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A FAREWELL EDITORIAL

With this issue (17.4) *Contemporary Hypnosis* has reached the tenth year of publication under its present name and I have been its editor for the past five of those years. As I noted in my incoming editorial, I took over as editor in 1996 from Brian Fellows with a sense of a great honour being bestowed on me, followed swiftly by the realization that Brian was an almost impossible act to follow. Nevertheless, with the support of an excellent editorial board and an understanding publisher I seem to have survived. However, rather than tempting providence by trying to emulate Brian's 16-year stint,¹ I have decided to retire at this point and to hand over the editorship to someone else.

I am pleased, therefore, to announce that from issue 18.1 Professor John Gruzelier will be the new editor of *Contemporary Hypnosis*. John is based in London, at Imperial College of Science, Technology and Medicine, where he is the head of the Department of Cognitive Neuroscience and Behaviour. He has been a frequent contributor to our journal and has served as one of its associate editors from the start of my editorship in 1996. The range of his research interests outside hypnosis is impressive and includes the neuropsychology of schizophrenia, psychoneuroimmunology and recently the media have been excited about his work on neurofeedback and its relationship to performance in young musicians. As readers of this journal will be well aware, he is an important contributor to the ongoing theoretical debate about the nature of hypnosis, and in particular to the question of its neuropsychophysiological underpinnings. Importantly for a journal that serves the needs of practitioners as well as researchers, his work also has strong links to clinical practice. It is reassuring for me to leave the journal in such good hands and I wish John every success in taking *Contemporary Hypnosis* on to new heights.

It is no secret that Brian Fellows as editor was a staunch supporter of the non-state or sociocognitive approach to 'hypnosis' and in my first editorial I speculated that Chevreul's pendulum might be swinging back towards what I called a 'neo-state' view. In my own editorial decisions I have tried to reflect the recent moves towards a reconciliation between these two positions so that the advocates of either or both will feel equally at home in these pages. Those familiar with John Gruzelier's writings will know that he too advocates reconciliation – although it is equally no secret that his theoretical position is closer to a special process view than Brian would ever have dreamed of entertaining. I will follow future developments with interest.

A brief look backwards

In my first editorial I promised to leave the basic structure of the journal alone, as it was working well. One early change, however, was to move from three issues to four issues a year in 1997 in order to bring the publication schedule of *Contemporary Hypnosis* into line with that of other professional journals. To keep costs down this increase in the number of issues a year was accompanied by a more modest increase in the number of pages (12% compared with the previous year), although over the 10 years of its existence the number of pages a year in *Contemporary Hypnosis* has

grown 32%. In his own final editorial Brian Fellows included a breakdown of journal contributions from 1979 to 1995. These figures have been brought up to date in the table below, which also shows separately the corresponding figures for the past five years (1996–2000) and the previous five years (1991–6).

Breakdown of journal contributions 1977–2000

	1977–2000	1991–5*	1996–2000*
Number of pages	3782	952 (190.4)	1254 (250.8)
Main papers	247	54 (10.8)	69 (13.8)
Discussion commentaries and replies	192	23 (4.6)	37 (7.4)
Brief reports	57	20 (4.0)	10 (2.0)
Clinical reports and case studies	35	12 (2.4)	15 (3.0)
Book reviews	108	22 (4.4)	20 (4.0)
Obituaries and appreciations	14	9 (1.8)	5 (1.0)
Editorials	40	7 (1.4)	6 (1.2)
Abstracts of current literature	327	68 (13.6)	259 (51.8)
Abstracts of BSECH conference papers	89	–	89 (17.8)

* The figure in brackets is the annual mean.

The comparison between 1991–5 and 1996–2000 shows an overall increase in the number of main papers published per volume and an increase in discussion commentaries and replies. Some of this change reflects the introduction of special issues containing selected papers from BSECH annual conferences and an increased frequency of topic-based special issues such as ‘The Nature of Hypnosis’ based on the 1996 CIBA Discussion Meeting (Issue 15.1), ‘The Definition of Hypnosis’ (15.3), ‘T.X. Barber – Hypnosis: A Mature View’ (16.3) and ‘Hypnosis and Madness’ (17.3) as well as continuing the ‘main paper with discussion commentaries’ formula that has been a distinctive feature of the journal for many years. There has also been a small increase in the number of clinical and case reports but, despite exhortations from both Brian Fellows and myself, the rate of submission of this type of paper remains low compared with research and theory oriented ones. Increasing the proportion of case reports published in the journal remains as a challenge for John Gruzeliar over the years to come. The number of brief reports, by contrast, has decreased in 1996–2000 compared with 1991–95, whereas the numbers of book reviews and editorials has remained relatively constant.

Abstracts of current literature (complete sets of edited abstracts from the other main hypnosis journals) was introduced as a regular feature by Brian Fellows in 1994 with Victoria West as the abstracts editor. This role was taken over by Richard Brown in 1997. It is of some satisfaction that with the current issue of the journal we have caught up with the backlog of abstracts caused by publication problems with the *American Journal of Clinical Hypnosis* and I can hand over with the abstracts of current literature up to date.

In my first year as editor, in 1996, I introduced the regular publication of the Proceedings of the BSECH Annual Conference, consisting of a complete set of the abstracts of all the papers presented and, in later years, brief reports of the conference workshops. These have appeared annually in the final issue of the journal in the

year the conference was held, and in the current issue we have the Proceedings from the 17th Annual Conference of BSECH (held jointly with the British Society for Medical and Dental Hypnosis) held in Devon in May 2000. A special issue is planned for next year (issue 18.2) which will contain complete versions of a selection of papers from the conference. The editor for this issue will be Richard Brown, who was responsible for organizing the academic programme at the conference.

As some of the above implies, our journal has passed through a number of transformations on the way to its present form and this seems an appropriate time to document its progress so far. To that end I have compiled the following brief history.

A history of *Contemporary Hypnosis*

- 1977 Publication in November/December of issue number 1 of *Hypnosis: The Bulletin of the British Society of Experimental and Clinical Hypnosis*. This inaugural issue ran to 15 pages and Brian Fellows was the editor.
- 1979-1981 Publication continued at one issue a year (in April) under the same title (issues 2-4).
- 1982-3 Still publishing one issue a year (still in April) but now called simply *Bulletin of the British Society of Experimental and Clinical Hypnosis*. Reached issue number 6 in 1983.
- 1983-1990 Commencing in October 1983, the journal became *British Journal of Experimental and Clinical Hypnosis*, with three issues a year, starting afresh with volume 1. Initially followed the academic year timetable with issues in October, January and April, but, in 1987, it converted to a calendar year with issues in January, April and October. With the 1990 issues (volume 7) production of the journal was taken over by Whurr Publishers – up to this point the journal (and the bulletin before it) had been published independently by BSECH.
- 1991-5 With volume 8 the title of the journal became *Contemporary Hypnosis*. The same three issues a year publication schedule was continued. Brian Fellows retired as editor with volume 12 in 1995.
- 1996-2000 David Oakley became editor of *Contemporary Hypnosis*. In 1997 the publication schedule changed to four issues a year.
- 2001 John Gruzelier becomes the new editor of *Contemporary Hypnosis*.

Endnote

In closing I would like to take this opportunity to express my thanks to the editorial team and reviewers who have worked so hard with me over the past five years: to Colin Whurr and his colleagues for their unfailing support and consideration, to Pam La Rose for her calm efficiency in dealing with the day-to-day running of the editorial office and to all those of you who have contributed papers, reports, reviews and commentaries to the pages of *Contemporary Hypnosis* – without you it never would have worked.

Note

1 Sixteen years is Brian's own estimate – in fact, if we count what he described as 'the somewhat tentatively entitled "Pilot Issue Number 1"' in 1977 I make it 18 years.

AUTHOR'S REPLY

ON THE PHYSIOLOGICAL REDEFINITION OF HYPNOSIS: A REPLY TO GRUZELIER

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Gruzelier's (2000) paper entitled 'Redefining hypnosis: Theory, methods and integration' was initiated as a response to my earlier paper entitled 'The semantics and physiology of hypnosis: Towards a definition of hypnosis' (Wagstaff, 1998), so it is mainly in this context that I shall reply. In his paper Gruzelier puts forward the view, based on extensive neuropsychological and physiological research, that hypnosis is an unusual or atypical brain state that differs from everyday neurophysiology (2000: 50). Although Gruzelier allows that sociocognitive processes may influence and even give rise to this brain state, he nevertheless contrasts his position with the sociocognitive view, advocated in my paper, that construes hypnosis primarily as a social construction, or role enactment, based on suggestion (that is, 'hypnosis' is essentially a suggestion that one is entering a culturally defined special state, normally associated with hypersuggestibility).

However, one of my main reasons for writing the original paper was to counter the idea that neuropsychological and neurophysiological studies of hypnosis are necessarily opposed to sociocognitive views of hypnosis. On the contrary, I argued that many neurophysiological findings may be deemed supportive of, and indeed make most sense in terms of, a sociocognitive position. Like Gruzelier, therefore, I believe that neuroscience may have a lot to contribute towards our understanding of hypnosis. A major problem, however, is that, although neurophysiological data can look impressive, their usefulness depends entirely on the adequacy of the theories we devise to interpret them. As Gruzelier acknowledges, current ideas about brain function are 'impressive but primitive' (2000: 52). It is important to begin, therefore, on a note of caution.

Neuroscience and explanation

Gruzelier argues that one of the difficulties with the sociocognitive view is that, unlike the neurophysiological approach, the former simply 'labels' phenomena rather than explaining them in terms of the processes involved. In this context, for example, Gruzelier refers to a case report by Davies and Wagstaff (1991) that I discussed in my original paper. This concerned an elderly woman who could not walk unaided. She was referred to us after numerous tests (including a PET scan) had not indicated any organic pathology that might account for her condition. She was successfully treated using a brief suggestive therapy that did not include a formal induction procedure or indeed any mention of hypnosis. Because of the nature of her recovery, we proposed that the treatment may have worked through a psychological mechanism such as the reversal of the 'sickness role', or by changing her attributional system such that she

now believed that she could walk. Gruzelier criticizes our reasoning here as 'labelling' rather than providing an explanation of the mechanisms or processes involved in her recovery. Instead he argues that there may have been an undiscovered 'neuropsychological cause'. However, apart from the fact that no 'neuropsychological cause' had been established after extensive investigation, Gruzelier's criticism here rests on a misunderstanding of what it means to 'explain' something in terms of its processes.

In psychology there are many complex explanatory process models that are not based on physiology (Skinnerian conditioning, social learning, attribution theory, reciprocal altruism, personal construct theory, psychoanalysis, connectionist models of memory, and so on). However, the argument that such models cannot ultimately explain anything because they do not refer to physiology is mistaken; instead it is more accurate to say that the processes described by such theories represent *a different level of explanation*. But, in any case, the idea that there is a fundamental distinction between psychological and neuropsychological processes can be misleading. If one accepts standard views on the subject, all psychological manifestations must have physiological correlates at some level. If the patient in our case had made a mistaken attribution and believed she could not walk, no doubt this would have manifested itself physiologically somewhere, but what evidence we had suggested that the brain physiology was not the most appropriate level of explanation to determine treatment. From a medical point of view, it is always necessary to establish whether a patient has some kind of recognized brain abnormality (such as a tumour), as this helps to determine treatment. In cases such as the present, however, where no such abnormality was found despite extensive neurophysiological investigation, sometimes the most sensible option is to pursue treatment based at the level of psychological rather than neuropsychological explanation.

In the context of hypnosis, therefore, it would be unwise to assume that neuropsychological models are somehow, of necessity, superior in terms of their explanatory power to the main psychological models, sociocognitive and otherwise. Instead the models should be seen as illuminating each other. Indeed, no matter how impressive neuropsychological data may look, they are useless without an adequate psychological explanatory model to interpret them and guide research. Consequently, I see no need for those interested in neuroscience to ally themselves with the idea of hypnosis as an atypical neurophysiological brain state that negates the sociocognitive position, and there is certainly no need for those adopting sociocognitive and other non-mechanistic positions to dissociate themselves from neuropsychological and neurophysiological studies.

Hypnotic induction and the physiology of role enactment

For example, in many respects the view held by Gruzelier (2000) regarding the nature of the standard hypnotic induction procedure coincides well with a sociocognitive view. Gruzelier argues that the standard hypnotic induction procedure involves the enactment of roles that result in the unfolding of neurophysiological processes. Most typically, we are told that this allegedly involves a shift from 'left anterior selective attention processes', followed by 'selective anterior inhibition', followed by 'posterior involvement which is greater on the right side of the brain'. Gruzelier proposes that this type of induction results in an 'alteration of brain systems through an interpersonal and cultural context'; and these include, importantly, 'frontal suppression',

particularly of the left hemisphere. Of course, it remains to be established whether all induction procedures give rise to these changes, and whether all subjects who seem to be hypnotically susceptible respond in this way. Let us assume for the moment, however, that these findings are valid, reliable and generalizable. If so, as I noted previously (Wagstaff, 1998), such changes can readily be interpreted as a shift in brain functioning that might be expected to occur as the subject first concentrates, listens to the hypnotist intently and then, in accordance with instructions, 'lets go' and tries to think and imagine along with the hypnotist in a more holistic and agentic manner. In other words, here we may have the neurophysiological concomitants of enacting the hypnotic role in response to standard induction procedures. However, it is important not to get too carried away with the idea that such processes are extraordinary, or directly relate to those shown by patients with brain damage or psychiatric illness. Left hemisphere frontal suppression may sound technical and dramatic, but you do not need to undergo a hypnotic induction procedure to show a laterality shift to the right hemisphere with relative frontal suppression – all you need to do is to agree voluntarily to follow instructions on a computer (Morris, Ahmed, Syed and Toone, 1993).

Hypnosis as a category error

This brings us to another issue I raised in my original paper. According to the sociocognitive position, to understand what is going on in hypnosis we need to look at the normal behavioural repertoire of individuals. Accordingly, we should expect the hypnotic subject to respond psychologically and neurophysiologically using various elements from a standard behavioural repertoire; so, for example, when examining the physiological data we should expect to find physiological correlates of the various activities engaged in by the subject as he or she responds to the demands of hypnotic suggestions by variously relaxing, shifting attention, concentrating, 'drifting', imagining, 'letting go', thinking, complying and so on, depending on the suggestion.

Gruzelier does not accept this view; instead he insists that there is something atypical or unique about the brain state of the hypnotized individual that cannot be explained in terms of these activities. To illustrate his view he catalogues a range of studies that purport to show that hypnosis is not the same as using these processes – for instance, high susceptibles responding to hypnotic suggestions differ in their responses from simply being absorbed in a story or being deeply relaxed. Moreover, he cites evidence that the physiological responses of high susceptibles to painful stimuli are different from those in a distraction condition (Miltner, Weiss, Friederich, Trippe and Ozcan, in press).

But none of the studies cited by Gruzelier in this context speaks to the central theme of my argument. Indeed, Gruzelier's argument graphically illustrates again the problem I was addressing in my original paper (Wagstaff, 1998). According to the sociocognitive position, during a hypnotic induction procedure a responsive subject may be doing many things; focusing on the hypnotist, relaxing, following the imagery suggestions, 'letting go', being prepared to follow instructions, anticipating what is to come and so on. Different suggestions also require different strategies and expectancies to pass them. As such, of course there is more to hypnotic responding than simply being 'absorbed in a book' or plain 'relaxing'. Hence, one of the central tenets of the sociocognitive approach has long been that one does not need to be relaxed to enact the hypnotic role (Wagstaff, 1981, 1986, 1991); one will, however, find the

neurophysiological and psychological correlates of relaxation in an induction procedure that involves relaxation if one looks for them (see Edmonston, 1991; Kirsch, Mobayed, Council and Kenny, 1992). Similarly, hypnotic responding is a complex social role that rests on far more than the capacity to become 'absorbed' in a task (see, for example, Milling, Kirsch and Burgess, 2000). A standard hypnotic induction procedure is a complex social task that follows a sequence of events. Therefore, until we measure the physiological correlates that accompany the *sequential* nature of a social task comparable in complexity, we cannot possibly argue that the physiological responses reported by Gruzelier are inexplicable in terms of a range of everyday social processes.

Consider, for example, what might happen if we took physiological recordings of a group of students entering a lecture theatre, taking out their notebooks, looking at the visual material, listening to the lecturer, writing notes, asking questions and so on. If we compared such a group with those simply 'reading a book', 'focusing attention' or 'listening to some music', we might expect substantial differences. But we would not necessarily then conclude that participation in lectures requires an 'atypical neurophysiological brain state' that goes beyond paying attention, focusing, listening, reading, following instructions and so on. Of course the definition of hypnosis as a socially constructed role does not, in principle, rule out the possibility that some of the defining characteristics of the role, like these of many other socially constructed roles (such as religious leader, footballer or rally driver), may at some level be reflected in a unique pattern of physiological activity; however, I doubt whether it would make any more sense to attempt to define these roles primarily in terms of brain physiology than it does to define hypnosis primarily in terms of brain physiology.

Dissociation

In a further attempt to dismiss the sociocognitive view, Gruzelier presents a range of results regarding what he calls cognitive and neurophysiological dissociation. It will be noted, however, that he uses 'dissociation' here in its more mundane sense (as it is often used by cognitive psychologists) to refer to any separation of cognitive or brain function, as distinct from the more profound dissociations, for example, of personality, sometimes associated with hypnosis. The main evidence cited by Gruzelier for these types of dissociation is a series of studies purporting to show that, towards the end of an induction, high susceptibles show a suppression of the left hemisphere, a favouring of the right hemisphere and the maintenance of left temporal function (the last occurring, according to Gruzelier, because the subject continues to listen to verbal instructions). But again, there is nothing inconsistent here with a sociocognitive view.

To reiterate, according to the sociocognitive view, when 'good' hypnotic subjects undergo a standard induction procedure they tend literally to follow the instructions; thus they focus on the hypnotist, relax, 'let go', concentrate on their bodily processes and prepare to follow instructions. As noted earlier, not surprisingly, this might be expected to coincide with a relative shift from left to right hemisphere activity accompanied by the additional left temporal component that Gruzelier associates with continuing to monitor instructions. Low susceptibles, on the other hand, are unlikely to show the same pattern because their response to the induction procedure is rather different; thus they tend not to go along with what the hypnotist says for a variety of reasons (to be discussed shortly).

Gruzelier then cites a number of other studies purportedly showing dissociations unique to hypnosis in other modalities. But the problem remains that without proper controls for the complex sociocognitive processes that may be operating when hypnotic subjects respond to suggestions, it is impossible to argue that the results illustrate some unique quality of the hypnotic 'brain state'. By way of illustration, consider a study not cited by Gruzelier that I discussed in a recent paper (Wagstaff, 2000). In this study, Rainville, Duncan, Price, Carrier and Bushnell (1997) showed that when hypnotic subjects are given a pain stimulus and are asked to experience it as pleasant or unpleasant, the neurophysiological data from the cingulate cortex and other parts of the brain are consistent with the view that the pain can be experienced as pleasant and unpleasant, even though its perceived intensity remains the same. However, as is typical in neurophysiological studies of hypnosis, in this study Rainville et al. did not include a non-hypnotic control group given the same analgesia suggestions (that is, a group of subjects given no hypnotic induction procedure but the same suggestions). This is crucial in that studies by sociocognitive theorists, conducted on subjects in both hypnotic and non-hypnotic situations, have shown that subjects often report that suggestions for analgesia are effective even when there is no evidence that the pain stimulus is any less discriminable; in other words, although subjects, regardless of whether they have received hypnosis, often report that they felt less pain in response to an analgesia suggestion, there is no evidence that their brains successfully 'blocked out' the pain stimulus (see, for example, Spanos 1989, 1991). This led Spanos to conclude that many subjects who successfully experience suggested analgesia may do so not through blocking out the painful stimulus, but by interpreting the stimulus in a more positive way. Thus, Spanos concludes, "These findings may indicate that suggestions for reduced sensitivity produce their effects by inducing subjects to reinterpret (rather than to "block out") sensory activity" (1991: 341). It seems, therefore, that Rainville et al. may have found neurophysiological support for Spanos' sociocognitive proposal that, regardless of whether an induction procedure is used, many individuals who respond positively to suggestions for analgesia do so by trying to reinterpret the pain stimulus such that it is not perceived as unpleasant. Sociocognitive data also fit with the study conducted by Miltner et al. (in press), cited by Gruzelier, showing that responsiveness to analgesia suggestions by high susceptibles involves more than distraction (which is only one strategy for dealing with pain).

Overzealous and restrictive theorizing

Gruzelier then moves on to the notion that sociocognitive theorists use overzealous and restrictive theorizing. As an alleged example of this, he cites my explanation of an experiment by McCormack and Gruzelier (1993). Briefly, this study compared subjects of 'high' and 'medium susceptibility' (unfortunately no 'lows' were included). Results indicated that both groups showed improvement in d' a measure of perceptual discriminability, in the hypnosis condition. Given the large range of findings indicating that improvements in perceptual functioning in within-subjects designs tend to be motivational in origin (subjects 'try harder in the hypnosis condition', or 'hold back' in the non-hypnotic control condition), I argued that this was the most likely explanation of this overall effect (Wagstaff, 1998). However, further results showed that for high susceptibles, the shift in d' , or perceptual sensitivity, occurred only for the right hemisphere, whereas for the medium susceptibles it was bilateral. My

explanation for this was that because the highs were more likely to be engaging in holistic activity, and in 'following instructions' mode towards the end of the induction, the increase in d' was moderated by this effect and occurred only in the right hemisphere. Gruzelier rejects what he calls this attempt to 'explain away' (2000: 59) these results primarily on the grounds that (a) the holding back effect reflects attitudinal differences between the groups, and, therefore, (b) would have shown up in response criterion shifts (β) rather than d' changes. Gruzelier's rejection, however, is based on a confusion about the role of attitudes and motivation in signal-detection theory.

In signal-detection theory, a response bias effect (β shift) occurs when people, for example, 'see' or perceptually discriminate something, but become more and more reluctant to report what they see, as, say, the penalties increase for making a mistake. Shifts in d' , however, occur when the perceptual ability of the subject to actually discriminate objects changes. Accordingly, attitudes that manifest themselves in motivational factors (those that directly affect 'how hard we try') are more likely to influence d' than β . Suppose, for example, for attitudinal reasons I decide first to 'hold back' and concentrate poorly on a stimulus object. My ability to discriminate would be poor. But if I then put all of my energies into searching for it, my ability to discriminate would improve. Consequently, rather than 'explaining away' (Gruzelier, 2000: 59) McCormack and Gruzelier's data, hopefully, by making sense of them in a theoretical context, I have gone some way towards 'explaining' their results.

Individual differences

Another example of how a sociocognitive perspective may be able to provide an explanation where Gruzelier's viewpoint does not concerns sources of individual differences. Gruzelier argues that during hypnotic induction procedures low susceptibles fail to show initial engagement of attentional mechanisms and ensuing frontal suppression. Indeed, sometimes they seem to show changes in opposition to those of high susceptibles. Gruzelier is unable to explain why this happens, as he claims that the two groups do not show conscious differences in cognitive strategy, and many 'lows' vigorously attempt to follow instructions.

However, Gruzelier's observations on this matter do not address a large amount of evidence collected on this topic indicating that 'highs' and 'lows' differ on a range of social and cognitive variables. These include differences in attitudes and expectancies about hypnosis; the recognition that responsiveness to suggestions involves active involvement not passive responsivity (this would be manifest by an unwillingness to use the strategies in a situation defined as hypnosis because this is 'not what is supposed to happen'); inappropriate interpretational sets (expecting too much or not knowing how to label ambiguous experiences); and difficulty in using the strategies (some find it difficult to imagine sounds and images) (see, for example, Spanos, 1986; Bertrand, 1989; Lynn and Rhue, 1991; Jan and Wagstaff, 1993). In addition, 'lows' can display a 'negative subject effect' (Jones and Spanos, 1982; Jones and Flynn, 1989) – that is, 'lows', because they do not consider themselves to be hypnotically susceptible, may actually reject or perform counter to the suggestions given to them in the context of hypnosis. It is through recognition of the influence of these variables that Spanos and his colleagues devised the Carleton Skills Training Package, which ostensibly transforms low susceptibles into highs (for a review, see Bertrand, 1989). Consequently, if it could be shown that lows transformed into highs on the CSTP had

the same physiological changes as highs during induction, Gruzelier may be able to find a basis for explaining the physiological differences that he reports.

The future of the neurobiology of hypnosis

This brings us to what I consider to be the major problem with any attempt to devise a theory of hypnosis physiologically – from the ‘bottom up’, as it were. As I stated at the beginning of this commentary, no matter how impressive neurophysiological data and the range of accompanying technical anatomical jargon may seem to be, they mean nothing without an adequate psychological theory to interpret them and guide the construction of experimental paradigms in this area. For example, consider Gruzelier’s proposal that left frontal inhibition is a defining characteristic of hypnosis, such that planning functions are given over to the hypnotist, and there is a suspension of critical evaluation and reality testing. Certainly Gruzelier provides a variety of evidence that is consistent with the view that highly susceptible subjects show inhibitions in especially left frontal activity during and immediately after an initial hypnotic induction procedure. However, as I commented in a recent paper (Wagstaff, 2000), for frontal lobe inhibition to be a defining characteristic of hypnosis, this inhibition should remain throughout a full and varied series of hypnotic suggestions. This does not seem to be the case. Thus, in reviewing the psychophysiological literature, including studies of cerebral blood flow during analgesia suggestions, Crawford (1996) concludes that, ‘Rather consistently there is *increased* involvement of regions within the frontal cortex during hypnotic suggestion’; moreover, as Crawford points out, ‘PET studies show increased activity in the frontal cortex during the performance of willed actions (1986: 269–71; emphasis added). Furthermore, Jasiukaitis, Nouriani and Spiegel (1996) found that hallucination suggestion effects were associated with left hemisphere superiority, another supposed indicator of increased planning activity; hence Jasiukaitis et al. conclude, ‘Highly focused attention, simultaneous with a relative independence of behaviour from context, has long been considered a fundamental aspect of hypnotic behaviour. Such narrow attentional focus would seem to be a function of the *left hemisphere’s* detailed analytical and sequential processing’ (Jasiukaitis et al., 1996: 667; emphasis added). These findings would apparently indicate that many hypnotic phenomena are associated with increased planning activity for highly susceptible subjects. From the perspective of sociocognitive theory, however, these data are entirely consistent with the view that we might expect brain activity to vary according to the type of suggestion given; a standard induction procedure might encourage frontal suppression, but some difficult suggestions, such as analgesia and hallucinations, may require increased planning and frontal activity (see Wagstaff, 1998).

In conclusion, I agree with Gruzelier that studies of the neurobiology of hypnosis may prove to be fruitful; however, in my view, their true value will be in helping to delineate the various psychological activities and physiological processes involved in responding to a varied range of suggestions, hypnotic and otherwise, rather than attempting to identify an ‘atypical brain state’ that we can specifically associate with ‘hypnosis’. Perhaps the most important paradigm shift in hypnosis over recent decades has been the move away from the mechanistic Braidian idea that hypnosis is some kind of unitary brain state that the subject ‘falls into’, to the idea that hypnotic subjects are active, cognizing individuals, trying to make sense of and use the instructions given to them. The technology and language of neuroscience are both

challenging and exciting, but it is important that, in our awe, we do not forget the vast amount of less colourful evidence that gave rise to this shift.

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