced the view that discovery was irrational. This also led to a rift between the normative prescriptions of the logical empiricists and the generalisations of empiricist historians of sciences concerned with how research had actually been conducted. Thus Kuhn was condemned for presenting advances in science as the outcome of a kind of 'mob hysteria'. The aggressive stand adopted by logical empiricists has created the illusion that there is a synonomy between the logic of discovery and the actual process of discovery, despite specific denials by scientists involved. (See for example Einstein's reply to Reichenbach reported in Holton (1973) p 278 and the commentary by Watson on the discovery of the double helix, Watson, (1968).)

On the contrary, Brannigan argues, the concern of the sociology of knowledge ought to be to distinguish between the philosophical adequacy of reconstructed knowledge claims and the explication of the conditions under which discoveries occur. These are separate domains, and Brannigan is primarily interested in the latter. Brannigan proceeds by briefly dispensing with the normative prescriptions of those he identifies as (Reichenbachian) positivists and the opposing views of the likes of Feyerabend and Holton. What the upshot of this confrontation is, according to Brannigan, is the revelation that discovery and justification are interrelated and not independent elements of the production of scientific knowledge. This is confirmed in general by a somewhat tortured reference to the simple concept of the theory-laden nature of observation and reinforced, empirically, by reference to certain case studies. (Galileo and his telescope, the thematic asymmetry of Einstein's equations, etc.). Apart from this, Brannigan considers the positive versus anti-positive debate as merely hinging on conflicting normative prescriptions and therefore futile.

Brannigan proceeds then to outline two basic approaches to an assessment of how discoveries occur. The predominant approach is one which treats discovery as the 'outcome of some earlier mental condition'. Such 'mentalistic' or 'naturalistic' approaches posit discovery as a kind of natural phenomena whose presence is nomethically linked to other phenomena. Discovery,
according to this approach will inevitably occur. The naturalistic models are of two types, the psychologistic and the cultural. This latter type treats discoveries as a function of cultural growth, and may be glibly summed up in the cliche 'Necessity is the mother of invention'. Psychologistic or mentalistic elements are construed as contingent on the cultural context. The second approach suggests that the focus of enquiry should be on explaining how discoveries are constituted, rather than how they occurred to individuals. The 'attributional' approach is concerned with the process by which the status of 'discovery' is conferred on social events.

Brannigan proceeds to investigate the naturalistic models of discovery provided by Hanson, Blackwell, Kuhn and Koestler. These reviews are highly selective, concerning themselves only with those parts of more general models of the production of scientific knowledge that deal with discovery. The reviews are thus somewhat inadequate as they gloss over more complex aspects that are integral to a fuller understanding of the models of knowledge production, as, for example, the assessment of the mechanism of change in Kuhn's paradigm thesis. By concentrating on discovery, Brannigan misses the crux of these various theories and examines elements out of context. As will be argued, this severely restricts the scope of his critique for sociologists concerned with the production of scientific knowledge.

Brannigan objects to the reductionism of the naturalistic models, which merely offer accounts of the discoveries by showing how researchers, through interaction with their 'environment' get 'new' ideas. They differ, Brannigan argues, only in terms of the controlling variable, which ranges from 'gestalt switch', through 'retroduction' and 'insight' to 'luck'. Detail apart, all reductionist accounts are insufficient in principle. While the controlling variable may be necessary for scientific discovery it is not sufficient. At one level, insight, for example, does not imply discovery in the sense of scientific advance, (a child, whilst learning, experiences insight), and at another level, the control variables apply equally to non-scientific realms (humour, art, literature) and thus there is nothing in them that identifies what is unique about discovery.

In effect, at a substantive level, the mentalistic models confuse learning and discovery. Scientific discovery thus becomes absorbed by the realm of general psychology. The reductionist accounts ignore the different social factors that differentiate learner, the forrunner, discoverer and so on. By concentrating on the mental correlates, the reductionist approach obscures the social statuses attached to events identified as discoveries, thus saying little, or nothing, about the occurences of discoveries or why they were recognised as such. The same mentalistic processes may lead to errors but their social status is quite different. Brannigan opposes the mentalistic approaches because the invocation of control variables like 'gestalt switch', 'retrospection', 'anomaly' and so on, simply provide a descriptive element to discovery (implying construction over time of a discovery) rather than providing an explanatory (causally adequate) variable.

Although he does not say so, Brannigan implies that the control variable cannot be measured, or even identified, and that it renders the reductionist approaches inadequate. Thus nomothetic models of discovery are unable to live up to their own claims. Furthermore the historical reconstruction of discovery retrospectively attributes 'genius', 'wit', 'insight' etc.. Mentalistic accounts, therefore, are post hoc accounts. The argument becomes circular. In retrospect, a discovery is seen to have taken place, so genius (or another variable) is seen as a necessary prerequisite, and consequently attributed in a post hoc fashion.
Brannigan is little more impressed by the cultural view of discovery as espoused by Ogburn, D. Thomas, and White. This view maintains that a social context is created that is ammenable to discovery. The 'cultural determination' of discovery and advance of science is 'verified' by the large number of multiple coincidental discoveries. This thesis was originally developed by Ogburn and Thomas and later extended by Merton. Brannigan casts doubt on both the actual number of real coincidental discoveries and the relative number of such discoveries. Merton's development is to posit an institutionalisation of discovery—i.e. the identification of a discovery as an event—thus the sociological account of discovery turns on the processes whereby certain events are determined socially to be discoveries. Brannigan, however, wants to go beyond Merton and, himself, advocates an approach aligned with the 'strong programme' perspective developed by Bloor, Barnes and Mulkay, among others. Brannigan regards this approach as cognitive rather than simply normative, the label he attaches to Merton's work. Rather than be concerned with the abnormal in science, Brannigan argues that the cognitive approach is concerned with the prosaic. The normative approach of Merton, for example, sees social influences as extra-scientific, as providing explanations for peculiarities in science. Thus, studies have been made of why Lysenko should have kept Mendelian genetics at bay in the U.S.S.R., why Galileo was persecuted by the Roman Catholic church, and so on. Brannigan's concern, like the supporters of the 'strong programme', is to:

articulate the social basis of discoveries not through an examination of 'social influence', but through a phenomenological approach that posits that all phenomena endemic to scientific research are socially constituted and identified - not in the sense of being 'swayed' by extra-scientific facts, but in the sense of being constructed by members of society as 'scientific' in the first place. (Brannigan, 1981, p. 64).

This is true above all with discovery, says Brannigan, and proceeds to outline a model of how members of society 'confer the status of scientific discovery on candidate events by virtue of criteria of intelligibility learned as part of our common stock of knowledge about the world.' (Brannigan, 1981, p. 64).

The scientist's discovery must be inspected not for its content or psychological origins, but for the context that makes it a possibility or a candidate in the first place. Discoveries, he argues, are discoveries because there are contexts that provide a designation of activity as discovery. Discoveries can only occur if there is an explicit common-sense practice seen as science.

Brannigan supports the thesis which he attributes to Winch's reconstruction of Wittgenstein, that the 'intelligibility of the world is an attribution of our conceptual makeup', which is built into our language and becomes taken-for-granted or 'natural' as we are socialised into culture. For Winch, the world consists of what is presented in linguistic concepts. Thus Brannigan says that sociologists should be concerned with the meaningfulness that animates human behaviour. Referring to discovery, by viewing it as a meaningful act, Brannigan argues, we should be able to 'inspect the criteria by which it is defined, recognised and constituted by members of society'.

Brannigan identifies four criteria, in what he calls the attributional model, that concentrate on the notion of 'making a discovery'. Discovery is not, he argues, about how ideas come into the mind
but about how they come into society. The discovery of America is attributed to Columbus despite earlier finds because society did not recognise the earlier discoveries as such. These earlier 'discoveries' are so labelled only retrospectively. Brannigan points to the 'N rays' as an example of short-lived social discovery. They were perceived as discoveries by the scientific community (or at least part of it) because they were possible, motivated achievements that were substantially true or valid and whose announcements were unprecedented. Even if anomalies or 'gestalt switches' are evident in such discoveries they are secondary to the common-sense grounds that constitute intelligible criteria for discovery.

Brannigan argues that the 'possibility' of a discovery inheres in two factors: the anticipation of the discovery by society and that the discovery was the result of work conducted within a research framework. Darwin's evolutionary theory was in the air and thus acceptable, by some of the scientific fraternity. Einstein's work on relativity was so unprecedented that eminent thinkers, such as Russel and Whitehead resisted it. Similarly the use of a gyroscope as a toy spinning top was an inadequate environment for its 'discovery' as an aviation guidance system.

Thus, argues Brannigan, discovery may be differentiated from learning or fraud because the motivational element is different. Accidental discoveries do not take place except in the sense of being 'surprise' developments in a research programme.

In attributing discovery, 'validity' is a 'folk' accomplishment. The very fact that there are priority disputes shows that discoveries do not happen but are socially constructed. Discoveries are socially recognised as such, and this recognition depends upon the identification of a research context, a motivated researcher, the demonstration of substantially valid results that are new and unprecedented. Any potential discovery that fails to live up to these criteria will be disqualified, i.e., will not be regarded as a discovery by society.

What has Brannigan provided by these observations? He seems to be saying nothing more than that discovery is socially constructed and that there are procedures that operate to allow some such potential discoveries and disallow others. Some of these procedures are anticipation, research framework and validity demonstration. However there are others that effect the acceptability in terms of 'validity' criteria which are not evident but operate at a common-sense level. These factors will be encompassed by a catch-all category of 'folk law'. Brannigan is aware that his position is relativistic. There follows a tortured and inadequate analysis of the problem of relativism in metascience, which effectively leaves Brannigan saying that while he is loathe to admit that all knowledge is socially constructed and a function of its social context social scientists proceed as though it were in order to develop a sociological analysis of knowledge production. By way of legitimating his position, Brannigan refers to the relativism of Barnes (1974) and Bloor (1976) who adopt such a position when questioning the validity of natural scientific beliefs as 'true' belief. Instead, they argue, sociologists of knowledge should consider as knowledge that which people take as knowledge.

Brannigan takes up this position (which ultimately leads him in the direction of an ethnomethodological analysis) and uses it to confront what he calls the reason-cause controversy. He argues that reason and cause are interchangeable in explanatory accounts. His analysis of cause hints at major problems, not least its adequacy when removed from the inanimate physical
world. However, Brannigan takes-for-granted the adequacy of causal analysis in physics. He also assumes that the transposition of cause to typification when dealing with categories of human action is unproblematic. Brannigan's analysis of cause is relatively weak and, despite his claim, does not circumvent the teleological problems presented by equating cause with reason.

Brannigan maintains that he has demonstrated the independence and causal status of the attributional model (necessary criteria for an adequate model, as he sees it). This is not the case, though, and all he has shown is that in attributing discovery status to an event a set of criteria of intelligibility exist prior to the attribution, and that the fixing of the attribution of the status of discovery is a socially constructed act and that a host of alternative statuses could have been attached. In short, all he has demonstrated is that there is nothing inherent in discovery. Brannigan ignores the whole process of legitimization or persuasion that 'apprehends' the pre-existing criteria and distorts them to allow discovery status to be conferred. In other words, he has not really shown that the criteria exist prior to discovery in a clear deterministic way and that a dialectical relationship obtains between criteria of intelligibility and conferral. In short, the criteria do not constitute a prior objectivity, but are reconstructible. Brannigan's problems really arise in that he treats discovery as object not process. While he has established a point in denying the reductionist view of the discovery process, to ignore the complex 'external' and dialectical processes involved in the social conferral of discovery and to treat the social event as a 'social fact', albeit within a phenomenological framework, is to miss the point. Brannigan's treatment is 'astructural' in the sense of concentrating on the specific, ahistorical, components of conferral of discovery status. His model is simply a typification of those aspects identifiable as effecting common-sense judgements. It is a model that derives from, and is validated by reference to particular case studies, which, despite his avowed intention, are hardly prosaic moments in the history of science. Mendelian genetics is one of Brannigan's 'telling illustrations of the attributional model of discovery'. What this amounts to is to deny the various notions of Barber (1961), Eiseley (1961), Gasking (1959), Glass (1953), Hardin (1959), Merton (1973), Boring (1963), and Stent (1978), among others, who variously argue that Mendel was ignored because of the obscurity of his publication, the difficult mathematics used by Mendel, the low status of Mendel as researcher, the prematurity of the problem, the misunderstanding of the results, and the resistance of the scientific community to new work.

Brannigan argues, contrary to the commonly held belief that Mendel was the originator of genetics whose 1866 contribution got distorted or lost and was subsequently rediscovered simultaneously by several researchers, that Mendel was not the founder, nor was he overlooked in 1866, nor was he rediscovered as a result of independent enquiry in 1900. Brannigan argues that the content of Mendel's work remained the same but that its reception and meaning changed in different contexts and that the rediscovery (and reification) of Mendel was the result of a priority dispute between Correns and De Vries. Furthermore, to emphasise the very different light in which Mendel's work was placed in 1900 to that of 1866, Brannigan points out that, in Britain, Mendel was incorrectly used to provide support for the inheritance (mutationist) model of evolution in its dispute with the biometrical model.

Brannigan points to the use the 'Mendel legend' has been put to in normative positive reconstructions of the development of science. Despite a superficial appearance that would support the thesis that the work of 1866 and the reception accorded it in 1900 constitute the
context of discovery and the context of justification respectively, the two episodes 'were altogether different achievements for the parties involved not separate parts of the same episode as has been suggested' (Brannigan, 1981, p. 115).

Similarly Brannigan takes up the idea of multiple discovery and shows that the rediscovery of Mendel as a multiple occurrence was (like so many other so-called multiples) far from straightforward and the 'Mendel legend' provides only a superficial and shaky case in support of the multiple discovery thesis. The same may be said of theories that suggest that the 'fame' achieved by good ideas is a function of the social setting. The Mendel legend does not provide the clear example it is supposed to of a scientist out of step with his milieu. Mendel's ideas were not fundamentally at variance with the prevailing 'world view', but rather, in 1866, his work was not seen as revolutionary, as it was so reified in 1900.

Brannigan's other in-depth case studies are the discovery of oxygen, of America and of the Piltdown Man. In the case of the latter, the 'find' was attributed various statuses from 'discovery' through 'fraud' to 'hoax'. Each interpretation of the event attributed to it something of an implicit misunderstanding of how it was brought about, the integrity of the researchers, the likelihood of their success, the continuity of the work, its probable importance, and so on. In short, the assumption that the event was a discovery or a fraud or a hoax in itself constituted an interpretive framework and was used to explicate the relevant factual circumstances assumed to be co-present with the status of the find.

Here, Brannigan has an opportunity to develop an analysis that looks at the interrelation of the conferral of status and the legitimating processes, particularly through an analysis of ideology. However, he sidesteps this problem and settles for an 'astructuralistic' account that merely hinges on the value-laden nature of observation. The ascribed status serves to reconstruct 'observational fact'. (The revision of status in the case of the Piltdown Man, of course, was also prompted by observational inconsistencies, which took on meaning in the light of other 'external' events). The ahistorical nature of Brannigan's model is highlighted by his unsatisfactory suggestion that the attribution of status (e.g. discovery), is the result of past details being transformed by present knowledge. Superficially, this appears to be a mediation of traditions, but is nothing more than the imposition of the present on the past, effectively a utopian historicist position (commonly referred to as the 'Whig' interpretation of history). Brannigan really takes no account of the process of history writing. The attribution of status to potential discovery events is ultimately self-evident in his model, grounded as it is in 'folk decisions'.

Brannigan's claim that science has an ethnotheoretical (folk theory) element that distorts at the metascientific level is rather more polemical than analytic and is unconvincing primarily because of the failure to analyse the ideological component of such ethnotheories. Brannigan's analysis of ideology amounts to nothing more than a discussion of ideology as synonymous with a naive notion of 'political ideology', which serve solely to 'legitimate the social positions of the individuals' who develop the ethnotheories. Brannigan simply maintains that ethnotheories are erroneous and their impact is to distort. Ideology is thus viewed simply as false consciousness, and the concept becomes useless as an analytic tool.

The use of a small number of examples tends to make the book rather repetitive when read
straight through, as does his constant reiteration of a fairly simple idea. Brannigan maintains that the phenomenon of discovery is a substantive event for members of society, although for sociologists discovery should be seen as a method of identifying and elaborating the complexity of the production of scientific knowledge.

To conclude, he asks, though, how the status of an event as a discovery is maintained and evolved. He suggests, following the ethnomethodological work of Garfinkel, Sachs and Cicourel, that the 'documentary method' is in evidence and that members’ accounts provide a reconstruction informed by a 'deeper structure' of interpretive practices. Brannigan goes on to review, once again, the cases of Mendel, Piltdown Man and the discovery of America and show how, in the development of each, a changing interpretation was produced that, at each phase, involved a different set of assumptions, prescriptions and expectations. Different relevancies were constructed and the formulation about the discoveries were accountable. However, this says nothing more than that the status of discovery is attributed, discovery is not an objective state, that the maintenance of discovery is a 'common-sense' accountable, process and thus necessarily unique to each event or sequence of events. The resulting 'discovery' has a history reflected in its status.

While Brannigan's analysis provides a valuable overturning of the taken-for-granted objectification of discovery, the revelation is of little help in the advancement of metascience. Discovery, in being contextualised at an etnomethodological level is decontextualised at a metascientific one. There is no potential in Brannigan's account for the analysis of the role of 'discovery' in the process of the production of scientific knowledge precisely because 'discovery' is divorced from structural concerns by a failure to construe the links between status attribution, myth construction and ideology. Ignoring the historical process, and the analysis of myth inherent in retrospective accounts grounded in 'common-sense', leaves Brannigan's model firmly rooted at a descriptive astructural level.

Mulkay's prefatory remark that this books leads the sociological study of discovery in an intriguing new direction turns out to be an overstatement of the impact of the work. One that, in all fairness, goes beyond the modest aim of the author, which is to explode the myth of the objectification of discovery, to advance the 'sub:-discipline' of the sociology of knowledge, (with no real attempt to relate to metascience, especially the process of the production of scientific knowledge) and propose a simplistic programme for further research as the basis for examining the attribution of discovery.

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