

# ON THE CONSTRUCTION OF CREATIVITY: THE 'MEMORY TRANSFER' PHENOMENON AND THE IMPORTANCE OF BEING EARNEST (1)

DAVID TRAVIS  
*Polytechnic of North London*

## 1. Introduction

This paper is concerned with some aspects of the construction of a now widely rejected piece of scientific knowledge, the 'memory transfer' phenomenon (2), which has been the focus of a controversy in neuroscience over the last twenty years. Most interested scientists agree that, *if true*, it would have revolutionary implications for our understanding of how the brain works. But there has been a marked polarization of beliefs over the reality of the phenomenon, and the status of the participants. The field has been variously assessed as a case of bad science, unconscious experimenter bias and 'mass hysteria' while others believe that the major proponents should be in line for a Nobel prize. Chedd, writing in the *New Scientist*, has commented:

If anyone still holds any illusions about the objectivity of science, then even a cursory glance at the brief history of the chemical transfer of memory would surely unburden them. It is virtually impossible to enter the subject, either as a participant or observer, without pre-conceptions (3).

One early feature of the field was the perception of a subversive flippancy in some articulations of the memory transfer phenomenon. In this paper I will examine part of the climate of opinion surrounding the reception of this piece of scientific creativity, specifically the aura of 'schoolboy humour', relating the perceived incongruity of the conceptual structure of the phenomenon to concrete actions and institutions within the field.

## 2. The 'Memory Transfer' Saga

The 'official history' of the field begins in 1953 with two psychology graduate

students at the University of Texas. Thompson and McConnell attempted to condition a simple freshwater flatworm, the planarian, to contract or 'scrunch up' each time an electric light was switched on; the unconditioned stimulus being a mild electric shock delivered through the water in which the worm was placed (4). The planarian was strategically interesting because it is the lowest organism on the phylogenetic scale to have bilateral symmetry, a primitive brain, and human-type synapses. Thus it should be the lowest organism capable of true learning (5).

Thompson and McConnell's results – indicating that the planarian could be classically conditioned – were published in the *Journal of Comparative and Physiological Psychology* (6), one of the highest status journals for animal research (7), and the mainstream of physiological psychology (8). The paper was a straightforward piece of research which caused relatively little notice, and no controversy, until later experiments made it important.

One of the peculiarities of planarian worms is that they can undergo a-sexual reproduction through fission. Thompson and McConnell had speculated about what effect this might have on the learning they had induced, and when McConnell had set up his laboratory at the University of Michigan, he performed an experimental simulation. Trained worms were cut in half across the middle, allowed to regenerate, and tested to see how much they remembered (9). Surprisingly, the animals which regenerated from the tail sections remembered as much as, if not more than, the heads (10)! This report caused a good deal of interest: apart from being an intriguing phenomenon 'as such', it also carried the implication that the memory was not localised in the brain, but distributed throughout the worms' body, perhaps in all its cells.

A third set of experiments made the research 'glamorous' and attracted a good deal of comment in the popular press. Some species of planarians – including those used by McConnell – are cannibalistic when starved. Trained worms were chopped up and fed to their hungry, untrained fellows, who did much better at their learning task than those fed on a diet of untrained worm (11). 'Food for thought' to cite a common witticism of the period.

The planarian research, including the original conditioning study, became the focus of a bitter controversy which was conducted mainly among psychologists and zoologists. The important issue was whether the worms could be said to have learned anything in the first place, rather than the question of

whether the results of the experience could be transferred. As it happened, these concerns were effectively superseded for many scientists when in 1965 four research groups, working without knowledge of each other (12), published results claiming that the memory transfer effect could be produced in vertebrates (13).

No one doubted that rats could learn.

There followed a rash of attempts to replicate the phenomenon, many of which failed (14). Later, more positive results began to emerge, and by 1970 the 'hit score' in terms of experiments was 133 positive, 115 negative, and 15 equivocal (15). Since then the number of positive reports has increased more rapidly than the number of negative reports.

In 1972 a research group headed by Ungar claimed to have isolated, characterised and synthesised one of the transfer agents, 'scotophobin', a peptide said to code for fear of the dark (16). It has subsequently been claimed that the compound produces dark-avoidance behaviour in rats, mice, goldfish and cockroaches (17).

Few scientists now give the results of the field much credence, and active research has virtually ceased following the death of the major proponent, Ungar, in 1977. The failure *can now be seen* as the failure of a logical progression of experimentation related to the concept of a *molecular* code for memory (analogous to the genetic code), and most reviews adopt this construction of events. Until a difficult-to-define point in the early 1960's however, the implications of memory transfer (in planarians) were not connected to speculations about molecular memory mechanisms. From 1965 or so, a marriage was proclaimed by some proponents of the transfer phenomenon, but those concerned with molecular memory mechanisms *per se* were somewhat reluctant suitors. The memory transfer phenomenon was taken to require a molecular memory *code*, and was widely recognised as the only experimentally testable formulation of the hypothesis that specific memories are coded in specific molecules. Molecular memory *mechanisms*, on the other hand, need not necessarily imply specificity of *coding*, nor need they allow for the possibility of a memory transfer phenomenon.

One final piece of context is that initially quite separate from the planarian transfer experiments there were, in the mid sixties, high expectations that there would be some kind of advance related to the chemical basis of memory. As far as can be gathered there was a widespread feeling among those

concerned with the biochemistry of memory that in the next decade or so 'something was going to happen'.

### 3. Structure and Ambiguity in the Memory Transfer Phenomenon

#### 3.1. *The Idea of Memory Transfer*

In abstract, an experiment in the field can be characterised as comprising three phases. The initial training involves the application of a number of relatively standard procedures and practices that would be recognised by animal training psychologists all over the world. (In the case of planarian worms the situation is more complex. Obviously they had hardly ever been used in behavioural laboratories before this series of experiments, and although terms such as 'stimulus', 'response' and 'classical conditioning' denote shared cultural resources, the *detailed rules* and tacit knowledge necessary for their *agreed application* to planarians was problematic (18).)

The second phase is the extraction of the transfer factor (or a solution thought to contain the active material) from the body of the worm, or from the brain of the animal. Some protocols involved a crude homogenate of brain, others some kind of purification procedure which would concentrate, and give further information about the characteristics of, the transfer factor. Again, these are routine activities in the context of certain laboratories. Similarly, and thirdly, thousands of animals are injected every day with chemicals of biological origin, and the subjects' performance on running a maze or some other learning task recorded.

Taken separately, these are three culturally legitimate practices likely to be found in behavioural psychology, biochemistry, and pharmacology laboratories, respectively. Putting them together in the context of studying the behavioural effects of biological compounds – hormones for example – obtained from animal organs is a commonplace in some laboratories. However, linking these practices in the context of the transfer of specific memories in vertebrates produced a mixture of amazement, derision, and shock. In a loose manner of speaking, the phenomenon was 'odd'. It seemed to violate a number of assumptions about memory – it not being the sort of thing you can put through a homogeniser (a relative of the kitchen liquidiser) or hold

in a test tube (19). On orthodox theory, memory should be a matter of a network of nerve cells.

Whilst it was still associated with planarians the implications of memory transfer were to a large extent *contained*. Planarians are fairly primitive organisms, and have the potential for regeneration not found in vertebrates for example. Perhaps, it was thought, memory is somewhat 'different' than in higher organisms (20). But rats are another matter all together: the vast bulk of experimental data on the biochemistry and physiological psychology of memory is from studies of the rat. The principles of memory are thought to be basically the same as in humans, at least as far as the (bio)physical basis is concerned. The memory transfer experiments on rats broke a kind of implicit inferential barrier. As a phenomenon associated with worms, memory transfer *could* be regarded as a biological oddity of limited relevance and applicability. Claiming that the phenomenon operated in rats was far more consequential. So, despite a knowledge of the worm-running experiments, the first mammalian results produced strong reactions (21).

It was as if some kind of monster (22) had been produced, a *chimera* (23) constructed out of a lion's head, a goat's body and a serpent's tail. The results had no right to exist in the conceptual space they attempted to occupy – indeed, there was no conceptual space in which they could reasonably exist (24). To switch metaphors, the memory transfer field had produced the scientific equivalent of the sewing machine and the umbrella on the operating table (25).

Professor Ernest Chain at the 1968 UNESCO – IBRO meeting vehemently deplored "wasting precious time on such *chimerical* and *ridiculous* goals when it could be spent on better founded biochemical investigations". (26)

A scientist who first heard about one of the mammal experiments during a neuroscience meeting in 1965 when the results were reported 'informally' (not by one of the researchers concerned with the experiments) commented:

*X* came in with a copy of *Science*, to read it, and a discussion ensued . . . I guess [it's] fair to say it sounded incredible to me, and at the same time it sounded like if it was true it was the greatest thing since soap or something . . . most people [at the meeting] I think, thought it was a bunch of crap. (27)

Another researcher noted that "there was a kind of fever in the air at the time" and felt the need to stress that in his particular group "our doubts

*didn't take the form of laughing* or saying 'there's nothing to it' [they] . . . took the form of 'well we'd better see for ourselves', because the consequences were just too important to ignore" (28).

These extracts reveal a deep seated ambivalence about the claims to have shown the transfer phenomenon in vertebrates. There was surprise and amazement; reactions tinged in some cases, and dominated in others, by the possibility that this was some kind of a joke (29). That is not to say, of course, that the scientists reporting the results were widely seen as perpetrating a hoax. Undoubtedly *some* observers felt that this was a possibility, and there was a precedent for this opinion. Jacobson — one of the 'famous four', who had previously worked with McConnell, commented:

I recall that when the regeneration report [on planarians] was first submitted for publication, one referee angrily professed that this was either the biggest finding or the biggest hoax in psychology for years — and he suspected the latter to be true . . . at least the veracity of the experimenters is no longer challenged. (30)

Jacobson's final point was somewhat premature. One scientist informed me darkly "[at the time] I was not ready to call some of them liars . . . at a later point I was quite willing to do so" (31).

Such accusations were in the minority. A more common reaction was to see it as humour of cosmic proportions, a whimsicality, or sport of Nature which had led astray those scientists who had not kept an adequate grip on their control groups and interpretations. The ambivalence however was deep. The results just might be true, and if so it would not pay to be left out of "the biggest thing in memory for a hundred years" (32). Certainly many groups of scientists went ahead and tried transfer experiments, and some published their results in one form or another (33).

In this section I have emphasised an ambiguity in perceptions of the phenomenon, and described the deep ambivalence in reactions to the claim to have demonstrated the phenomenon in rats. Now there is always some uncertainty surrounding the introduction of a new and surprising scientific claim. What emerged here however, was that the specific ambiguity associated with memory transfer was part of a wider structure of ambiguity which related to a number of institutions and actions in the field (34).

In the rest of this paper I will look at one theme in the *structure* and the *source* of this ambivalence, and in doing so offer a partial explanation of why

this particular piece of scientific creativity failed to become more widely institutionalised.

### 3.2. *Funny Ha-Ha, Funny Peculiar or Creativity?*

As I have described it, there was a structural ambiguity in the reception of the vertebrate experiments. Should they be taken as a joke or as a serious piece of scientific creativity? Or perhaps both? The reactions associated with this particular episode are, no doubt, symptomatic of a general question of the relations between humour, other forms of understanding, and creativity (35). This is a difficult and little-explored area, but a useful analysis is to be found in the work of Arthur Koestler, especially *'The Act of Creation'*, and *'The Ghost in the Machine'* (36). The argument proposed is that creativity in humour, science and art have a similar 'logical' pattern – the discovery of hidden similarities – but differ in what Koestler calls the 'emotional climate'. To gloss his account, the 'logical' similarity subsists in the bringing together of frames of reference in a collision (humour), in a fusion (science) or in confrontation (art). These cognitive frames are not necessarily incompatible, but in the case of humour for example, are *habitually* held apart.

Let me unpack this a little further. (In doing so I would not claim to be following Koestler closely, but freely interpreting). In the case of assimilated scientific creativity we can occasionally recapture to some extent the moment of fusion when previously unrelated cognitive elements (frameworks) were brought together in a new configuration. Krohn has given a number of detailed examples of this kind of account in his thematic paper for the conference on *The Social Process of Scientific Investigation* (37). Whether the juxtaposition of frames is eventually seen as a piece of scientific creativity by the relevant community will depend crucially on subsequent events. It *might* be reconstructed as a 'clang association' or a pitfall (38). Institutionalised creativity on the other hand will be seen to have been fruitful in terms of further research but, *at the time* of its introduction, it may have seemed absurd.

The relativity of meanings in science when 'the uncertainty of the future is visited on the past' has now been well established (39). The difference between the recognition of a potential scientific contribution as 'creativity' or 'absurdity' may not be very great, and the attribution unstable. Explanations

of scientific innovation in terms of an interactive theory of metaphor stress this (40), and Barnes has noted that the introduction of a new metaphor may be indistinguishable from a category mistake (41).

This perspective blurs the sharp distinctions between creativity, the meaningful, and the absurd, seeking to uncover ways in which such attributions are constructed and maintained. Koestler's approach helps us to visualise the polyvalency of meaning extending beyond the strictly scientific into humour.

The comic, according to Koestler, is the bringing together of frames of meaning or associative contexts that are habitually held apart, so creating a surprising configuration. In some cases the juxtaposition is 'impossible' and the unstable conceptual space that is created explodes in laughter (42). One result of this can be the kind of cathartic effect that Koestler describes when the tension wrought by the conjoining of habitually incompatible frames is discharged (43). In more 'intellectual' forms of humour however, such as witticism, satire or irony (44), there can be an intention to leave a certain tension or discomfort. A further characteristic of these higher forms of humour found in the logical paradox for example, is that the interpretive work of 'seeing the joke' shades over into that of 'solving the puzzle' (45).

Let me illustrate some of these points through a series of examples. We are so familiar with statements such as 'light is a wave' that we tend to miss the literal absurdity. We know what is really meant. Other statements may be humorous *as well as* scientific: 'glass is a liquid' may be surprising and can produce a wry sense of amusement when we recognise the particular way in which the scientist is contradicting everyday categories. The humour is contingent, and separable from the scientific point. We can accept these divergent definitions of 'solid' and 'liquid', making sense of the statement according to the context. A *child* stating that 'glass is a liquid' would almost certainly be corrected.

In the particular context in which it was received, the memory transfer phenomenon in mammals was not perceived simply as a piece of potential scientific creativity. It was also seen to have some of the characteristics of a joke, and the question of appropriate reactions was problematic. The juxtaposition of frames of reference that was memory transfer in mammals was certainly surprising. It carried enormous implications that some saw as potentially fruitful, others as absurd. Seen against the background of the



great expectations held by some scientists for the possibility of a molecular memory code analogous to the genetic code, the phenomenon implied a startlingly simple experimental strategy. To stretch a point, it was *such* a startlingly simple extrapolation that for some it was almost *surreal*.

But why was this so? In looking at the sources of this response most of the discussion will centre on events related to experimentation on planarians – an occupation known as ‘worm running’. It was these events that structured the situation in such a way that the mammal experiments precipitated the reactions I have described.

#### 4. The Importance of Being Earnest

##### 4.1. Science and the Comic

One of the characteristics of an emergent specialism is the founding of a journal devoted to the area. This provides a distinctive forum, an outlet for research reports and a focus of identity which may be highly important in terms of group solidarity. So it was in the early memory transfer field – but with a difference. When McConnell’s research became known through the scientific and popular press he received many requests for information (mainly from high school students) on how to obtain planarians, how to keep them, feed them, and carry out experiments. His group produced a set of mimeographed instructions and – with characteristic humour – labelled it ‘*The Worm Runner’s Digest*’ Vol. 1, No. 1. This was sent out complete with a heraldic device bearing the legend ‘*Ignotum per Ignotius*’ (46). Some of the recipients took the joke at face value and sent back the reports of their experiments for publication, and so the journal was launched (47).

The masthead of *The Worm Runner’s Digest* proclaimed that it was ‘An informal journal of comparative psychology published irregularly by members of the Planarian Research Group’. It was also one of the small group of journals that published scientific humour (48), or as McConnell cynically put it, “it is the only scientific journal that *knowingly* publishes satire” (49). Mixed in with straight scientific reports (including experiments on planarians and later, transfer experiments on other animals) were such articles as ‘A Theory of Ghosts’ and ‘Aversive Conditioning in the Dead Rat’. In addition

to these spoofs and satires — some of them with a cutting edge, some of them schoolboy humour — there were cartoons and poems.

Later in its career the straight articles and the spoofs were separated, partly to gain respectability, and partly because some scientists complained of being unable to tell which were serious articles and which were not, until they had nearly finished the piece (50). Viewed from one side the '*Worm Runner's Digest*' (WRD) contained the humour; printed 'upside down' and starting from the 'back' cover, the '*Journal of Biological Psychology*' (JBP) contained straight scientific papers (51). In one sense this could be regarded as little more than an amusing scientific curio of no great consequence. Certainly, few of the important papers in the transfer controversy were published exclusively in the WRD/JBP. In the main, significant papers were published elsewhere, the WRD/JBP carrying modified versions or more informal accounts. In some cases 'early' or 'rough' results were reported in its pages, and in others (a small minority) the journal carried reports by those, like high school students, who would be unlikely to get a paper into an orthodox journal. But for all that, the journal did boast a normal refereeing pattern.

A number of scientists dismissed the WRD/JBP as 'a comic', 'a scientific joke-book', 'a scientific *Playboy*', but these comments should not be taken at face value by the sociologist (52). The journal was important in three respects relevant to this discussion. Firstly, it provided McConnell with an editorial forum (entitled 'Worms and Things . . .') in which to comment on the controversy and articulate his views, a function that was enhanced by the two other factors. Secondly, it carried annotated bibliographies of all research relevant to the running of worms and, later, to the wider memory transfer controversy. As an information source for those interested in following the field it was a 'must'. Lastly, it was sent, sometimes *gratis*, to those involved in the research and to those who might be interested, so achieving a high penetration of the field. The WRD/JBP was thus an institutional sign of McConnell's position at the nexus of an informal communication network for worm running and transfer experiments. Indeed, when a group of interested scientists met in 1967 to consider replication problems in the transfer field they organized an informal reporting network, to be administered by McConnell (53).

The journal also reflects McConnell's approach to science, that it should be fun *as well as* serious, and that the deflation of pretentiousness through

humour is not just compatible with, but can be part of, progress in science (54). The *WRD/JBP* is anomalous precisely in that it brings together two modes of understanding – science and humour – which if not in any formal sense incompatible, are at least habitually held apart and separate (55).

To put science – any science, not specifically memory transfer – together with spoofs and satires is to break accepted categories and taken-for-granted distinctions in a subversive manner. If the *WRD/JBP* were *just* satire and spoof it would have produced unease to the extent that the articles succeeded in showing the fatuity of some aspects of science (56). The effect of putting the subversive (57) humour with apparently straight science was, however, to produce an added dimension of unease, which reflected on the scientific articles.

The *WRD/JBP* as a social institution within the field can be seen as a concrete representation of McConnell's ideas on the relation of scientific creativity and humour. But with regard to the transfer studies, their presentation as straight research was weakened by their physical location amongst satire and spoof (58), *particularly so, given the nature of the conceptual framework of the idea of memory transfer*. The points I wish to make are these: *given* the reputation of the *WRD/JBP* institution, *any* straight piece of scientific reporting would be tinged by placing it in that context; given the particular configuration of cultural resources that constituted memory transfer the effect was multiplied. But the associative context did not in any simple way *determine* the response, for the reputation of the *WRD/JBP* institution itself was partly a concomitant of its being a vehicle for the articulation of memory transfer. It would be difficult, and unnecessary for present purposes, to disentangle this nexus and sort out which aspects were 'causes' and which 'effects'. What can be said is that a deep seated ambivalence towards memory transfer *grew up with* the *WRD/JBP* reputation: the institution acted as an amplifier.

#### 4.2. *Something Unusual . . .*

I have made several claims about ambiguity, unease, violations of taken-for-granted rules, and habitually separated frames of reference. A fertile way of looking at the situation is through the perspective developed by Joan P. Emerson in her paper 'Nothing Unusual is Happening' a resource that has

been used by Collins and Pinch in their study of parapsychologists, who adopt the stance 'nothing unscientific is happening' (59).

Emerson's central hypothesis is that "social interaction has intrinsic properties that naturally bias negotiations towards a "nothing unusual" stance; [and that] this bias inhibits the application of deviant labels" (60). The process of negotiation of appropriate stance in problematic situations is developed through two cameos. In the first a patient is undergoing a gynaecological examination which she sees, and reacts to, as 'something unusual', while the medical staff attempt to pass off the situation as routine, everyday, activity. In the second cameo two would-be robbers interrupt the proceedings at an all female, society 'hi-jinks' party where "[j]okes and pranks filled the evening", (61). The socialites resisted the attempted re-definition of the situation as 'serious' by passing it off as just another prank.

This is a stickup. I'm SERIOUS (one of the robbers) cried. All the ladies laughed. One of them playfully shoved one of the men. He shoved her back. As the ringing laughter continued, the men looked at each other, shrugged and left empty handed. (62)

The gynaecologist maintains a sober medical definition of the situation: under different circumstances the robbers fail to establish that something seriously unusual is happening.

In the memory transfer saga McConnell introduced a number of anomalous elements that did not square with the tacit rules for the conduct of science. Jokes and pranks filled the *Digest*. Should the transfer phenomenon be taken as 'serious' or as 'hi-jinks'? The reception of the reports of memory transfer in mammals can be seen to hover uneasily between the two situations. Like the female participants in both cameos there was an expectation that something unusual was going to happen. One scientist commented that memory transfer was "not an unexpected development, but 'strange' " (63).

A surprising new scientific theory (or revolution) can be seen as an attempted 'hold up' of orthodoxy, but in the ambiguous situation I have described, it was not consensually clear that this was what was going on. I am not claiming that the phenomenon was taken as 'simply a joke' (prank), but, rather, that there was uncertainty. The context bore confusing signs. McConnell had acted like a gynaecologist in fancy dress. 'Nothing unusual' (unscientific) was the necessary stance if the phenomenon was to be taken in

earnest, but once the fancy dress had been seen, it was correspondingly more difficult to pass off the event as a wholly serious attempt at re-definition.

Thus, one of the important tacit rules for doing science is to be seen to be in earnest.

#### *4.3. Spoiling the Act*

I have emphasised the cultural climate of humour that was associated with the *WRD/JBP*, as an index of 'not being in earnest'. In the minds of a good number of critics this was linked with a nexus of what they saw as 'social' factors that included sensationalist popular press accounts of the research, aspects of McConnell's personality, and his performance at conferences. Such interpretive accounts — 'home-grown sociology' as one respondent termed them — were extended to cover other participants, particularly those seen as important.

The truth of these attributions is irrelevant here — but for the sake of symmetry it should be pointed out that proponents used similar modes of explanation in making sense of the actions of the critics. The details of these perceptions will be explored elsewhere, but one point deserves mention. Many felt that McConnell was far too 'open' about the research. In the words of a *proponent* active in the field for some time:

McConnell talks about [the research] in a very casual and annoying way. Such as his first transfer study [in planarians] where he dropped the stuff on the floor, he scooped it up and shoved it back into the animal . . . (64)

Asked at a conference in 1962 whether the 'RNA' injection had been tested for polysaccharides, a possible contaminant, McConnell replied:

You will forgive me if I point out that I am not a biochemist and that I have to depend on my two medical students for information on this point. (65)

Here McConnell can be seen to have dropped his guard, allowing attention to be focused on 'backstage' happenings. Being open, or 'telling it like it was', is not the same as being earnest.

In reading the laboratory notebooks of some of the other scientists involved (on both sides of the controversy) it was noticeable that unfortunate events sometimes occurred during experiments. The air conditioning broke down making the animals uncomfortable, relays failed to deliver food pellets as

they ought, and some animals inexplicably fell sick. By the time the results reached the public (constitutive) forum of science these contingencies had been remedied (66). Order had been created. In these extracts however, McConnell can be seen to have allowed, if not actually encouraged, a glimpse behind the curtain while the scenery was still being wheeled into place, and this has weakened the dramatic illusion. He indulges in ‘telling it like it was’, and being earnest is not the same thing as being honest – nor is it as rhetorically persuasive.

Such events contributed to the possibilities of the audience adopting a ‘something unusual’ stance, and a further series of actions also encouraged such a definition. McConnell noted on several occasions that, in a sense, the regenerated worms could be said to have ‘inherited’ learning, a notion that sails perilously close to the Lamarckian Heresy. One scientist from the ‘Bible Belt’ of the United States described it as a “Church-burning” idea in the context of the mid-1960’s, because of its association with Lysenko and the Communist Threat. McConnell’s group went further with their indifference to orthodoxy by beginning a series of experiments to test the possibility of the transfer of memory through *sexual* rather than *a-sexual* reproduction, a very pointed challenge to neo-Darwinians beliefs. Perhaps fortunately, there were problems:

We got a species [of planarians] from Buckhorn Springs. Big worms. But as soon as we started training them they stopped mating. If we stopped the training they mated. It makes you wonder about the value of an education. (67)

The group made no further attempts in this direction, as McConnell explained:

The word came through loud and clear. If we succeeded we would really be in trouble. (68)

In these extracts McConnell *can be seen* to have followed a subversion strategy (69) not only in the constitutive forum, by transgressing ‘cognitive and technical norms’, but also by allowing backstage contingencies to show through. The latter factor is, I think, the more important. It is a violation of the ‘rules’ of the institution of science itself, rather than an attempted subversion of a particular theory – a process which is expected to take place *within* the rules.

Once more; one must be *seen* to be in earnest (70).

## 5. Structures of Interests in Reactions to the Phenomenon

I hope I have outlined the structure and established a major source of the widespread ambivalence that greeted the reports of the memory transfer phenomenon in mammals. In this section I want to look in greater detail at the slightly wider context in which the research was situated, in order to show more clearly why it could be seen to stand out as anomalous. This will involve showing why the ambiguity characteristic of the initial reception of the mammal experiments (roughly mid-1965 to the latter part of 1966) was not precipitated by the earlier planarian research. To some extent this will be a restate and more firmly ground the themes so far developed, but in doing so I will be engaging in a process that is akin to explaining an in-joke. The explanation destroys the force of the humour. The transfer experiments should now seem more serious.

### 5.1. *The Worm Running Experiments and the Containment of Implications*

I have indicated above (p. 169) that the implications of the phenomenon were to a large extent contained whilst it was associated with planarian worms. Before the experiments in higher organisms, such as the rat, most of the scientists concerned with worm running were psychologists and zoologists, and the arguments of critics were directed not so much against the notion of memory transfer itself, but the issue of whether the planarians could be said to have accomplished true learning, rather than some 'non-learning' phenomenon such as pseudo-conditioning or sensitisation. This issue related directly to the technical and conceptual competences of behavioural psychologists, for example. The reason behind the lack of criticism (by later standards) of the implied theory of a molecular memory mechanism has much to do with its *disjunction* from their main disciplinary interests. Some indeed were happy with a basically 'black box' approach of the kind associated with B. F. Skinner. What might be inside the black box of the brain was not of immediate consequence for their disciplinary practice. Others, (McConnell for example) were concerned to put a mechanism inside the black box, a practice for which there is also a time-honoured tradition in physiological psychology. (The postulation of neural mechanisms that might account for learning – which has been called 'carefree neurologising' – should not be seen

as contradicting my point about the disjunction of this concern from the mainstream of psychology. It is just that the scientific culture is relatively open on this point.) Though there was *individual* commitment to particular models and mechanisms of brain function, at a *disciplinary* level there was no uniform consensus except in the most general terms. It was *then* an open – and by later standards widely ignored – question as to whether the transfer phenomenon was in fact compatible with this general perspective, or anomalous (71).

As far as the group of scientists who concerned themselves with the planarian research were concerned, there was no general disciplinary constraint motivating involvement in the memory mechanism issue in the constitutive forum. However the knowledge that the planarian research apparently *could* have widespread implications for a mechanism of learning and memory helps to account for the *saliency* of the worm running saga.

This is only superficially a paradoxical point. There was a good deal of popular press publicity which dwelt for instance on the associated Lamarckism, and which cited McConnell's speculations about the implications for human learning and memory, including the notion of 'artificial memories'. For many such extrapolation belonged in the realm of science fiction rather than science fact; and this no doubt helped to raise the temperature of the debate. However, the reaction in the formal scientific reports (72) was conducted with legitimated cultural resources. If the results could be shown to be a consequence of non-learning phenomena, or if planarians could not be reliably trained (73), then the basis for extrapolation (flights of fancy) was undercut, and the implications contained. Had the planarian results achieved a greater scientific legitimacy, then some of the implications might have been considered more seriously, as was the case with the mammal experiments. (Though even if learning in planarians had been accepted as demonstrated, it would of course have been open to scientists to find *other* reasons for denying the implications – learning and memory might be 'different' in planarians for instance.)

This background helps, I think, to account for why the implied memory mechanism does not so easily stand out as anomalous, irrespective of whether it was believed or not. To put it another way, the scientific culture within which worm running was located in the early days was such that the umbrella and the sewing machine on the operating table were just a collection of (cognitive) objects which, granted, had no special reason to occupy the same (conceptual) space, but had no special reason *not* to be together, either (74).



### 5.2. The Mammal Experiments and the Extrapolation of Implications

The experiments with mammals such as rats reported in 1965, led to structural and institutional changes in the field that can be related to the interests and technical competences of the scientists who then joined the research area. Many of them were *directly* concerned with the question of memory mechanisms, but few of them had any primary commitment to psychology. To be sure, many had some background or training in psychology, but increasingly this was in harness with, or secondary to, expertise in biochemistry, pharmacology, or 'neuroscience'. A separate development is that the period from the mid-1960's to the 1970's marks the creation of a much more integrated approach to the study of brain function, and the creation of what some have called 'molecular neurobiology' (75). These features were visible in the transfer research. Reviewing the field in 1971 Bryant and Petty commented that:

by 1969, among transfer-related publications for which we could determine disciplinary affiliation, only 2 of 16 were by psychologists, the rest were by biochemists, physiologists, and pharmacologists. (76)

Thus the disciplinary background against which the mammal transfer experiments were judged was different to that of the disputes over the planarian research in the early 1960's.

I will now examine two further aspects of the context of the research and the possibilities of extrapolation. Firstly, when the planarian research began, events such as the Watson and Crick papers on the structure of DNA and the coding possibilities of macromolecules were well off-stage. By 1965 the spectacular rise of molecular biology in breaking the genetic code was taken to hold enormous promise as a model for a new endeavour: cracking the brain's memory code. The atmosphere is captured by Bonner in his Presidential Address to the Pacific Division of the American Association for the Advancement of Science:

And so, brain biology is the next great challenge – the challenge to break the brain code. It will be an enormous task, but it is already clear that it can be accomplished. (77)

He also noted:

It has of course been suggested that information stored in memory is stored in the form

of new RNA molecules which then contain the [experiential] data written out in RNA language. *No thought is so fantastic that the molecular biologist should not try thinking it for a while.* (78)

Bonner goes on to doubt that memory molecules exist, suggesting his own scheme. The point however is that visions of a new heroic age and fundamental novelty were not confined to the transfer field by any means. Rather, they are part of the background against which it operated, and of relevance to its reception. On this account one might expect that the transfer phenomenon would be *readily* received – it seems to have a good ‘fit’ with the conceptual structure and great expectations I have outlined, at least *prima facie*. As evidence for this fit, some scientists, though critical of the experiments in the field, maintain that the idea of a molecular memory code is a reasonable, logical extension of the genetic code, deserving of serious attention. More often these days however the transfer notion is held up as a kind of *reductio ad absurdum* of the idea of molecular memory code. After Kuhn, we are familiar with the notion that the ‘same’ data may be seen in radically different ways, and here we have a related case (79).

The *possibility* of these divergent attributions can be seen in the second piece of context. Consider the relationships between the various theories of molecular memory mechanism described by Bogoch:

perhaps in no other scientific endeavour have so many propositions managed to appear so lacking in conflict with each other. (80)

The transfer phenomenon and its associated theoretical implications were injected into this scientific culture of ‘free floating’ theories. Expectations were high, but they were diffuse. Further, unlike the other notions, the transfer phenomenon was seen to have experimental consequences that were directly testable. It held the promise of an exemplar (81) on which future research *could* be based. But of more importance for present purposes, the model channelled speculations towards the implications of the notion of transferrability. A number of consequences that *can now be seen* as implicit in molecular memory mechanisms were raised in a pointed form, and these implications ran far beyond expectations. Rose, writing in the *New Scientist* noted that Jacobson’s experiment (one of the first four mammal studies) was “at first sight unbelievable”. Later in the same article he argued that:

it would not only mean that specific RNA coding molecules existed, but that they had an identical interpretation not only in the brains of individual animals of the same species, but *across species* as well, i.e. that a learning response always and inevitably produces a defined and unique RNA molecule with one and only one specific interpretation. The prospect of students cannibalising their teachers' RNA would suddenly become debatable. Equally, many people might have second thoughts about the virtues, of including, say, grilled beef brain, on their menu. (82)

Some took the phenomenon to imply a universal coding scheme, as if the putative transfer molecules were the physical representations of Platonic Ideal Forms. To have certain kinds of memories would be to have certain kinds of molecules in the brain. When questioned on this point many scientists were non-committal about such 'philosophical' issues, but were not unhappy, and in some cases positively agreeable with my suggestions that the transfer phenomenon could be seen as implying a molecular representation of some kind of Chomskian deep structure.

Whichever way one looks at the results they were surprising. Bonner (above) notes that cracking the brain's memory code would be an enormous task. Memory transfer arrived almost as he was speaking. Bonner called the RNA-as-memory-molecule idea 'fantastic' just as a group of scientists were proclaiming that the revolution was not only at hand, but all over bar the shouting! As I see it, diffuse expectations were un-expectedly given the possibility of concrete content, allowing extrapolation which, in some, could induce a kind of intellectual vertigo.

But this is only one view. There were, as always in science, plenty of available cultural resources with which to deny the implications, or alternatively, to recast them as reasonable. It *could* be argued that the phenomenon was false, not reproducible, non-specific, applied only to certain simple or primitive kinds of behaviour, or that the radical disjunction between cognitive (brain) and emotional (hormonal) phenomena was misplaced. Alternatively, on the grounds of *parsimony* (always a good device), one would *expect* Nature to use the same or a similar memory code in different species. And *why not* molecular 'deep structures'?

It is apparent from interviews and the literature that, while some scientists saw memory transfer as scientific creativity, others regarded it as an absurdity. There was, however, an acceptance that the phenomenon represented a 'logical' extension of informational coding themes in molecular biology. Proponents saw the inferences as perfectly reasonable. Some critics agreed, seeing it as

reasonable but mistaken. For others the logic was of a twisted kind. "A sadist is a person who is kind to a masochist" captures it nicely (83).

Should memory transfer be taken to be a chimerical monster, or a new natural kind? Science is about creating the impossible, and after the surprise had faded, and the incongruity had collapsed into laughter, various groups began work on the further construction and deconstruction (84) of memory transfer. Incongruity was replaced by ambivalence, and rest was renegotiation.

## 6. Concluding Comments

In this paper I have sought to show how a certain structure of ambiguity grew up with the planarian transfer experiments. McConnell's violations of certain tacit rules for doing science reciprocally amplified the perceived incongruity of the transfer phenomenon, so structuring the initial reception of the mammal studies. The violations should also be seen as part of McConnell's heterodox personal approach to science, and that should not be under-valued. In Popperian terms for example, the bold and falsifiable hypotheses of memory transfer were the epitome of good science (85).

Two further sets of comments are in order: one on the notion of memory transfer, and the other on the role I have assigned to McConnell.

Firstly, I have shown in Section 5 above why the idea of memory transfer was anomalous for those concerned with memory mechanisms, and why it was not nearly so anomalous *in terms of disciplinary interests and competences* for psychologists in the worm running controversy. But most people — scientists or not — hearing of the experiments for the first time, react with humour and surprise. This is a kind of baseline to which the scientific reactions should be related.

It seems that in our culture the phenomenon is just odd. Why this should be so is, strictly, beyond the scope of this paper, but it is no doubt due in part to the fact that 'everybody knows' that memory is about impulses in an organised structure of nerve cells in the brain. We also 'know' that our memories are unique (86). In a comment on this paper Krohn has noted that the conceptual violations implied by memory transfer compare with those of Darwin's evolution/natural selection model and the discovery of animal to man disease contagion. However, much more analysis would be required to properly ground these suggestions.

To turn now to McConnell, I have described his actions as having been influential in producing a structure of ambiguity where radically divergent definitions of the situation were possible. But that is not the same thing as saying that he caused the rejection of memory transfer. Science is far more subtle than that! If the worm running saga had been played 'straight', the mammal experiments would still have produced some surprise, and even shock. No doubt there would also have been a certain amount of humour associated with the reaction (this would be expected on Koestler's analysis), but this would probably have been seen as *epiphenomenal*. What McConnell did was to attach the epiphenomenal social factor to the idea of memory transfer in a thorough-going way, and it took some time to cleave the two apart again. Indeed, the separation was never complete, and for a considerable time the atmosphere of 'not being in earnest' was available as a satirical resource for those wishing to attack the field.

What can be said is that without the comic connection, the open-ness at scientific forums, and the implicit Lamarkism; and perhaps with a more neutral term like 'transfer of response bias', the reaction would have been considerably muted. One set of resources in the rhetoric of rejection would have been denied, though others were available. That is, the ambivalence was certainly a significant factor in the rejection of memory transfer — but it was not a sufficient condition.

In examining this series of events I have attempted to remain neutral about the 'reality' of the phenomenon. Naturally, I find it unexceptionable that rational actors should hold widely divergent views. I do not want to devalue the reactions of scientists on either side of the controversy. None of them treated it as *just* a joke. Indeed there was a serious intent behind the humour. As McConnell put it:

today, Science stands fair to join Religion, Motherhood, and the Flag as a domain so sacrosanct and sanctimonious, that leg-pulling isn't allowed, levity is forbidden, and smiling is scowled at. (87)

... now perhaps you see the *Digest* for what it really is: the house organ of an anti-Scientific movement ... only when we learn to laugh at ourselves can we proceed to slaughter all those Sacred Cows and turn Science back into science. (88)

### Postscript

In a long and interesting response to this paper Professor McConnell agreed

with most of the points made, and emphasised the bitterness of the reactions among psychologists, to the worm running experiments, and among other scientists, to the later research on mammals.

Having been shocked to the core by responses such as those given me by Nobel Laureates, I'm sure I defended my ego by resorting to humour. Part of my wit was bitter attack. Part of it – mostly the self-deprecating type – was little more than the same 'submission' response that a young wolf shows to the pack leader when he bares his throat before the leader's teeth. I became a court jester in self-defence. (89)

In personal, psychological, terms the humour was more of an *effect* of the reception of memory transfer than a *cause*. McConnell also noted that "the humorous approach of the *Digest*, while it caused unease, did at least let us barely survive while the research went on" (90).

McConnell's reading of the bitterness of the reaction to worm running conforms to mine, and I find his personal (psychological) account both compatible with the sociological account given in this paper, and with the logic of his situation. To go any further one would need a biography to show why he reacted in terms of humour rather than in other ways, and that is beyond my compass. His comments are however a poignant reminder to the sociologist that there is always more that can be said.

### Acknowledgements

It is a pleasure to offer thanks to Harry Collins, Peter Glasner and Trevor Pinch for their comments, and to the scientists who spared the time to talk to me. As is obvious, I owe a special debt of gratitude to Professor James V. McConnell of the University of Michigan.

### Notes and References

1. The research reported is part of a case study carried out whilst a graduate student at the University of Bath. G. D. L. Travis, *The Sociology of 'Memory Transfer'*, Ph. D. thesis forthcoming.  
I had assumed that I had constructed this title directly from available cultural resources, but must have been unconsciously influenced by A. Koestler, *The Act of Creation*, Pan Books, London, 1969. Koestler stresses 'The Importance of not Being Earnest' (pp. 63–64) in freeing man from the rails of instinct.
2. 'Memory transfer' is the popular label for the field, but some proponents have

- sought to suppress it in favour of more neutral terms such as 'transfer of behavioural bias'. The phenomenon has been rejected, but not 'disproved' of course. Even the strongest critics were virtually unanimous in their belief that the phenomenon did not exist, *and* that they could not prove the point.
3. G. Chedd, 'Scotophobin – memory molecule or myth?', *New Scientist*, 240–241 (August 1972).
  4. A readable account of this and other experiments is to be found in J. V. McConnell, 'The Biochemistry of Learning', *Das Medizinische Prisma* 3, 1–22 (1968).
  5. According to Hebb's theory that learning is a matter of reshuffling the connections between neurons. See McConnell, *op. cit.*, 1968, Note 4; D. O. Hebb, *The Organisation of Behaviour*, Wiley, New York, 1949.
  6. R. Thompson and J. V. McConnell, 'Classical Conditioning in the Planarian, *Dugesia dorotocephala*', *Journal of Comparative and Physiological Psychology* 48 (1), 65–68 (1955).
  7. D. L. Krantz, 'The Separate Worlds of Operant and Non-Operant Psychology', *Journal of Applied Behaviour Analysis* 4, 61–70 (1971).
  8. J. Blundell, *Physiological Psychology*, Methuen and Company, London, 1975, p. 34.
  9. The number of trials required to reach a given performance criterion is compared with the (greater) number required by regenerates of untrained worm. The differences is taken to be a measure of learning. Other control groups would normally be included in the experiment.
  10. J. V. McConnell, R. Jacobson and D. P. Kimble, 'The Effects of Regeneration upon Retention of a Conditioned Response in the Planarian', *Journal of Comparative and Physiological Psychology* 52, 1–5 (1959).
  11. J. V. McConnell, 'Memory transfer through cannibalism in Planarians', *Journal of Neuropsychiatry* 3 (Suppl. 1), 42–48 (1962).
  12. Priority did not become a *public issue*. The publication, and more importantly, the acceptance dates are quite clear, but in interviews there was discussion about the real *quality* of the research reported, the differential *visibility* of the claims, and the fact that the results in one paper had been read at a conference in 1964. See R. K. Merton, 'Priorities in Scientific Discovery', *American Sociological Review* XXII, 635–59 (1957); 'Singletons and Multiples in Scientific Discovery', *Proceedings of the American Philosophical Society* 105, 470–86 (1961).
  13. They were F. R. Babich, A. L. Jacobson, S. Bubach and A. Jacobson, 'Transfer of a response to naive rats by injection of ribonucleic acid extracted from trained rats,' *Science* 149, 656–7 (1965); E. J. Fjerdingsstad, T. Nissen and H. H. Roigaard-Petersen, 'Effect of ribonucleic acid (RNA) extracted from the brain of trained animals on learning in rats', *Scandinavian Journal of Psychology* 6, 1–6 (1965); S. Reinis, 'The formation of conditioned reflexes in rats after parenteral administration of brain homogenate', *Activitas Nervosa Superior* 7, 167–68 (1965); G. Ungar and C. Ocegura-Navarro, 'Transfer of habituation by material extracted from brain', *Nature* 207, 301–2 (1965). Ungar and Ocegura-Navarro used rats as donors and mice as recipients; the others used rats only. (Later experiments involved other inter-species transfers.) As the above titles indicate, the transfer experiments in vertebrates were accomplished by injecting a recipient with a homogenate or solution of brain taken from a trained animal.

14. A sociological perspective on replication, especially in controversial areas of science, is to be found in H. M. Collins 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology* 9 (2), 205–224 (1975). An extension of that perspective to the memory transfer phenomenon is to be found in G. D. L. Travis 'Replicating Replication? The Case of Memory Transfer', to be published in *Social Studies of Science*.
15. The information is taken from J. A. Dyal, 'Transfer of behavioural bias: reality and specificity', in E. J. Fjerdingstad (ed.), *Chemical Transfer of Learned Information*. North-Holland, Amsterdam, 1971, pp. 219–264. The memory transfer field is an almost legendary case of problems over replication. See I. St. James-Roberts, 'Are researchers trustworthy?', *New Scientist*, 481–3 (2nd September 1976).
16. G. Ungar, D. M. Desiderio and W. Parr, 'Isolation, identification and synthesis of a specific-behaviour-inducing brain peptide', *Nature* 238, 198–202 (1972).
17. See for example G. Ungar, 'Peptides and Behaviour', in C. Pfeffer and J. R. Smythies (eds.), *International Review of Neurobiology*, Vol. 17. Academic Press, New York, 1975, which discusses some of the other behaviour-inducing peptides. H. N. Guttman and J. R. Cooper, 'Oligopeptide control of step-down avoidance', *Life Science* 16, 914–24 (1975) makes the claim to have isolated a further peptide 'Catabathmophobin'. Some of the conclusions of Ungar *et al.*, *op. cit.*, 1972, Note 14, are disputed in H. N. Guttman, B. Weinstein, R. M. Bartschott and P. S. Tam, 'Reputed rat scotophobin prepared by a solid-phase procedure shown invalid by comparison with a product derived from a classical synthesis on the basis of physical and biological properties', *Experientia* 31, 285–90 (1975).
18. For a discussion of tacit knowledge see M. Polanyi, *Personal Knowledge: Towards a Post Critical Philosophy*, Harper and Row, London, 1958. J. R. Ravetz, 'Scientific Knowledge and its Social Problems, Penguin Books, Harmondsworth, 1973, contains a discussion of the related notion of 'craft knowledge'. See also H. M. Collins, *op. cit.*, 1975. Note 14, and 'The TEA Set: Tacit Knowledge and Scientific Networks', *Science Studies* 4, 165–86 (1974).
19. The notion of cannibalism carries its own unsettling connotations of course.
20. Feeding a homogenate of 'trained brain' to mammals would not work since digestive enzymes would break down the molecules thought to be involved. The planarian digestive system does not break down food in this way before taking it up. It was thought possible at one time that viable 'trained' cells were incorporated into the recipient worm – more of a transplant than a transfer.
21. Some of the factors relevant to the initial containment and subsequent 'explosion' are dealt with in Section 5 below.
22. There is an extended discussion of monsters and monster barring techniques in mathematics in I. Lakatos, *Proofs and Refutations. The Logic of Mathematical Discovery*, Cambridge University Press, Cambridge, 1976.
23. I should make it clear that the 'memory transfer' field is quite distinct from the biological speciality which studies chimeras or 'genetic mosaics'.
24. In using the notions of conceptual space I am not implying a picture of pre-existing finite space waiting to be 'filled', a point made against Mulkay by Law and Barnes. I assume the geometry of conceptual space is distinctly non-Euclidian and created by the actors involved! See M. J. Mulkay, 'Three Models of Scientific Development', *Sociological Review* 23, 509–26 (1975); J. Law and B. Barnes, 'Research Note:



- Areas of Ignorance in Normal Science: A Note on Mulkay's 'Three Models of Scientific Development', *Sociological Review* 24, 115–24 (1976); M. J. Mulkay, 'The Model of Branching', *Sociological Review* 24 125–33 (1976).
25. This famous image of the Comte de Lautréamont symbolised the enigmatic linking together of objects which had no previous connections with each other, in order to produce ambiguity, and 'exceptions to the physical and moral order'. This was adopted as a theme by the Surrealists, who also sought to break the distinctions between reason and absurdity, seriousness and humour. See *Lautréamont's 'The Maldoror'*, translated by A. Lykiard, Allison and Busby, London, 1970; S. Gablik, *Magritte*, Thames and Hudson, London, 1977, pp. 45–46. An alternative way of conceptualizing some of the points in this paragraph is through the work of M. C. Escher, particularly the impossible buildings, e.g. 'Ascending and Descending', 1960; 'Waterfall', 1961. Taken separately the parts seem reasonable, but the particular relations Escher produces radically alter the 'meaning', making them 'impossible'. Reproductions of the prints are to be found in, for example, M. C. Escher, *The Graphic Works of M. C. Escher*, Pan Books, London, 1978.
  26. M. R. Rosenzweig, 'Discussion'. In S. Bogoch (ed.), *The Future of the Brain Sciences*, Plenum Press, New York, 1969, p. 321. Emphasis added.
  27. Interview material, 1976.
  28. Interview material, 1976.
  29. I am not trying to argue that this was the only possible response, or to suggest that scientists perceived what was going on in terms of umbrellas and sewing machines on operating tables, any more than they perceive major theoretical controversies in terms of ducks and rabbits. However I do claim that this is a valid sociological interpretation, and that the 'happening' involved a confusion of contextual signs relevant to the application of categories such as 'scientific creativity' and 'joke'.
  30. A. L. Jacobson, 'Learning in Planarians: Current Status', *Animal Behaviour* (Suppl. 1), 78 (1965).
  31. Interview material, 1977.
  32. Interview material, 1976.
  33. Many results – probably more 'negative' than 'positive' were never published.
  34. The notion of structural ambiguity has been developed from the discussions of 'systematic ambiguity' in P. Winch, *The Idea of A Social Science and its Relation to Epistemology*, Routledge and Kegan Paul, London, 1971, pp. 18–33, but also has an affinity with the notion of 'sociological ambivalence' in R. K. Merton and E. Barber, 'Sociological Ambivalence', in E. Tiryakian (ed.), *Social Theory, Value and Sociocultural Change*, Free Press, New York, 1963, pp. 91–120. "In its most extended sense, sociological ambivalence refers to incompatible normative expectations of attitudes, beliefs and behaviour assigned to a status or set of statuses in society" (p. 95). From this, the idea of norms and counter-norms is developed. (See also R. K. Merton, *Sociological Ambivalence and Other Essays*, Free Press, New York, 1976, Chaps. 1–3; I. Mitroff, *The Subjective Side of Science*, Elsevier, New York, 1974.) I am not here concerned with the terminology of statuses and roles; and 'polyvalence' rather than the oppositional pairs suggested by 'ambivalence' would, strictly, be more appropriate. Insofar as norms of science are implied in this paper they are treated as part of the 'social rhetoric of science' as in, for example,

- M. Mulkay, *Science and the Sociology of Knowledge*, George Allen and Unwin, London, 1979, pp. 21–26, 63–73.
36. A. Koestler, *The Ghost in the Machine*, Pan Books, London, 1967; *The Act of Creation*, *op. cit.*, 1969, Note 1.
  37. Roger Krohn's paper was circulated to participants before the conference. R. Krohn, 'The Social Processes of Scientific Investigation' unpublished paper, McGill University, June 1978.
  38. J. R. Ravetz, *op. cit.*, Note 16, pp. 94–191.
  39. H. M. Collins and G. Cox, 'Recovering Relativity: Did Prophecy Fail?', *Social Studies of Science* 6, 423–45 (1976).
  40. In this paper I have assumed an *interactive* theory of metaphor. See W. H. Leatherdale, *The Role of Analogy, Model and Metaphor in Science*, North Holland, Amsterdam, 1964; D. A. Schon, *The Displacement of Concepts*, Tavistock, London, 1963.
  41. S. B. Barnes, *Scientific Knowledge and Sociological Theory*, Routledge and Kegan Paul, London, 1974, p. 86.
  42. See M. Foucault, *The Order of Things*, Tavistock, London, 1970, p. xvi for a similar point.
  43. A. Koestler, *op. cit.*, 1969, Note 1, p. 88.
  44. Since writing this I have come across a paper by E. Wright, 'Sociology and the Irony Model', *Sociology* 12, 523–43 (1978). Wright sees the need for a model of rationality that can handle inconsistency, misunderstanding and 'falsity' as well as consistency, understanding and 'truth' (p. 540, my scarequotes), and discusses the *joke* in terms of ducks and rabbits. Indeed, Wright seems to draw on many cultural resources that are also used in 'relativistic' sociology of science.
  45. A. Koestler, *op. cit.*, 1969, Note 1, p. 90.
  46. Roughly, the explanation of the known by the still less known.
  47. A fuller account is given in J. V. McConnell, 'Worms and Things . . .', *Worm Runner's Digest*, 11(1), 1–4 (1969).
  48. The other long running example is the *Journal of Irreproducible Results*; and closer to home there is the *Subterranean Sociology Newsletter*. Further details of these and other journals are to be found in E. Garfield, 'Humour in Scientific Journals and Journals of Scientific Humour', *Current Contents*, 5–21 (20th December 1976). Sadly, the *Worm Runner's Digest* ceased publication in 1979.
  49. Interview material, 1976.
  50. Interview material, 1976.
  51. The relation between the science and the humour – mixed up, or back to back – can be seen as an embodiment of the relation of frames of meaning that Koestler describes. There was a homology between the structure of the idea of memory transfer and the *WRD/JBP*.
  52. Though many of the scientists interviewed appreciated some of the humour, few were whole-heartedly enthusiastic about the *WRD/JBP*. On the other hand, B. F. Skinner and Michael Polanyi have been numbered among its supporters, and Arthur Koestler has written "One of the last Palinurian joys of civilised middle age is to sit in front of the log fire, sip a glass of brandy, and read *The Worm Runner's Digest*."
  53. Another characteristic of the attempted institutionalisation of a new scientific

area of course. I do not mean to suggest that McConnell was the sole motivator in this institutionalisation. W. L. Byrne, then of Duke University, was especially important in arranging seminars to tackle the replicability problem, and in organising a session at the 1967 AAAS.

54. Escarpit has argued that humour in science is necessary in order to remain intellectually open – it changes the angle of view of reality. R. Escarpit, 'Humorous attitude and scientific inventivity', *Impact of Science on Society* 19 (3), 253–58 (1969). See also Koestler, *op. cit.*, 1969, Note 1.
55. This is not to deny the 'white haired eccentric scientist' and 'schoolboy humour' syndromes in science. Such 'deviations' are in the category of 'honoured exceptions' and not seen as *constitutive* of the science.
56. To the extent that satires failed as good demolition jobs they could bring the journal itself into dispute.
57. McConnell felt that the humour of the Digest was mainly 'gentle'. Reactions among interviewees (about 70) varied, from amusement and approval, through indifference to mild irritation and annoyance. Questioned about the role of the *WRD/JBP* in the controversy many felt that 'it hadn't helped', and McConnell himself was widely seen among critics to have been engaged in 'knocking' the establishment.
58. The inclusion of 'non-scientific' resources and thoughtful counter-points is not, of itself, subversive of the high-minded intentions of science. The journal *Perspectives in Biology and Medicine* for example publishes poetry between serious scientific papers. (The Autumn, 1970, issue contains M. Lipman 'Latent Thumb-Sucking – a New Chimerical Syndrome', pp. 86–97, which the author states "was taken seriously by three psychiatrists and two associate professors of medicine, and one professor of pediatrics. Two medical students laughed heartily.") As a related point, many scientific diagrams are caricatures in the sense of being 'one-sided' exaggerations designed to make a particular point. See S. B. Barnes, 'Science, Ideology and Pictorial Representation' unpublished paper read to the Conference on the Sociology of Science, University of York, September 1975.
59. H. M. Collins and T. J. Pinch, 'The Construction of the Paranormal: Nothing Unscientific is Happening', in R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge*, The Sociological Review Monograph No. 27, Keele, 1979, pp. 237–70.
60. J. P. Emerson, 'Nothing Unusual is Happening', in T. Shibutani (ed.), *Human Nature and Collective Behaviour*, Prentice-Hall, Englewood Cliffs, 1970, pp. 208–22.
61. *ibid.*, p. 209.
62. *ibid.*, p. 217.
63. G. Ungar, 'Introduction' to *Molecular Mechanisms in Memory and Learning*, Plenum Press, New York, 1970, p. viii.
64. Interview material, 1976.
65. J. V. McConnell, 'Discussion'. In M. A. B. Brazier (ed.), *Brain Function, Volume 2: RNA and Brain Function, Memory and Learning*, University of California Press, Berkeley, 1964, p. 178.
66. A distinction between contingent and constitutive forums in science is made in Collins and Pinch *op. cit.*, 1979, Note 52.
67. Interview material, 1976.

68. Interview material, 1976.
69. See P. Bourdieu, 'The Specificity of the Scientific Field, and the Social Conditions for the Progress of Reason', *Social Science Information*, 14 (6), 19–47 (1976).
70. Some aspects of the humorous style of McConnell and several of his associates were occasionally visible in their reviews of the field. In reviewing a book containing a number of memory transfer papers, Brindley complained that the papers were "in several cases marred by a strange facetiousness." G. B. Brindley, 'Chemical Mnemology', *Nature* 228, 583 (1970).
71. McConnell (and others) are seen as having been contradicting the orthodox belief that memory is stored in a specific structure of neurons. Ungar's formulation is that the transfer factors (peptides, not RNA) act as signposts or chemical markers within the neural structure and that the phenomena is perfectly compatible with, but an extension of, orthodox views. The relation between these views are explored in G. D. L. Travis, 'Creating Contradiction, or why let things be difficult when with just a little more effort you can make them seem impossible', unpublished paper read to the British Sociological Association, Sociology of Science Study Group, Manchester, February 1978. For the establishment of an anomaly in physics see T. J. Pinch, 'Theoreticians and the Production of Experimental Anomaly: The Case of Solar Neutrinos', this volume.
72. While memory transfer research reports were stylistically fairly 'normal' a proportion of reviews of the field are unusual in adopting a narrative and sometimes anecdotal style. See also Note 70.
73. Bennett and Calvin published a report 'Failure to Train Planarians Reliably' concluding that the animals were [then] of little use in studies of the biochemical bases of learning. This paper is often cited as a 'knock-down of the worm-running experiments, but in interviews I did not find a single scientist who was not prepared to accept that planarians could in some sense be trained. E. L. Bennett and M. Calvin, 'Failure to Train Planarians Reliably', *Neurosciences Research Program Bulletin*, 3–24 (July–August 1964). A more detailed account of the events surrounding this paper is to be found in my paper cited in Note 14 above.
74. This point relates to the analysis to be found in R. Whitley, 'Cognitive and Social institutionalisation of scientific specialities and research areas', in R. Whitley (ed.), *The Social Processes of Scientific Development*, Routledge and Kegan Paul, London, 1974, pp. 69–95.
75. There have been arguments about appropriate titles both in what I have called the transfer field, and in the wider area of the study of the biological (biochemical, neurological, . . . , etc.) bases of learning and memory. Whitley notes this as a general feature of biology. "The difficulty that biologists, in particular have in naming their specialty attests to the ambiguity of cognitive and social structures in this field . . . . . and may well occur in others." R. Whitley, *op. cit.*, 1974 Note 74, p. 91. See also Note 2 above.
76. R. C. Bryant and N. Petty, 'Field in Transition' [Review of Fjerdningstad 1971, *op. cit.*, Note 15.] *Journal of Biological Psychology* 13 (2) 50–52 (1971).
77. J. Bonner, 'The Next New Biology', reprinted in *Plant Science Bulletin* 11 (3), 1–8 (1965).
78. *ibid.*, p. 6. Emphasis added.
79. A related case, because the controversy over the transfer phenomenon does not

- easily fit Kuhn's notion of a paradigm clash. See T. S. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago, 1970.
80. S. Bogoch, *The Biochemistry of Memory*, Oxford University Press, New York, 1968, p. 81.
81. See T. S. Kuhn, 1970, *op. cit.*, Note 79.
82. S. P. R. Rose, 'Is Learning Transferable', *New Scientist* 781–83 (16th December 1965).
83. Quoted in Koestler, *op. cit.*, 1969, Note 1, p. 197.
84. An excellent account of this kind of process is to be found in B. Latour and S. Woolgar, *Laboratory Life*, Sage Publications, London, 1979.
85. See K. R. Popper, *The Logic of Scientific Discovery*, Hutchinson, London, 1972. I should say that I do not in general adhere to the Popperian scheme, but I agree with the implication in this case.
86. Whether or not memory transfer contradicts these ideas, or rather scientific refinements of them, is a matter of debate. See Note 71.
87. J. V. McConnell (ed.), *The Worm Re-turns*, Prentice-Hall, New York, 1965.
88. J. V. McConnell, 'Confessions of a Scientific Humorist'. In J. V. McConnell and M. Schutjer (eds.) *Science, Sex and Sacred Cows*, Harcourt Brace Janovich, New York, 1971, pp. 8–9.
89. J. V. McConnell, personal communication, 9th September 1979.
90. *loc. cit.*