How Classification Works

Nelson Goodman among the Social Sciences

edited by
MARY DOUGLAS and DAVID HULL

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The sub-title, *Nelson Goodman among the Social Sciences*, may need explaining. It is not chosen because this book is by social scientists. Admittedly Gary Downey, Paul Starr and Mary Douglas are social scientists in the ordinary sense, but the other contributors are using Goodman's work for scrutinising collective efforts at classifying. David Hull is a philosopher-biologist looking at anthropology, Ian Hacking looks at social workers and the family, Ruth Katz is writing as much about history as about music, and Timothy Engstrom's questions about style and ideology are right at the centre of social criticism. The result is a volume about Goodman's impact on the social sciences in the largest sense of the word.

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Agency and Structure in Negotiating Knowledge

GARY L. DOWNEY

Relativist, constructivist and reflexivist approaches to the sociology of scientific knowledge are faced with a problem that Nelson Goodman grapples with in seeking a relativist philosophy of knowledge. The problem derives from the emphasis each places on the role of agency in processes of knowledge development. Although providing a welcome corrective to the determinism of much structure-based analysis,[1] these agency-based interpretations of knowledge change have too often been a dialectical over-reaction, identifying with their predecessors by inverting their limitations.

The sociological approaches contribute to what Goodman calls a 'mainstream' transition in epistemology 'from static absolutism to dynamic relativism' (Goodman 1986: 19). From this perspective, the heretofore dominant image of scientific action as shaped by the prior constraints of cognitive and social structures has been replaced by its polar opposite, the view that scientific action is constantly being reshaped by contingent sociocognitive practices. As Goodman puts it, knowing 'is conceived as *developing* concepts and patterns, as *establishing* habits, and as *revising* or *replacing* the concepts and *altering* or *breaking* the habits in the face of new problems, needs, or insights' (1986: 19; emphasis added). Individual actors are no longer devalued as vehicles for reproducing structures but have been reconceived as autonomous agents and moved to centre stage as the source of heterogeneous, constitutive practices. The primary analytic goal is to identify and describe how diverse individual choices and judgments combine to effect knowledge change. Yet by viewing knowledge change as the product of contingent practices, this perspective has difficulty accounting for continuities in knowledge structures and for the linkages that occur between change and continuity. It would be implausible to expect, for example, that congeries of contingent practices produced by autonomous agents would yield continuities of structure on a regular basis.

In this paper, I show that agency-based approaches typically treat change and continuity asymmetrically as qualitatively distinct and separate mechanisms. These approaches then de-emphasise the mechanism of continuity in favour of the mechanism of change. By thus opposing change and continuity, this strategy leads to the view that actors need to make an explicit choice between the two. I maintain that posing an asymmetry of this sort produces implausible models of actor judgments. These models tend either to reduce actors to schizophrenics who divide their time between contradictory decisions to constitute practices or reproduce structures, or to treat them as black boxes that mysteriously resolve the competing predispositions.

I elaborate this argument by comparing the asymmetry between world-making and habit in Goodman's philosophical relativism with those in two sociological programmes. The first is the asymmetry between negotiation and cultural institutions in Harry Collins' empirical programme of relativism. The second is the twin asymmetries of negotiation and resource manipulation and of reflexivity and analysis in Bruno Latour's and Steve Woolgar's constructivist/reflexivist study *Laboratory Life*. I then sketch an alternative model of actor judgments that accepts most of the claims that agency-based theorists make about cognition but also seeks to treat change and continuity more symmetrically as integral parts of every judgment.

World-making and Habit

By claiming disinterest in the metaphysical character of world-making and explicitly focusing on its instruments and mechanisms, Nelson Goodman champions an agency-based approach to knowledge change. He describes 'composition and decomposition', 'weighting', 'ordering', 'deletion', and 'supplementation' (1978: 7–16), for example, as all instruments that world-makers manipulate in processes of 'inquiry' and 'invention' (1978: 128), of 'frustration' and 'initiative' (1978: 136, 131), as they 'fabricate facts', exhibit 'styles', and 'modify traditions' (1978: 91–107, 23–40, 138). The scientist provides clear instances of world-making, for he 'as much decrees as discovers the laws he sets forth, as much designs as discerns the patterns he delineates' (1978: 18). The products of constitutive practices are ephemeral structures. Science itself is reconceived on the model of an amorphous living organ. It is 'a mobile unsteady structure . . . with all the bits moving about, fitting together in different ways, adding new bits to themselves with flourishes of adornment as though consulting a mirror, giving the whole arrangement something like the unpredictability and unreliability of living flesh' (1986: 6). Science's most distinctive characteristic is constant change, which practitioners achieve through diverse world-making practices.

The changes are not the product of random agency, however, for the contingencies of world-making practices are actively constrained by the presence of previous worlds. 'Worldmaking as we know it', Goodman writes, 'always starts from worlds already on hand—the making is a remaking' (1978: 6). An existing world serves as a frame of reference by providing a set of entrenched symbolic categories. World-makers do not have the option to disregard entrenched categories, for these provide criteria of rightness that the wider community uses to evaluate proposed modifications. In order for changes to be accepted as right, they must 'fit', or be consistent with, pre-existing categories (1978: 138). Randomness has no place: '[W]e no more make a world by putting symbols together at random than a carpenter makes a chair by putting pieces of wood together at random' (1978: 94).

But though every process of change is constrained by a structure of entrenched categories, world-making does not therefore become structured change, as a structure-based approach might have it. Nor does it involve a systematic interpenetration of change and continuity. Rightness of categorisation serves Goodman's framework only as a boundary constraint on change. It provides a sharply limited mediator between change and continuity. As I elaborate below, 'world-making' refers only to change, in the sense of bounded practice. The continuity of entrenched categories over time is achieved by an entirely different process—habit.

Unlike world-making, habit is not presented as a symbolic phenomenon. This is important because Goodman explicitly calls his philosophy an 'analytic study of types and functions of symbols and symbol systems' (1978: 5) rather than a study of beliefs. The book *Languages of Art*, for example, is subtitled *An approach to the theory of symbols* (1968). Indeed, the term 'habit' is occasionally replaced by 'habitual', which might be descriptive of a public symbol system if it referred only to the frequency with which categories appeared in world-making processes (e.g. 1955: 87–120; 1978: 101). But far more is indicated, for Goodman regularly presents habit as a seeming universal predisposition among actors, even a generic psychological characteristic, and clearly one to avoid or overcome.

The greatest threat to a functioning world-maker, for example, is the ever-lurking 'grip of habit' (1978: 97). Being gripped by habit means that one lacks the 'determination and skill' of good world-makers and exhibits 'inertia' rather than 'initiative' (1978: 97, 131). Habits become something we fall or recede into if we are not careful. They keep us 'stuck with' some old version or world until we can remake it into a new one (1978: 97, 9). And knowing is actually conceived in contrast to habit, for it consists of 'establishing' or 'breaking' habits but not of following them (1986: 19). In sum, habit stands outside the core of Goodman's philosophy of bounded practice in world-making, tacked on underneath to account for the fact of continuity.[2]

Treating habit asymmetrically as a predisposition distinct from world-making weakens both the symbolic account of how actors effect change and the psychological account of how they produce continuity. Rightness of categorisation provides only a boundary constraint on world-making in the sense that it cannot account for the particular symbolic choices that communities of world-makers make. Harry Collins makes this point in contesting Goodman's claim that grass (or an emerald) is green rather than grue because green is an entrenched, projectible category and grue is not (Collins 1985: 9–12). Collins points out that when the community made the judgment that now makes grass appear green, it not only chose between entrenched and non-entrenched categories but also made selections among sets of well-entrenched categories. There are many such categories whose application to grass would 'fit' and produce equally 'right' projections. Grass reflects all sorts of wavelengths of light, is visibly a variety of colours, and even appears black at night. Yet the community selected green alone.

According to Collins, rightness of categorisation may have solved the philosophical problem of induction by showing why the eventual solution was an acceptable, or 'right', one, but it raised and did not solve the sociological counterpart: accounting for how a particular selection was made from the pool of acceptable choices.

Sociologically, the choices that actors face in effecting knowledge change are much more limited than a survey of the unbounded sets of 'right' projections would suggest. The primary indicators of this limitation are continuities in social action. Unless randomness does in fact rule, every process of change necessarily *includes* some kind of continuity. Every new scientific theory, for example, reproduces a variety of assumptions, including theoretical and methodological categories and an understanding of theory-building as a legitimate, epistemologically significant enterprise. These continuities are integral to the meaning of any particular theory change. It is true, as Goodman implies, that knowledge producers may elect to be adventurous or conservative or something in between. But a completely radical expression of agency that achieves no continuities would marginalise itself as simply meaningless. The particular selections that knowledge producers make are not choices *between* change and continuity but are choices *among* particular combinations of changes and continuities.

'Rightness' is arguably an important dimension of continuity in knowledge. But in order to provide a symbolic account of any specific change in knowledge, one must identify other continuities and account for their relationships both to one another and to the change as dimensions or moments of a common symbolic phenomenon. Beyond rightness of categorisation, Goodman refers only to an 'interplay' between the qualitatively distinct mechanisms of world-making and habit (Goodman 1986: 17). But this idea of interplay takes us beyond the world of public symbols into a weak psychology. That is, it requires us to explain the selections that actors make among right options wholly in terms of their varying willingness to either passively follow habits or actively make new worlds.

Furthermore, there is arguably more to continuity of knowledge than the sum of the contrasting processes of rightness of categorisation and habit. It is true that any account of knowledge continuities must recognise that actors often reproduce right categories automatically, without making explicit judgments. However, it is also important to remember that continuities are indeed often a matter of judgment. Since specific changes always involve continuities in entrenched categories, a choice among instruments of change is frequently an explicit choice among which continuities to achieve in entrenched categories. From the point of view of an agency-based theory, it would be more consistent to treat the automatic reproduction of categories as an extension, extrapolation, or special case of judged continuities in categories rather than as a qualitatively distinct process. But Goodman's philosophy has drawn a sharp distinction between the automatic psychological reproduction of habit and the continuity of 'rightness' that occurs as an implicit component of explicit change, and it provides no model at all of explicitly and systematically judged continuities.

Why not then discard the concept of habit and reconceive rightness of categorisation to encompass all cases of change and continuity? That is, rather than viewing actors as either passively following habits or actively making worlds, what if we view actors as simultaneously reconstituting *and* reproducing previous worlds by rightly projecting entrenched categories? Although potentially attractive to social scientists, this option is probably not acceptable to philosophers. It necessarily involves us in systematically comparing the features of actor judgments that function to compose, decompose, reweight, etc., existing categories with the features that simply reproduce them. But such comparisons edge the analysis out of the epistemology of world-making and into a social theory of actor judgments. The concept of habit made it possible for Goodman to leave individual decision-making out of the analysis even while adopting an approach that emphasises the actor's manipulative ability. Yet a more developed theory of actor judgments than the bidimensional view that actors either make worlds or follow them is needed to describe the relationship between moments of agency-driven change and structure-driven continuity. Goodman has already pushed philosophical epistemology past its limits by expanding cognition to include emotion and aesthetics. It is perhaps too much to ask him to push past philosophy as well and produce a theory of actor judgments, that is, of the ways of worldmakers. But the cost of excluding an account of routine relationships between change and continuity in actor judgments is, I maintain, to sacrifice his larger goal of achieving a general theory of knowledge change that was begun with right categorisation.

Negotiation and Cultural Institutions

Like Goodman's philosophy of world-making, Collins' empirical programme of relativism explicitly advocates agency-based analysis of knowledge change in science, although the base image of change differs. Rather than involving individual efforts at world-making that do or do not gain acceptance from the wider community, Collins' image of agency in scientific practice involves the inherently collective (i.e. social) process of negotiation. A negotiation consists of a give-and-take exchange among competing actors who offer contrasting interpretations of the object of negotiation. In scientific developments, there is generally a small number of negotiators, comprising what Collins calls a 'core set'. Knowledge changes emerge as the product of negotiations that take place within the core set. Each negotiation appears as a movement from a disordered heterogeneity to ordered stability in knowledge.

Collins' most significant early contribution was to provide a particularly clear picture of negotiation at work in a scientific dispute (Collins 1975). At issue was the presence of a new natural phenomenon—gravitational radiation. Relativity theory predicted that sidereal gravitational radiation reached earth in fluxes too small to detect, enough to elongate a one metre bar by only a tenth of the radius of an electron. Yet one physicist claimed to have found it experimentally at much higher fluxes. Sceptics countered with their own experiments and conflicting data. Collins showed that participants in this debate, whom he viewed as members of a core set, were operating without well-defined methodological standards for replicating experiments. They also were faced with explicit social disincentives to attempt even to replicate the initial experiment. Supportive results would have provided credit only to the original experimenter, while negative results could have been rejected as the product of poor replication. Core set members consequently engaged in a give-and-take among competing interpretations of what made an experiment to detect gravitational radiation a good one. Collins concluded that they were 'negotiating' simultaneously the appropriate standards for replication, the character of the natural phenomenon, and their own identities as competent scientists.

Although Collins' model of negotiation emphasises the role that contingent social practices play in scientific change, an asymmetry creeps in between his account of change and his account of continuity when we learn that scientific negotiations are actually an isolated process within a larger continuity. The form and importance of this continuity emerges in the 'third stage' of the planned empirical programme of relativism. The first two stages reinforce the emphasis on agency by documenting heterogeneities in negotiations. The goal of stage one studies is to demonstrate the interpretative flexibility of scientific data by showing 'that scientists can argue interminably over the meaning and significance of their data and that experiments cannot provide an answer' (1985: 130). Stage two studies show diversity in how controversies come to an end by exploring 'some of the ways in which scientists bring such arguments to a close in practice' (1985: 130). Studies in the third stage have turned out to be more difficult to achieve, however, for these differ from the first two by encountering a stable structure outside of science. That is, they 'look at these closures in the context of the wider network of science and of society' (1985: 130). For Collins, the stable structure is culture and the wider network is comprised of cultural institutions. This addition of culture surrounds the image of negotiating core-set scientists with a model of structure-driven external constraints, for cultural institutions reproduce themselves, in effect, through actors.

In a scientific negotiation, according to Collins, the outputs that scientists produce initially contribute to the construction of scientific culture. The gravity wave core set negotiating experimental protocols, for example, was actually engaged in a process of 'enculturation': they were 'building the culture of that part of science' (1975: 216). But once the scientific culture is established, it becomes a 'milieu' that other actors 'absorb' (1975: 217, 208). New knowledge appears to reproduce itself through actors because actors absorb, and hence disseminate, it through essentially automatic, unreflective processes. It is, Collins writes, 'the transmission of a *culture* which legitimates and limits the parameters requiring control in the experimental situation, *without* *necessarily formulating, enumerating, or understanding them* . . .' (1975: 207; emphasis in original).[3]

In providing continuity to actors, Collins' cultural institutions predictably go beyond the bounding function that rightness of categorisation plays in Goodman's framework. Yet these institutions actually function more like Goodman's habits, for they provide continuity by serving as an external 'influence' (1985: 151). For example, influences on a scientific field flow in from 'the more distant regions of the [social] network' or 'the wider web' (1985: 141, 151). The actual mechanisms of influence include the actor's social need to defend structure-given interests and to seek to maximise power, both to avoid a loss in identity.[4]

Collins documents these influences by citing a selection of case studies. For example, a successful scientific negotiation in physics served the interests of the largest possible group by producing an outcome that preserved the maximum number of existing theoretical and methodological agreements (Pickering 1981). And actions in pursuit of power may include selective reporting of evidence in professional journals (Travis 1981; Collins and Pinch 1979), publishing small errors that others make (Collins 1981), concealing results that might prove embarrassing (Wynne 1976), and magnifying the importance of trivial experiments that support a popular view (Collins 1983: 275). In sum, these external influences turn individual scientists into 'delegates' of cultural institutions, who represent 'the disciplines or other social and cognitive institutions which form their background' (1985: 148).

This account of continuity raises questions about the linkage between continuity and change. If scientists are all delegates of cultural institutions, how can we avoid the structure-based conclusion that scientific action is wholly determined by cultural structures? What is the connection between the continuity that culture provides and the actor's ability to produce change through negotiation? Collins' solution is to confine negotiation within the cultural institution of the core set. Core sets are unique cultural institutions in that their internal structure is agency. That is, the pre-structured task of a core set is to continually reshape scientific structures. 'In the core set', Collins writes, 'the actions of individuals and the influences of the wider web are melded together' (1985: 151). As a component institution in the wider web, the core set works as a funnel: it 'funnels in' structured social interests (1985: 144). But once inside, structure turns into agency as social interests become contingent negotiating devices for actors to manipulate. In Collins' terms, '[t]he core set gives methodological priority to social contingency' (1985: 144). In short, Collins fixes the core set as the mediator between continuity and change.

But how does this mediation work? What is the mechanism through which structured continuities are funneled into the maelstrom of change, through which change and continuity are melded together? Collins sometimes skirts the issue by seemingly characterising the core set as a black box that works by 'magic' (1985: 152). But when he confronts the problem directly, he, like Goodman, resorts to making continuity and change a matter of actor choice:

> We may think of them as spiders sitting in a web of concepts. Their choice turns on how much attention they try to attract to themselves. Given a piece of unwelcome or unexpected data, they may sit quietly and digest it, or ignore it, or they may shake the web until others notice what they have done—and, perhaps, its implied threat. Swallowing quietly will give them a little nourishment but shaking the web may assure them of a glittering future at the risk of disturbing others or creating enemies. (1985: 134)

While scientists certainly vary in their inclination to shake the web of established knowledge, this phenomenon does not account for the melding of change and continuity that scientists achieve routinely. The scientist's choice between sitting quietly and shaking the web involves a selection among types or dimensions of changes and among types or dimensions of continuities.

Lacking a more developed model of the mediation of continuity and change through actor judgments, Collins' framework leaves problematic the respective roles that agency and structure play in scientific action. Ostensibly, negotiation is specific to core-set developments of new scientific knowledge. But do scientists not also negotiate with other actors when they make budget requests and argue for promotions, or when they plan vacations with their spouses or get their kids to do homework? Isolating negotiations within scientific core sets actually limits the role that agency plays in social action and places greater emphasis on structure. In his original article on the gravity wave experiments, Collins conceived as his goal 'to make a partial escape from the cultural determinism of current knowledge in studies of science' (1975: 205). Yet he escaped from the determinism of knowledge only by subordinating it within a larger determinism of culture.

Negotiations and Resources: Reflexivity and Analysis

In *Laboratory Life*, Bruno Latour and Steve Woolgar try to avoid all determinisms in accounting for knowledge change in the science of neuroendocrinology. They apply agency-based approaches in two arguments that run in parallel throughout the book. First, they offer 'constructivist' interpretations of research undertaken at the Salk Institute in order to show that scientific facts are 'socially constructed'. Second, they take an initial step toward applying agency-based analysis to their own analytic categories, working to achieve 'reflexivity' by locating themselves in a process of social construction. I examine these accounts of change in turn, along with the continuities that each imports.

The main features of this analysis as a constructivist study derive from its status as a laboratory ethnography. Latour and Woolgar (hereafter L&W) sought to initiate an 'anthropology of science' in order to confute rationalist speculation in philosophy and show what 'in fact' happens in 'actual scientific practice' (1979: 36). Where Collins relied on interviews and published documents, L&W build their account from '*in situ* monitoring of scientists' activity in one setting' (1979: 27). Latour spent two years as a participant-observer at the Institute, managing the tension between the closeness gained by 'prolonged immersion in the daily activities of scientists' and the distance needed to preserve 'anthropological strangeness' (1979: 29).

Laboratory Life parallels Collins' work in treating knowledge change as a negotiation process, but the constructivist image of negotiation differs by picturing the give-and-take exchange as a sequence of public ('semiotic') operations on documents. 'A laboratory', they assert, 'is constantly performing operations on statements' (1979: 86). L&W interpret these statements broadly as 'inscriptions', a notion that 'designates an operation more basic than writing' for it refers to 'all traces, spots, points, histograms, recorded numbers, spectra, peaks, and so on' (1979: 88). The Salk lab is full of 'inscription devices' whose job is to 'transform pieces of matter into written documents, or a material substance into a figure or diagram' (1979: 51).

Document manipulation becomes negotiation when one realises that any document has a potentially unlimited set of 'readings'. The reading is the literary critic's analogue of the philosopher's problem of induction. Just as the previous association of any linguistic term with objects in the world provides no necessary constraints on future associations with other objects, so any document can refer to, or be 'read' into, the world of ever-changing ways. From this point of view, laboratory scientists negotiate knowledge developments by agreeing on readings of inscriptions that transform them from less certain to more certain statements.

This focus on documents rather than on conflict among actors reinforces the argument that change through negotiation involves a movement from a disordered state of affairs to an ordered state of affairs. L&W use every opportunity to emphasise this point, seeking especially to counter common-sense realism, as in the following:

> Actual scientific practice involves the confrontation and negotiation of utter confusion. (1979: 36)

> Our claim is not just that TRF is surrounded, influenced by, in part depends on, or is also caused by circumstances; rather, we argue that science is entirely fabricated out of circumstance. . . . (1979:239)

> It is not simply that the phenomena *depend on* certain material instrumentation; rather, the phenomena *are thoroughly constituted by* the material setting of the laboratory. The artificial reality, which participants describe in terms of an objective entity, has in fact been constructed by the use of inscription devices. (1979: 64)

> In sum, our discussion is informed by the conviction that a body of practices widely regarded by outsiders as well-organized, logical, and coherent, in fact consists of a disordered array of observations with which scientists struggle to produce order. (1979: 36)

L&W also make this case through more familiar analogies, such as the game of 'Go'. The purpose of this game is to maximise the amount of territory that one can control by strategically placing stones on the game board. The game begins in a state of disorder with an empty board and then becomes ordered progressively as players make moves (inscriptions) that eventually remove uncertainties and produce a fully constrained, 'stabilized' conclusion.

L&W avoid Collins' cultural determinism by treating individual actors solely as a source of change. They borrow Bourdieu's image of science as a competitive struggle in which participants invest their resources in order to maximise symbolic profit (Bourdieu 1975). Unlike Bourdieu, however, L&W merge the concepts of technical content and social power into the concept of 'credibility', justified by the argument that 'the credibility of the proposal and the credibility of the proposer are identical' (1979: 202). The mechanism of constant change is the 'credibility cycle' around which scientists revolve in an endless sequence of producing work, receiving recognition, and getting support. Since scientists do not maximise specific profits but manipulate resources for constantly changing purposes in constantly changing circumstances, they are, in principle, constraint-free agents of change.

But pointing to the empirical fact of constant change is not an argument against the systematic presence of continuities. L&W's constructivist analysis also treats scientific actors as vehicles for at least two types of continuities. Explicitly recognising these continuities alters the image of negotiation as a movement from disorder to order.

The first continuity concerns the manipulation of resources. L&W maintain that resource calculation, maximisation, and the presence of the individual vary so much that 'we cannot take them as our points of departure' (1979: 232). They do take as a point of departure, however, the view that actors produce public meaning only by manipulating resources. Resource manipulation is the one continuity that pervades every action, and it leads to a picture of every actor as a super-self-conscious quasi-economic man. This is an analytic model that L&W apply to each new event of action, and we can ask if it is a plausible model of the relationship between the actor and the context of action.

It has limitations. Conceiving action entirely as resource manipulation is an approach ill-suited to telling us, for example, when a feature of the context does or does not become an actor's resource in the first place. It cannot tell us the difference between legitimate and illegitimate resources, or when actors fail to manipulate available resources to their advantage. And it cannot tell us how a given inscription serves in practice as a constraint on future statements rather than just another resource to be manipulated. Just as structure-based analysis can be taken to task for trying to eliminate contingencies and viewing actors solely as vehicles for reproducing social and cognitive structures, so constructivism must grapple with the analytic limitations of viewing resource manipulation as the only continuity in any process of knowledge change.

Secondly, other parts of *Laboratory Life* show that context itself is a source of numerous additional continuities. Every reading of a document necessarily takes place in a particular context. 'The number of alternative readings of this particular utterance', L&W write at one point, 'will be constrained by the particular context which is brought to bear on the reading' (1979: 35). At every stage of each particular negotiation, a component part of the context is the set of previously completed negotiations. Laboratory scientists begin, for example, with stabilised, shared understandings that their research contributes to science, that inscription devices work, that they have established some standing in their field, and that their work will carry authority in outside contexts and groups. Each inscription communicates these presuppositions. Individual scientists may always be free to manipulate old resources in new ways, but the *meanings* that alternative choices will have for science and for their careers are also always constrained by these continuities. No negotiation ever begins in a state of utter confusion because each takes place in the context of previously completed negotiations.

That context provides continuity is particularly clear in the game of 'Go'. L&W forget that Go has been stabilised as a game. The players communicate through the negotiation of simulated territory a shared categorisation of what it means to play a game and to control territory. An empty board is not in a state of disorder, for players do not have the option of placing stones off the board, dropping large rocks on it, building piles of stones, or pushing opponents' stones around. Contingencies are indeed built into individual moves, but the meaning of every possible move is highly ordered. L&W implicitly recognised this when they 'circumstancised' (as opposed to reified) the game in the paragraph that describes it. The beginning of the paragraph characterises the first moves as 'almost entirely contingent', but by the end this had changed to 'entirely contingent' (1979: 248–9). An analogy can still be drawn with science if we include the continuities provided by context, but the analogy has an important difference. Science, like Go, may be described better not as moving from disorder to order, but from order to order by means of disorder.

L&W's attempt to eliminate determinism by means of constructivism leaves the actor as the sole arbiter of continuity and change. The actor could choose one or the other depending upon his or her changing purposes. But if a state of disorder never exists for any social actors, including scientists, then change always includes continuities and actors select among changes and continuities rather than between them. Consequently, we can say that constructivism seeks more than it can achieve. Although it makes a valuable contribution to the analysis of knowledge change by forcing attention to the freedom scientists have to read documents in new ways, it has difficulty in accounting for any of the continuities that occur in every reading.

A similar case can be made with regard to the second major argument in *Laboratory Life*, its attempt to achieve reflexivity. One of the book's most interesting features is its self-conscious interest in its own structure. 'By reflexivity', L&W write, 'we mean to refer to the realization that observers of scientific activity are engaged in methods which are essentially similar to those of the practitioners which they study' (1979: 31). Accordingly, L&W explicitly adopt an agency-based approach to accounting for their own agency-based analysis of neuroendocrinology.

They claim, for example, to have brought few conceptual presuppositions to their ethnographic observations. 'There was', they assert, 'no prior hypothesis about a concept (or set of concepts) which might best explain what was to be encountered in the field' (1979: 29). The analysis seemingly emerged directly from the field experience: 'Our approach relies on the emergence, from the circumstances of our study, of themes for discerning patterns in our observations' (1979: 39). Put in other terms, just as scientific documents have an irreducible fictional component because every reading is constructed, so L&W classify *Laboratory Life* as 'no more than *fiction*' (1979: 257).

Woolgar later justified this second argument in *Laboratory Life* as an attempt to go beyond instrumental ethnography to reflexive ethnography. Instrumental ethnography uses participant-observation data to support a preconceived analytic framework, but the problem with any organising framework is that it necessarily contains reifications. The framework freezes objects in the world with denotative labels and then reifies the labels by identifying them with the objects frozen. A genuinely 'reflexive ethnography', on the other hand, 'does not nominate a preferred [reifying] question, but instead reserves the right to take as problematic what is involved in posing and addressing questions in the first place' (Woolgar 1982: 498). The second argument in *Laboratory Life* thus alerts readers to their relationship with the text in order to heighten their sensitivity to the way reporting is done. Unfortunately, as Woolgar points out in concluding, *Laboratory Life* was indeed just a first step and 'it is not yet possible to specify the exact form of a reflexive ethnography' (1982: 493). In Woolgar's view, reflexivity remains an unattained, but desirable, objective.

An examination of continuities in *Laboratory Life* shows that reflexivity is both unattainable and already attained. L&W's sense of the term is unattainable because a wholly agency-based approach to analysis is impossible. L&W were not the naïve inductivists that they presented themselves to be. Both brought along a wide range of presuppositions about what would constitute a good analysis of scientific knowledge. They had established views about the limitations of rationalist Mertonian sociology of science. They had read Garfinkel, Collins, Mulkay, etc. Latour was not a man off the street innocently watching scientists but had already labelled, hence reified, himself as an anthropological observer of knowledge change. He organised the seemingly disordered mass of signals he received accordingly. Clearly, he was looking for scientific actors to display heretofore unrecognised capabilities. For L&W to characterise themselves as virtually pure inductivists was both somewhat disingenuous and not a little ironic.[5] L&W indeed cast an unusually wide analytic net, even brilliantly so, but it was nevertheless an analytic framework filled with continuities rather than a convergence of contextual contingencies. The most important continuity of all is that L&W knew they were constructing an account that would take its place within what might be called a cultural tradition of 'analysis'. In Western culture, 'analysis' is a relatively stabilised category that comprises part of the context of any attempt at analysing. Part of its meaning derives from its opposition to 'fiction', which seves as another stabilised category. *Laboratory Life* may construct the world of scientists using devices that a fictional account might use, but we readers possess the cultural knowledge necessary to recognise it as analysis rather than fiction. The book is loaded with textual devices that invoke the distinction between analysis and fiction and cue us to read it as analysis. Its final sentence, for example, speculates on what is needed to make its account 'more plausible' than others (1979: 258). Presupposing the category of analysis in one's account renders it impossible to achieve a fully reflexive account, in L&W's sense of a fully agency-based presentation. To classify the work as analysis is already to provide a continuity that shapes it. While specific analyses do rise out of circumstances, the circumstances always include stabilised categories that constrain the meaning of whatever is produced.

Yet, in an equally important sense, *all* analyses are reflexive, even the most seemingly detached, dispassionate, and unselfconscious scientific paper. By arguing for a new theory, method, or interpretation, every analysis alerts the reader that it is to be understood in relation to some stabilised analytic categories. It tells the reader that the author is taking a shot at modifying those categories in some way and, in the process, is putting his or her credibility and career on the line. The document is loaded with cues that indicate it expects to be read by a critical reader who evaluates the analysis and the analyst at the same time. In this sense, every analysis indexes itself, its author, and its readership, announcing that the author is trying to make a contribution to knowledge. If reflexivity means realising that observers of science use methods similar to science itself, then indexing oneself as an analyst is a consummate act of reflexivity. Everybody already does it.

L&W's agency-based interpretation of their own reflexivity makes continuity and change wholly a matter of choice for the individual analyst. But just as every analysis constitutes a change, it also conveys continuities, and each analysis selects from among changes and continuities rather than between them. The alternative approach to categorizing reflexivity that I propose here recognises the dual nature of all analysis. It transforms reflexivity from an unattainable goal into an instrumental device for comparing analytic accounts in Western culture. And, from this point of view, *Laboratory Life* stands out as an account whose unusually self-conscious reflexivity enables it to improve instrumental ethnography by demanding attention to agency as well as structure in scientific action.

Negotiating and Reproducing Identity

Since any analysis of knowledge change in science offers accounts of both change and continuity, the relative merits of alternative approaches can be evaluated according to how each mediates the two processes. On the one side, structure-based approaches are notoriously deficient at analysing change. On the other side, as I have tried to show, agency-based approaches have difficulty accounting for continuities. Both sides cope by positing asymmetries that de-emphasise the significance of their limitations. Structure-based approaches typically have sought to isolate change as a problem for specialised concern. One often finds, for example, separate courses labelled 'Social Change' or 'Culture Change' in sociology or anthropology programmes, as well as courses labelled 'Scientific Change' in philosophy of science programmes. Agency-based efforts to conceive continuity as psychological habits, external cultural institutions, and universal manipulative abilities all produce models of actor judgments that view actors implausibly as explicitly choosing between change and continuity. A solution to the problems of both sides may lie in treating change and continuity more symmetrically by seeking to recognise equal statuses for agency and structure in processes of knowledge change.

One way of achieving this may be to focus on the identity of the individual actor. Agency-based and structure-based approaches appear to converge, or at least intersect, in a common concern for the significance of actors' positions with respect to other actors and objects. A great variety of terms serve to describe the positional definitions of actors, each with its own implications, including interests, enrolments, statuses, theory groups, agonistic fields, networks, etc. However, agency-based and structure-based approaches differ systematically in treating positional definitions as phenomena that emerge as a product of agency or as sets of initial conditions that serve to structure action.

I assign the term 'identity' as a cover label for the positional definitions of actors and other cultural objects. 'Actor identity' refers to actors alone. These labels make no a priori commitment to the role that positional definitions play in action, with one important exception. For the moment I am not using actor identity in the psychological sense of a structured personality or an individual's feeling of continuous existence. I will return to individual-specific phenomena below. Instead I emphasise the externally-defined, positional sense of identity in order to argue for the benefits of examining the interactive processes and mechanisms through which actors simultaneously constitute and reproduce their identities. In other words, as a nexus between agency and structure in negotiating knowledge, I propose the study of *identity dynamics*.

In what follows, I identify some critical features of identity dynamics in the negotiation of knowledge by briefly reinterpreting a familiar case example, the gravity wave debate. In the spirit, although not the letter, of Goodman's philosophy, the example highlights the importance of symbolic categories in identity dynamics.

By systematically following the changing identities of actors and objects in the negotiation of knowledge, we can see that heterogeneities in a negotiation may frequently be organised, rather than contingent, phenomena.

The main protagonist in Harry Collins' studies of gravitational radiation was the physicist Joseph Weber. Collins argued both that core-set scientists were involved in a heterogeneous negotiation of gravity waves, gravity wave detectors, and scientists all at the same time, and that because of this heterogeneity closure of the debate was a function of contingent social phenomena. Following Collins' lead, we could say that when Weber claimed in 1969 to have detected gravity waves, he initiated a negotiation that simultaneously reconstituted the identity of gravity waves, the identity of gravity wave detectors, and his own identity as an actor. For example, when Weber first published his claims, he was hailed as 'discoverer of gravity waves', as a scientist who had opened a new window on the universe. He was offered a year's leave at the Institute for Advanced Study, became a regular on the university lecture circuit, and was portrayed by journalistic reports in heroic terms.

Weber's new identity was inextricably linked to his detector.

The detector was an aluminum bar suspended in a vacuum. It was three feet in diameter, five feet long, and weighed three and a half tons. Weber chose this size because it had a resonant frequency in the kilohertz range, which is characteristic of bursts of radiation from supernovas, the source expected to deliver the highest intensities of radiation at regular intervals. Piezoelectric crystal sensors glued to the bar at various points served to convert linear strains into electrical signals. The electrical signals were then amplified and converted into a visual output by a pen recorder on graph paper. The experimental apparatus thus consisted of a large number of objects with specified positions in relation to one another. These included the aluminum bar, wire suspension, acoustic filters, control instruments, piezoelectric sensors, amplifiers, pen plotter, computer programmes, programmers, graduate students, Joseph Weber, etc.

For Weber to link himself to gravity waves by means of this apparatus, he had first to subtract the effects that other sources of vibration could be expected to have on his pen plotter. Possible sources included seismic, acoustic and electromagnetic disturbances, electrical disturbances from cosmic rays, Brownian motion in the atoms of the aluminum, electronic noise, and so on. The wire suspension system and acoustic filters eliminated some vibrations. Other sources could be eliminated from the print-out by comparing outputs with control equipment and by correlating results from two bars at widely separated locations. Still others could be eliminated only through statistical techniques.

When Weber reported detecting fluxes of gravitational radiation much higher than background theories predicted, he built these claims upon the output of his pen plotter. He maintained that, after the expected effects from other sources were subtracted, the presence of significant peaks above background noise indicated the presence of gravity waves. The act of establishing links between gravity waves, peaks on the plotter and the experimenter himself made Weber a discoverer. Gravity waves became a part of Weber's identity like no other scientist before him.

But other physicists also constituted new identities for themselves as Weber's opponents by claiming that he had not detected gravity waves. Their claims were heterogeneous in so far as these sought to disrupt Weber's link to gravity waves at different points in the chain of apparatus. For example, a computer expert discovered a flaw in Weber's computer programme. An astrophysicist disrupted the connection Weber claimed between the frequency of peaks and sidereal time, that is, star time, by showing that the peaks correlated with non-sidereal orientations. Several astrophysicists designed alternative detectors to have greater sensitivity and then reported their null findings. And one colleague disrupted Weber's connection to gravity waves by reporting evidence of another case in which Weber fudged his data, suggesting that maybe the link in this case was tenuous as well.

Although these various strategies were indeed heterogeneous, all appeared as attempts to disrupt the connection between Weber and gravity waves by searching for weak links in his apparatus or by replacing them with stronger links. In other words, the heterogeneity in this case may indeed be evidence that Weber's identity could stabilise as discoverer of gravity waves only if the links he asserted to exist among the objects in his apparatus withstood systematic scrutiny and challenge, and if other experimenters achieved the same links through their apparatus. I expect it would be easy to find other such cases in which attempts are made to disrupt the linkages that give identity both to scientists and to their objects of study. If such attempts are indeed common, then perhaps we can describe the stabilisation of Weber's identity as 'unreliable scientist' to be the outcome of a mechanism with routine dynamics rather than consisting only of unpredictable social contingencies. Hence we should focus our attention on trying to account for the variations that make up the dynamics of this organised diversity.

It is very difficult to isolate a single type of motivation, for example, interests versus reasons, for scientists' actions when scientists act to constitute or reproduce their identities. In other words, actor identities reveal a structured ambiguity in the psychological content of agency.

For example, during the years between 1969 and 1975, Weber systematically defended both his claims and his new-found elite status against challenge after challenge. It is impossible, in principle, to determine absolutely whether the agency in these actions was motivated psychologically by a firm belief in the validity of his experiment or by a desire to preserve his interests as discoverer of gravity waves, or both. The identity of a particular actor at any point in time consists of a web of links to other actors and objects. If the significance attached to some event of action is 'congruent' in meaning with more than one feature of the actor's identity, such as Weber's connection to his detector and his status in the community of astrophysicists, then an observer can never be certain about which of these features motivated the action.

This point reinforces the argument commonly advanced by agency-based approaches for the need to overcome the traditional structure-based distinction between social and cognitive phenomena by recognising the routine roles played by both in the negotiation of knowledge. Within the dynamics of identity both interests and reasons, to simplify drastically the potential variables, routinely function together as potential motivators for action. There is no way, in principle, to eliminate any specific type of positional definition in accounting for a given scientist's action in negotiating knowledge.

Positional identities do not link actors and other cultural objects *qua* objects, but only in so far as these objects are classified as tokens of types of symbolic categories.[6] Objects become stabilised as their classifications in terms of symbolic categories become stabilised.

Much as Goodman might argue, I maintain that symbolic categories play an important, if implicit, role throughout Collins' account of the gravity wave debate. One key source of symbolic categories in this scientific negotiation was the background theories on which the scientists all predicated their claims. Every account of gravity wave detection I have read, including Collins' work, begins by referring to features of Einstein's General Theory of Relativity (e.g. Fisher 1981, Thomsen 1972, Weber 1972). According to the field theory of gravitation, when matter is violently accelerated in a non-symmetrical way, a disturbance in the local geometry of space-time would be produced, which would then propagate and transport energy at the speed of light. Background theory also predicts that the passage of gravity waves through an elastic solid would excite internal vibrations at the frequency of the waves. If this frequency matched the natural acoustic frequencies of the solid, a resonant response would occur, producing an intensified shortening and elongation across the entire mass of the solid. Finally, the elimination of predicted signals from seismic, acoustic, electromagnetic, electrical, Brownian and electronic disturbances all relied extensively upon other background theories. Taken together, these background theories provided numerous categories of symbols that enabled experimenters to give meaning to their detectors, gravity waves, and even themselves by classifying all as instances or tokens of these categories.

In like fashion, Weber's established identities as professor of physics at the University of Maryland, recipient of funds from the Gravity Research Foundation, adviser to students, etc., presupposed a range of symbolic categories that give meaning to the institutional dimensions of science. These too contributed necessary content to the experimenter's identity in relation to the detector and to gravity waves.

This view of identity dynamics draws from a commonly-held view of symbolic categories as deriving their significance through contrasts with one another, and which can be identified methodologically through presuppositions. The grammatical categories of language provide the clearest examples. Just as linguists describe speech as presupposing shared categories of grammar, so all meaningful action can be described as presupposing shared symbolic categories. However, the presence of sharedness must be demonstrated empirically, and one cannot assume that any given set of categories will form a coherent system as do linguistic categories.

That symbolic categories play an active role in the dynamics of identity is illustrated by the fact that Weber's claim to have discovered high fluxes of gravity waves experimentally prompted a re-evaluation and restructuring of background astrophysical and cosmological theories. Weber's claim rendered ambiguous the symbolic category 'gravity waves'. The inconsistency between detecting high fluxes of gravity waves and theoretical predictions of low fluxes could be seen easily by using the theories to calculate how much matter would have to be destroyed each year for Weber's results to be correct. The necessary conclusion was that the entire Milky Way should already have burned out. Yet astronomers had collected data from many other sources indicating a much lower expenditure of mass in the Milky Way. One response by supportive astrophysicists was to adjust the background theories in ways to predict higher fluxes, by postulating 'beamed' radiation, for example. In other words, in order for Weber to acquire the identity of 'discoverer' and for his apparatus to become a 'detector', the meaning of gravity waves as a cosmological category would have to change.

Since actor identities and object identities gain their content from symbolic categories, the stabilisation of a scientist or scientific object is better described as the stabilised classification of that actor or object in terms of symbolic categories. And since the negotiation process is likely to include a mix of traditionally cognitive and traditionally social considerations, the relevant categories are likely to be drawn from distinct sets of theoretical and institutional categories. Weber's identity, for example, developed precisely by invoking categories that had stabilised as belonging to distinct domains of theory and institutions.

This point highlights probably the most important feature of identity dynamics, that is, that theories and institutional considerations contribute to a scientist's identity through the same vehicle, symbolic categories.

An identity stabilises when it appears routinely in the presuppositions of action. In a controversy or debate over knowledge, identities become stabilised when these appear in the presuppositions of participants who occupied previously opposed positions of power, rather than through consensus.

By 1975, Weber's identity had stabilised, at least temporarily, as 'unreliable scientist'. There was widespread agreement among his colleagues that his apparatud was not a gravity wave detector, and that gravity waves still possessed the identity established by relativity theory. Some scientists still disagreed, including Weber, but an absolute consensus was not necessary for stabilisation to occur. Rather, stabilisation occurred when the agreement was sufficiently wide to include a significant preponderance of power within the astrophysical community. Empirically, we can identify this agreement by tracing presuppositions implicit in the scientist's actions.

For example, Weber had difficulty obtaining continuing funding. If funding committees were treating Weber as the discoverer of high fluxes of gravity waves, we can presume that funding would likely have been no problem. That funding was a problem presupposes that the committees of peers were classifying Weber instead both as a regular member of the research community and a potentially unreliable one at that. That is, the funding actions presupposed an identity for Weber other than discoverer and an identity for gravity waves that was unchanged from its previous form. Such examples could be multiplied by examining Weber's experience in seeking publication, pursuing lecture opportunities, trying to influence actions within his own department, etc.

Previously stabilised identities give structure to action by providing a criterion of congruence in meaning, but giving meaning does not constrain agency.

An important lesson of agency-based analysis is that actors are always free, in principle, to constitute their identities in wholly unanticipated, even random, ways. A previously stabilised identity provides neither fixed plans of action nor normative rules that constrain the actors themselves. Thus, for example, Joseph Weber had the freedom to claim that he had discovered the workings of the active hand of God. Free will always reigns, in principle.

However, despite the freedom that exists in principle, actors generally appear to choose from fairly limited sets of alternatives. Free will reigns, but order is rampant. Exploring the dynamics of identity provides a way of understanding this duality through a focus on meaning.

A stabilised feature of an actor's identity serves as an interpretative lens by providing a criterion of congruence in meaning. That is, that feature serves to distinguish behaviour whose meaning is congruent with the identity from behaviour that is incongruent. An action is congruent with an identity when it communicates contrasts in meaning that are proportional to the contrasts in meaning that characterise the identity.

For example, the output on Weber's pen plotter consisted of a zig-zag chart representing changes in the length of his aluminum bar. The question Weber faced was whether or not the zig-zag represented anything more than random fluctuations, or noise. By arguing that the contrasts between peaks and troughs on his output were proportional to the categorical contrast between 'gravity waves' and 'noise', Weber posited relations between objects in the world that were incongruent with those relations as specified conceptually in theory. The incongruity was resolved when the community agreed that these peaks were indistinguishable from noise. The proportionality was lost, and there was no longer any need to revise background theory. In the same way, for Weber to claim that he had discovered gravity waves was initially congruent with his identity as a scientist, while positing that he had discovered the actions of God would have been incongruent with that identity.

Within the dynamics of identity, the criterion of congruence regularly serves to limit the choices actors make. An actor's positional identity consists of many links to other cultural objects. When an individual is deliberating a course of action, the number of alternative choices that may be congruent with more than one component of that person's existing identity can appear quite small. Thus when Weber advanced his original 1969 paper, few alternative characterisations of his identity were available. Basically, he was either discoverer of gravity waves or unreliable scientist. Both Weber and his opponents acted in ways that preserved these as the only two viable alternatives.

Finally, the study of identity dynamics focuses our attention on individual-specific phenomena in a new way, as decision styles, without demanding that actors explicitly choose between change and continuity.

The limitations that identities place on actors' choices returns us to explore the content of agency. The key question is: if actors are free to be incongruent and even random in their actions, how do they routinely achieve congruence and order?

As I indicated at the beginning of this section, a common approach to identity characterises it in psychological terms. While gaining access to psychological realities is a laudable objective, I see no way of realising it in practice. The only data about psychological processes that we have to work with as observers, even when we are observing ourselves through introspection, are the outputs from processes whose contents remain opaque. The opaqueness of psychological mechanisms need not deter us, however, from drawing upon observable data to identify individually-specific patterns of agency. I call these patterns 'decision styles'.

I developed the concept of decision styles partly by observing my own decisions. Although I can, in principle, exercise free will in every context of my life, I find I exhibit a distinct tendency to mediate conflicts. I exhibit this tendency among my family and friends, within my academic department and graduate programme, in previous studies of public controversies over technology, and even here as I try to mediate agency-based and structure-based approaches to the negotiation of knowledge. Mediating conflicts is, for me, a decision style that an observer can use to predict my behaviour in specific contexts, given my identity and the circumstances I encounter.

Empirically investigating decision styles both encompasses and goes beyond the narrow psychological assumption in many agency-based approaches that actors seek to maximise power and control by manipulating resources. Many people do not seek to maximise power and control all the time. Other people actively avoid seeking power and control. I maintain it is much more enlightening to explore empirically how individual actors tend to make decisions than to assume that all formulate strategies according to the same calculus. In addition, investigating decision styles gives content to agency by distinguishing different types rather than assuming agency to be an undifferentiated, hence unanalysable, phenomenon.

For example, we may discover on the one hand that some scientists in the gravity wave debate tended to argue on the basis of experimental results regardless of their career interests in the matter. Weber, on the other hand, was a scientist whose arguments were based on his interests seemingly exclusive of his results. By systematically identifying the decision styles that participating scientists exhibited in the debate, we might be able to take another step in deconstructing the heterogeneity of the debate into organised mechanisms, without sacrificing agency in the process.

Connecting the concepts of decision styles and actor identities provides a fruitful area for further research on the nexus between agency and structure. We may find that decision styles sometimes correlate with status, as when senior scientists appear to be more open to challenge than their junior colleagues. We may find a tolerance within science of a wide range of decision styles or, alternatively, an intolerance for certain decision styles, such as a regular tendency to make claims that are sharply incongruent with background theory. We may be able to better explain the rhetoric of scientific papers by analysing how scientists with varying decision styles transform those differences into more restricted styles of writing. And we may be better able to explain the need for logical consistency in valid scientific argument by showing how scientists reconstruct the congruities and incongruities in their everyday actions in purely logical terms. In short, a focus on decision styles as part of a larger enquiry into identity dynamics provides a place for the individual-specific phenomena of agency alongside the examination of structure. In the process, it provides new reasons for conducting biographical studies of knowledge producers.

I have described six different features of identity dynamics in order to advance the argument that every event of agency both constitutes and reproduce an actor's identity. In a trivial sense, agency always constitutes at least some portion of an actor's identity because every event is itself a new context, and agency always reproduces at least some portion of an actor's identity because the event of agency minimally reproduces the potential to act. What is far more interesting than these polar extremes, however, are the routine, yet highly variable, relations between events of constitution and events of reproduction. Since an established identity gives meaning to decisions that an actor makes, any knowledge producer is forced to deal with the implications of consistency and inconsistency raised by each specific judgment. I maintain that we can overcome the problem of change and continuity raised by the opposition between agency and structure by asking: how do established identities merge with decision styles in changing contexts to produce the actual knowledge developments we observe?

Notes

1. The term 'structure-based approaches' applies generally to rationalist approaches in philosophy and sociology. These tend to account for individual actions as implementing, manifesting, or realising previously-established structures. Examples in the two disciplines also tend to complement one another by drawing on an a priori distinction between cognitive and social phenomena. This paper assumes some familiarity with structure-based analysis. For an explicit comparison of structure-based and agency-based approaches to engineering undertaken in sociology, philosophy and history, see Downey *et al.* (1989).

2. Note, for example, Goodman's willingness to entertain the notion that the distinction between continuity and change might be correlated in some way with the separation between the right and left sides of the brain, even while offering devastating counterarguments (Goodman 1986).
3. For further discussion of enculturation, see Collins' (1985) analysis of joint entrenchment.
4. For example: '. . . technical arguments have been found to be limited by cultural constraints and the distribution of power, rather than "internal" technical knowledge or logical possibility' (Collins 1983: 281).
5. See Woolgar's discussion of the significance of irony in 'mediative' and 'constitutive' accounts (1983). I shall be arguing below that all accounts are mediative for the idea of a constitutive account contains an internal contradiction.
6. What follows can be read, in part, as a challenge to the hyperrealism of Bruno Latour's (1987) actor-network theory. I drafted the critiques in this chapter prior to the publication of *Science in Action* and so must postpone for another occasion a more detailed treatment of Latour's work.

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