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## ***RECOVERED MEMORIES***

*Edited by Michael Heap*

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# EDITORIAL

## REFLECTIONS ON RECOVERED MEMORIES

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**Michael Heap**

*Chairman of ASKE*

This issue of the *Skeptical Intelligencer* contains two articles that have appeared in the Newsletter of the British False Memory Society. The first is a review of the recovered/false memory controversy by Daniel Wright, James Ost and Chris French and the second is a discussion by James Ost and Kimberly Wade of a research paper that investigated the neural correlates of memory suppression. The latter paper is not only of academic interest: it illustrates how research findings can be interpreted sensationally and uncritically by the mass media.

The BFMS has very kindly sent me complimentary copies of its Newsletter since January 1994 (Volume 2, Number 1) and they are available for inspection by any ASKE member. I am grateful to Madeline Greenhalgh, the Director of the BFMS, and to the authors themselves for allowing me to reproduce the two articles.

This paper is largely an account of my professional experience of 'recovered memories' and related phenomena. Much is anecdotal and does not aspire to the rigorous scholarly and scientific standards of a formal paper. I have not always provided references and anyone wanting these is welcome to contact me.

### **The British False Memory Society**

I first became aware of the BFMS in 1993 when I read about it in the papers. There was already an American society, and others were forming across the world. Members were mainly parents whose adult children had accused them of sexually abusing them in childhood. These allegations were made after the accusers had been undergoing psychotherapy or counselling, during the course of which traumatic memories of events in their childhood, of which they had no awareness prior to the therapy, were 'recalled', usually with the aid of procedures such as hypnosis or merely through leading questions and suggestions by the therapist.

As a rule these people had sought help for the usual problems for which people seek psychological therapy - depression, eating disorders, phobias, anxiety states and so on - and not because they had any thoughts or feelings of having been sexually violated by anyone, least of all their parents. Most often they would be women and the alleged perpetrators would be their fathers, sometimes with the collusion of their mothers. Uncles and friends of the family would also be collectively involved in some cases. The

'memories' elicited were usually of quite extensive physical and sexual abuse. Often they were not merely vague ideas or feelings: they were very vivid and realistic (sometimes with dreams and 'flashbacks') and the clients had no doubt in their minds that the events really happened.

Put very simply, the theoretical rationale for this kind of therapy is that the experiences of abuse were so traumatic that the person cannot handle the associated memories and they are 'repressed' - denied access to consciousness by defence mechanisms of which the individual is not conscious (i.e. it is not simply a deliberate strategy of trying not to think about the events). However, the distressing memories are still present 'in the unconscious' and continue to have an adverse effect on the individual's psychological wellbeing - hence his or her problems and symptoms and inability to make sense of them. Therapy allows the defences to be relaxed and the memories are allowed conscious representation and hence the opportunity to be 'processed' and resolved within the therapeutic context and beyond.

Returning to the contemporary recovered-memory controversy, once the memories of abuse were 'recovered', with the encouragement of their therapists the clients would often telephone or write to their parents, informing them that they had now recalled what had happened and indicating that they would have no further contact with them, at least not until they themselves decided. This would be considered part of the therapeutic process: the victim (or 'survivor') of abuse is now empowered and in charge her life and able to confront her abuser as a mature, assertive adult.

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**Typically, there would not be one shred of evidence that any abuse had occurred.**

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All of this would be come as a 'bolt from the blue' for the parents and other family members, especially those who had maintained a close and loving relationship with the person concerned. Typically, there would not be one shred of evidence that any abuse had occurred and sometimes the supposed events could not, in any case, have happened because they were contradicted by known historical facts; or they were so extensive and bizarre,

involving, for example, neighbours, other children including siblings, and animals, as well as Satanic worship - as to defy any possibility of their going undetected at the time and remaining so for years afterwards. Yet the accused parents had no opportunity to defend themselves; indeed some were prosecuted in the criminal courts purely on the evidence of the therapy.

No excuse can be offered that the therapists concerned were untrained, unqualified, inexperienced or unregulated. Some clearly were, but some were highly qualified professionals - psychotherapists, psychologists and even psychiatrists.

Around 14 years ago I took the opportunity of a visit to the UK by the late Campbell Perry, Professor of Psychology at Concordia University, Montreal, to arrange for him to speak at a joint meeting of the British Society of Experimental and Clinical Hypnosis, of which I was then secretary, and the BFMS. This was at University College London and was attended by BSECH members, trainees on the Diploma in Applied Hypnosis course at UCL, and members of the BFMS. I had some apprehensions about this meeting as I expected that Professor Perry would give a rather academic lecture on 'recovered memories' (which he did) whereas it seemed to me more likely that what the parents most wanted to hear was how the injustice and damage done to them and their children could be repaired.

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**There is nothing, in my opinion, that is fundamentally at fault with the role of a professional psychotherapist or counsellor.**

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One thing that immediately struck me was that these people seemed to come almost uniformly from the professional classes – affluent, educated and articulate. Perhaps this was partly due to selection bias and so on, the meeting being held in central London. Understandably there was a great deal of anger in the air. My colleague Tony Gibson, by then around 80, was in the audience and at one point he attempted to alleviate the tension by suggesting that it wouldn't be long before people would be accused of sexually abusing children in their previous lives. A very irate gentleman stood up and said, 'Excuse me, I *have* been accused of abusing someone in my previous life – my niece - and it's a damned impertinence!' Indeed, what was most distressing for me at the meeting was realising how badly these people had been let down (to put it mildly) by the psychotherapy profession.

**The Role of the Psychotherapist or Counsellor**

There is nothing, in my opinion, that is fundamentally at fault with the role of a professional psychotherapist or counsellor. But the occupant of that role is a human being, subject to all the frailties and shortcomings of any

other human being. By and large the psychotherapy professions recognise this and a good psychotherapist is self-reflective and self-critical, often with the aid of a third party, his or her 'supervisor'. It is, however, all too easy for the practice of psychotherapy to become detached from any firm scientific base (which should not only inform the ideas and methods employed but also evaluate the outcome of the therapy for the client population). This rejection of science represents an abdication by the practitioners of their responsibility to have some external process of accountability. Elsewhere (*note 1*) I have described this process as the means by which practitioners of any healing or therapeutic modality (including mainstream medicine) *authenticate their roles*. The most effective way of achieving this (or its converse) is by scientific evaluation.

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**Practitioners whose ideas and methods are not clearly supported by existing evidence will understandably feel intimidated by the process of scientific evaluation.**

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In many applied scientific endeavours, especially those that come loosely within the biological and human sciences, there will always be some tension between the theoreticians and academics on the one hand and the practitioners on the other. This can be very healthy and mutually beneficial, but potentially it is the former who threaten the authenticity of the latter, say by revealing that the theoretical assumptions on which the practitioners base their ideas and methods are invalid or that the methods appear ineffective when subject to clinical trials. Practitioners whose ideas and methods are not clearly supported by existing evidence will understandably feel intimidated by the process of scientific evaluation; that is, their *role authenticity* is threatened. Characteristically they may avoid or dismiss such evaluation and resort to other means of authentication ('We know our treatment works: our patients tell us it does'; 'Our methods have been used for thousands of years'; 'We treat the *whole* person and you can't measure a whole person'; etc.).

This lack of accountability can be very unhealthy and at times harmful. The practitioners tend to form a closed group, running their own training courses, disseminating their ideas through their own publications, and so on. As such, as a group they come to take on the semblance of a cult, and often there may be one or two influential guru figures whose writings are eagerly digested by their followers and who spend much of their time lecturing and training others in their ideas and practices. Commonly they will assert that no one is qualified to judge or criticise the therapy unless he or she has undertaken the training, which

may even involve undergoing a course of the therapy itself (as, e.g., in the case of psychoanalysis).

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**Unchecked by any system of external accountability, ideas and practices may become increasingly extreme and bizarre.**

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This kind of ‘only we are the experts’ mentality is evidenced by those therapists who claim the special ability to detect that a person has suffered childhood sexual abuse from certain signs and symptoms exhibited by the person, even when she or he has no awareness of having been abused. During an invited talk at the 1994 annual BSECH conference in London, the then Director of the BFMS, Roger Scotford (*note 2*), made a very salient comparison between this claim and the claim of witch hunters in the Middle Ages to be able to identify witches by examining them for particular signs and stigmata

Unchecked by any system of external accountability, ideas and practices may become increasingly extreme and bizarre. It may be very difficult or impossible for the adherents to recognise this: once anyone starts to reject the more advanced ideas, the whole belief system is in danger of unravelling all the way back to its original premise. I have previously referred to this as the ‘psychotic phase’ of a belief system, by analogy to pathological mental states (*note 3*), and I shall illustrate it in a moment.

Finally I have identified (*op cit*) a ‘paranoid phase’ when the believers interpret the rejection of their ideas and methods as evidence of a conspiracy on the part of some people or organisations (or ‘the establishment’) to prevent the public from knowing the truth (cf. the idea that the medical profession and the pharmaceutical industry are conspiring, out of self interest, to discredit alternative medicine or that governments are covering up the truth about alien visitations.) I have heard similar ‘paranoid’ ideas expressed about False Memory Societies that deserve no mention here except with reference to the subject of multiple personality disorder, which I shall now discuss.

**Multiple Personality or Dissociative Personality Disorder**

A feature of the false memory controversy in America – and to some extent in other countries, though less so Britain – is that many of the clients or patients who ‘recalled’ memories of sexual abuse were diagnosed by their therapists as having multiple personality disorder (MPD; later relabelled dissociative identity disorder - DID). Putting it very simply, it is claimed that in the face of severe and repeated emotional, physical and sexual abuse, the individual’s defence mechanism of repression or dissociation is so profound as to lead to a splitting or fragmenting of his or her personality or self. Thus the

person’s daily experiences and behaviour may, at any time, be ‘under the control’ of one of two or more personalities or ‘alters’ that may not be aware of each other (or awareness may not be mutual). The person diagnosed with MPD or DID may display a range of symptoms and problems that are consistent with this account – e.g. frequent changes of mood and character, extensive memory lapses that cannot be the result of simple forgetting, and little insight into their erratic behaviour.

The diagnosis of MPD or DID is most often made during the course of therapy. The therapist helps the patient identify the attributes that characterise each ‘personality’ – ‘the person in you that’s angry’, ‘afraid’, ‘hurt’, etc. The patient is also invited to provide a name for each one of these and the therapist will ask permission to communicate with the different personalities by asking, say, ‘May I speak with John now?’, ‘Will Mary come out?’ or ‘Who am I speaking to now?’ Thus the patient is encouraged in the therapy session to ‘switch’ personalities. The aim of therapy is to integrate the personalities into one whole and therefore they are encouraged to communicate with one another. This may happen out of therapy: for example one method is for the patient to put up a bulletin board at home so that the various alters can leave messages for one another.

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**In America, the CIA has been cited as being in cahoots with the False Memory Foundation and the backlash against the diagnosis of multiple personality disorder.**

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MPD was once regarded as very rare, even by those who considered it an authentic diagnosis. Then, in the 1980s and 1990s, reported cases increased exponentially in the USA. But not only did the number of *people* with MPD increase: the number of *personalities* that any one individual might have also increased, with cases having 100 and even 200 personalities being reported. These personalities were not just ‘other people’: they might be animals, inanimate objects (I recall that one reported case had a cloud as an alter), previous lives, and so on.

Maybe the reader is now thinking of my idea of the ‘psychotic phase’? And what about the ‘paranoid phase’? In America, the CIA has been cited as being in cahoots with the False Memory Foundation and the backlash against the diagnosis of multiple personality disorder. Why the CIA? Because one theory is that after World War II, American Intelligence was involved with Nazi doctors in the programming of children to develop multiple personality disorder. These ideas have been espoused not by some oddball in a Breakfast Television interview but by a Professor of Psychology at the

University of Utah who informed us, 'My best guess is that the purpose of it is that they want an army of Manchurian candidates - tens of thousands of mental robots who will do prostitution, do child pornography, smuggle drugs, engage in international arms smuggling, do snuff films, all sorts of lucrative things and do their bidding. And eventually, the megalomaniacs at the top believe, (*they will*) create a satanic order that will rule the world' (*note 4*).

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**Thirty-seven years of professional work as a psychologist has rewarded me with not one single encounter with anyone with MPD or DID.**

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The British public have displayed a robust immunity to MPD. There are a number of reasons for this. I believe one of them is that the psychoanalytical influence on mainstream psychiatry has traditionally been much stronger in the USA than in Britain. Associated with this is the fact that American psychiatrists and psychologists make much greater use of the concept of dissociation in clinical practice than their British counterparts (at least this is my impression; *note 5*). Another reason may be that the diagnosis of serious mental disorders is usually the domain of psychiatrists who, in this country, tend not to undertake psychotherapy with their patients. It is likely that here, the preferred diagnoses for patients presenting with severe symptoms are schizophrenia, bipolar affective disorder, and borderline personality disorder.

At a scientific meeting I once asked the late Professor Sidney Brandon (senior author of the 1997 Royal College of Psychiatry's Report on 'recovered memories') why, unlike recovered memories of childhood abuse, there has been no 'epidemic' of MPD in this country. His reply was, 'We don't allow our patients to have it!' What he meant was that when patients start to talk as if they have different identities the professionals looking after them do not collude with this to the extent of turning the metaphorical into the literal.

This approach is consistent with the majority view in the UK, supported by many professionals in other countries, including in fact the USA, namely that MPD or DID is mainly or wholly iatrogenic and results from the explicit and implicit coaching of patients by the therapist into this way of thinking of themselves and their problems in life.

Speaking for myself, 37 years of professional work as a psychologist has rewarded me with not one single encounter with anyone with MPD or DID. In the last 8 years I have worked with offenders with mental illnesses and personality disorders serious enough to warrant their being compulsory detained under the Mental Health Act.

More often than not they have endured childhoods of extreme emotional deprivation, often blighted by severe physical and sometimes sexual abuse. This is the very population in which one would expect to find patients who might be diagnosed with DID. Yet on no occasion has this diagnosis been seriously entertained. Rarely is the concept of dissociation itself invoked in any formal diagnostic usage, though we use it now and again in its more everyday descriptive sense (and would probably use it more frequently if we had more patients with severe personality disorders). We do find the concept of 'fragmented personality' useful to understand the mental states of some acute patients on admission. Such patients seem to lack a consistent core personality or 'ego' in the Freudian sense. Their behaviour and emotions and the demands they express are wildly erratic and difficult to make sense of both to themselves and to the staff. But no reference is necessary to multiple personalities: with the proper medication and their containment in the stable and caring environment of the hospital, and free from malign influences, not least amongst these being street drugs, these patients calm down and their 'true' personality gradually emerges from the chaos.

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**The BFMS is informed by one campaigning principle: a statement about the nature of human memory, and a negative one at that.**

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**More Thoughts on the British False Memory Society**

I have absolutely no doubt that, like my own, the sympathy of ASKE members lies overwhelmingly with the parents and families whose interests the BFMS represent. The majority of the media is naturally supportive too and this makes the BFMS a powerful voice. It boasts an impressive Scientific and Advisory Board, mainly consisting of prominent academic figures in the field of Psychology. The Newsletter itself contains, amongst other features, academic papers of high quality, two of which are reprinted in this issue of the *Skeptical Intelligencer*. This is one reason why it strikes me as an unusual Newsletter, since I assume that its main target readership, the BFMS membership, is largely composed of people who have no academic interest in, or specialised knowledge of, human memory.

But it is unusual because of more than this. The BFMS is informed by one campaigning principle: a statement about the nature of human memory, and a negative one at that, namely that it is not possible for people to repress memories of major traumatic events or episodes that can later be rendered accessible again by psychological means. Putting it somewhat baldly, this means that any potential instances of people allegedly

displaying recovery of repressed memories must be immediately discredited. Some authorities may go along with this, some may not; and some lay people may claim that they have indeed rediscovered extensive 'repressed' memories. This intensifies the politics of the debate somewhat.

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**Understandably this hostility sometimes manifests itself in *ad hominem* attacks on individuals.**

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This antithesis to the idea of 'repression' is evidenced in the Newsletter by an undercurrent of sensitivity or hostility to related subjects such as psychotherapy and counselling generally, psychoanalysis in particular, everything Sigmund Freud said and did, dissociation, psychological amnesia in general, hypnosis, post-traumatic stress disorder, and most recently (March 2007, pp 18-19) the Government's concern about the low rates of conviction (more accurately, indictment) in allegations of rape. Understandably this hostility sometimes manifests itself in *ad hominem* attacks on individuals, even just for seeming, in their writings, to have some sympathy with the possibility of dissociative amnesia. The risk of this is that critics of the BFMS may accuse them of the same 'witch-hunting' devices as they ascribe to therapists who claim expertise in identifying the sexually abused patient.

Occasionally I read in the Newsletter a statement to the effect that the validity of repression and recovered memories is the biggest controversy in psychiatry today. Few people who work in NHS general psychiatry would agree with this statement. In my years of clinical practice I have only known one patient, whom I shall describe shortly, for whom 'recovered memories' of sexual abuse was an issue. I do not recall ever being informed by a work colleague about any such case or the matter ever being raised at ward rounds, case conferences and so on. The last time I gave a presentation to work colleagues that touched upon recovered false memories, there seemed to be very little awareness of this issue in the audience, and the unit manager, who had had many years' experience in psychiatric nursing, asked me to lend him a book on the subject.

**My own Experience of 'Recovered Memories', Psychogenic Amnesia', etc.**

Have I ever intentionally elicited 'repressed memories' in my work as a clinical psychologist and practitioner of hypnosis? No. Even before the contemporary false memory controversy came to the fore I was never comfortable with the idea of directly suggesting to patients that there may be some 'hidden memory' that needs to be restored to consciousness in order for them to resolve their problem. One danger of this is that the patient may feel obliged to

come up with some memory - or even a fantasy - that will satisfy the demands of the therapist.

There are plenty of case reports in the literature of 'recovered memories' that immediately raise these kinds of doubts. For example, in one account a man with torticollis ('wry neck') relived the memory of suddenly turning away in disgust from a plate of meat that had gone rotten and was infested by maggots. This was supposed to be the event that had precipitated the torticollis but the author had no supporting evidence and it did not appear that recalling it itself (if indeed the man had 'repressed' it at all) alleviated the man's condition.

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**I do recall one speaker who appeared to attach some significance to his patient's exclaiming 'Poppycock!' at any idea that her father had abused her.**

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At conferences I have occasionally listened to case presentations by colleagues who have elicited the memory of a (usually childhood) experience that supposedly 'lay at the root' of the client's problem. The problem resolves once the memory is 'relived', sometimes with considerable emotion. (I can't recall an instance of a 'recovered memory' of sexual abuse; I do recall one speaker who appeared to attach some significance to his patient's exclaiming 'Poppycock!' at any idea that her father had abused her.) This is not to say that it can be quite profitable, therapeutically, to encourage patients to revisit difficult memories, so long as some kind of resolution is effected.

The one case of apparent recovered memories of sexual abuse that I recall seeing in my clinical practice was a man I had been treating with cognitive behaviour therapy for depression who had done well and the course of therapy was drawing to a close. Then, for several weeks, he came to the sessions with notes that he had written describing vivid flashback experiences he was having of his father's abusing him. (Both his parents by that time were deceased). It was difficult for both of us to understand what was happening, although he himself had previously described similar vague memories. He worked in Social Services and, at that time, concern was beginning to be expressed in the media about false recovered memories, so he did raise the question of the validity of these 'memories'. It was puzzling to me why, all of a sudden, he was experiencing any of this. He had recently been working with disturbed children, some of whom had been abused, so this might have been a trigger. Around that time, I left my post and I do not know what the outcome was.

I also recall that about 25 years ago I was referred a patient who told me a long story about how recently he or she (I can't even remember which now) had recalled 'forgotten memories' of being sent away to be fostered as a

child for a certain period (I can't remember for how long but it wasn't just a matter of days or weeks). This person told me in detail about his or her life as a foster child and we arranged a further interview but the person did not attend any more sessions.

In my experience in the adult mental health services generally, I have known two patients who described extensive retrograde amnesia; neither of them was convincingly dissociative. It is true to say that opinion in this country is divided as to whether, in the absence of any evidence of brain injury, such individuals are consciously aware of their apparent 'lost memories', though a general scepticism prevails. My first case was about 30 years ago when a man was admitted to the psychiatric unit where I worked, averring that he did not know who he was and had no memories of his life. His amnesia resolved when his true identity was finally established and it transpired that his bank manager was keen to speak to him. The other case was referred to me by a neurological unit with a world-class reputation for specialising in organic amnesic conditions. All this patient's brain scans were normal. Despite my efforts to help her, she did not regain the extensive autobiographical memories that she had lost (she was not *totally* amnesic for these). I remain convinced that her condition was neurological in origin.

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**Many professionals seem comfortable with the idea that...a man claiming to have no memory of committing a violent murder may be genuine and may, in the fullness of time...begin to remember the event.**

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In my forensic work I have occasionally given opinions on complainants of indecent assault and rape where the defence has questioned whether false memory could be at work. In all but one of these cases the people were claiming to have been assaulted as *adults*. Even in such cases, I believe that there may be a recent tendency for defence lawyers, for no particular compelling reason, to seize upon the idea that such an accusation may be a 'false memory' on the complainant's part.

In fact, in the bulk of the cases in which I have provided evidence (always in written form) the complainants have been patients or clients of doctors, psychologists, therapists of various sorts and so on, who, it is alleged, sexually assaulted them during a session of hypnosis or some kind of relaxation procedure. I have written accounts of this work elsewhere (*notes 6 and 7*). In the last case I was involved with, several women complained that an osteopath had sexually assaulted them during treatment (which in most instances appeared to be some form of aromatherapy; hypnosis was not used). Their testimonies were in the main very similar to those of previous cases I

have examined and were very plausible. The defence hired their own expert, someone on the BFMS Scientific and Advisory Board, who opined that the women's accounts might constitute false memories or be the result of their placing a sinister interpretation on the defendant's innocent behaviour. The latter may constitute a plausible account in some cases, but in my experience, the false memory defence can only be seriously entertained in rare cases. Video evidence from covert surveillance of the osteopath at work left little to the imagination and he was sent to prison for several years.

I have seen one adult complainant who alleged sexual abuse by her father when she was in her earliest years. Her 'memories' for the events at such an early age were far too detailed to be authentic. But it was not a straightforward case: she did say she had always remembered being abused, but lately she had been experiencing nightmares and flashbacks. She had gone to a hypnotherapist who 'regressed' her and I had to agree with the prosecution psychiatrist that her evidence was not reliable.

Also in forensic practice I have occasionally seen a defendant who claims amnesia for a violent offence such as murder or rape where the amnesia cannot be accounted for by intoxication with alcohol or drugs or some condition of the brain. The claimed amnesia is usually quite extensive. (There is a phenomenon that has been labelled 'red-outs' in the literature that refers to amnesia for a period of a few seconds during which, overcome by intense rage, the accused person has committed a violent assault. 'State-dependent memory' has been invoked to account for such amnesia but I am not convinced by this explanation.)

There is a healthy scepticism on the part of forensic psychiatrists and psychologists in this country about more extensive claims of amnesia, but my impression is that many professionals seem comfortable with the idea that, for example, a man claiming to have no memory of committing a violent murder may be genuine and may, in the fullness of time (usually while serving his sentence), begin to remember the event. This was the opinion of a psychiatrist who assessed a man who was charged with (and who eventually pleaded guilty to) the brutal murder of a child and the attempted murder of her mother. The scene of the crime bore all the evidence of a frenzied attack, but the defendant claimed amnesia for an extensive period, which included anything that might have precipitated the attack. I was doubtful about his claim when I saw him for a pre-trial report but I still have an open mind. I have no experience of any convicted person exhibiting 'recovered memories' of this nature and hope one day to research this subject.



### **Is there anything special about traumatic memories of child sexual abuse?**

Those who are antagonistic to the idea of repressed memories of childhood abuse, often employ the argument, used by McNally, 2006, as quoted in the Ost and Wade paper ‘... survivors of trauma generally find it difficult *not to think* about the events they have witnessed’. A striking example is that of Holocaust survivors, including those who were children at the time, who have all-too-vivid memories of their ordeals.

I have assessed, for medico-legal purposes, hundreds of people who have suffered some degree of trauma due to an accident. In most cases the trauma is mild relative to that experienced by, say, soldiers on active service or people who have been violently attacked. I cannot recall any convincing case of repressed memory in the absence of head injury, except possibly only for very brief periods of acute shock (e.g. ‘The next thing I remember was lying on the ground, but I can’t remember actually getting out of the car.’) However, we do need to acknowledge the fact that adult memories of the trauma of being sexually abused in childhood present a more complex picture.

Let me explain further. Unlike, say, a Holocaust victim, the child who is being sexually abused is on her own (I shall use the feminine for simplicity). She has not the knowledge or maturity to understand or make proper sense of what is happening (unlike when, later on in life, she considers the events from an adult perspective). She has very confused feelings about it and, importantly, these usually include shame and guilt. There is usually considerable denial about what is happening or has happened. The perpetrator is often a trusted person who spins a web of deceit about what is taking place. A child often does not tell anybody and does not hear a proper explanation that she can understand. If the child does tell somebody, such as her mother, she may be informed that she is lying or mistaken and that the event or events never occurred. In the case of extensive abuse, some children learn to detach themselves from what is going on and create a fantasy life in which such terrible things do not happen. Over the years, the person may indeed develop strategies of trying not to think about what happened. All these factors have implications for the person’s memory of the events as the years go by. Of course none of this implies that an over-simplified notion of ‘repressed memory’ has any validity. However, it does mean that the comparison with other kinds of traumatic experiences is not so straightforward. I would like our academic colleagues to pay more heed to this and to give us the benefit of their expertise concerning the implications for how the events in question are recalled.

### **Posthypnotic amnesia**

The paper reviewed by Ost and Wade in this issue is about ‘directed forgetting’, when participants are instructed to try

to forget some material presented to them. What about posthypnotic amnesia (PHA)? We should consider two kinds, spontaneous and suggested. Spontaneous PHA (total or partial amnesia for events that happened during hypnosis) has a long documented history and at times it has been considered the defining characteristic of true hypnosis. For the amnesia to be real it must be clear that the subject was responding, or at least attending, to the events at the time. A complete spontaneous amnesia is rare but can happen and the subject will usually recall the events with prompting. My experience and the results of one investigation by Crawford et al. (1992) (*note 8*) suggest that the incidence of spontaneous PHA is higher for stage hypnosis subjects than for laboratory subjects. The blanket amnesia reported by one of Crawford et al.’s subjects could not be breached by prompting. I once assessed a man claiming damages against a stage hypnotist who, nearly 5 years after the event, had minimal recollection of the entire show in which he took part. He also described marked time condensation, the whole show seeming to last just a few minutes, when in effect it went on for 1½ hours according to his friends. In criminal cases known to me of alleged sexual assault during hypnosis, once in while a complainant claims amnesia for the actual assault; one complainant claimed that the amnesia lifted over several hours but I offered what I considered to be a more likely interpretation in my report. I usually advise that any account of such events where some amnesia is reported should be treated with caution.

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**Traditionally, suggested posthypnotic amnesia is conceived of as something that happens to the subject but it is much better to think of it as something the subject does.**

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When it comes to hypothesising about the cognitive processes associated with *spontaneous* PHA I find the literature disappointing, and mainstream explanations (e.g. expectancy and state-dependant memory) unconvincing. The phenomenon is taken by some as indicating that at least a proportion of hypnotic subjects are in an altered state of consciousness, by analogy with the common occurrence of amnesia for dreams on waking. The late Professor TX Barber, who spent decades debunking the notion that hypnosis was a special state, finally conceded (*note 9*) that there are a small proportion of highly susceptible people who appear to resemble the traditional ‘deep trance’ subjects when they undergo hypnosis. He described them as ‘amnesia prone’ (I think he must have balked at using the term ‘dissociation’). These individuals exhibit confusion and amnesia when alerted from hypnosis and they report having amnesic episodes in their everyday life. They

have limited memory of their childhood, unlike the small proportion of highly hypnotisable subjects who are described as ‘fantasy prone’. They also have histories of severe physical, and sometimes sexual, abuse in childhood. The bulk of highly hypnotisable subjects are, according to this analysis, neither ‘amnesia prone’ nor ‘fantasy prone’; they are described as having positive expectations and motivations about hypnosis. These categories have not received universal acceptance.

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**Highly suggestible, highly responsive subjects seem adept at inhibiting retrieval of the targeted material by some kind of pre-conscious mechanism.**

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What about *suggested* PHA? The suggestion is that the participant will not recall the contents of the hypnosis session or some specific information until he or she is alerted from hypnosis and not until the hypnotist has provided a signal – the ‘reversal cue’ (which the hypnotist specifies) – for the memories to return. Hence, by definition, PHA is reversible: the memories are retrievable, not unencoded or erased.

The material to be forgotten may be items learned or experienced during hypnosis or information learned prior to hypnosis such as word lists and even autobiographical events. Many, though not all, responsive subjects experience the amnesia as involuntary. The amnesia may be complete, partial or absent and this is related to the subject’s measured suggestibility. Highly suggestible subjects may experience total amnesia for the targeted material.

Traditionally, suggested PHA is conceived of as something that *happens* to the subject but it is much better to think of it as something the subject *does*, even those responsive subjects who describe the experience as involuntary. That is, they retain ultimate control over their memory processes. The reversal cue is an indication to the subject that he or she is to ‘stop being amnesic’, but the amnesia is usually relinquished under strong pressures to be honest (e.g. by producing a ‘lie detector’) or other cues that imply that recall of the material is expected. This is consistent with responding to posthypnotic suggestions in general.

Although it appears that responsive subjects are consciously unable to recall the target material, its ‘presence in memory’ is manifested implicitly. For example, in contrast to their reaction to new material, subjects show a galvanic skin response (sweating) when presented with items from a previously presented word list for which they deny any conscious recall. Such material still has the potential to interfere with the subject’s learning of new material that is not included in

the amnesia suggestion. Indeed the targeted material, while seemingly not available for explicit recall, nevertheless is manifest in a range of indices of implicit memory such as word associations and the completion of word fragments.

As well as dissociation, mechanisms that have been proposed to underlie suggested PHA include compliance (i.e. the subject is knowingly pretending) and distraction strategies during recall. It seems that some responsive subjects do employ these tactics, but highly suggestible, highly responsive subjects seem adept at inhibiting retrieval of the targeted material by some kind of pre-conscious mechanism (which some would identify as dissociation) that is different from those mechanisms adopted by subjects directly instructed to forget the material (David et al., 2000; *see note 10*). Consistent with this are the findings of a recent brain-scan investigation of the neural correlates of suggested PHA in highly suggestible subjects (*note 11*).

**Final Thoughts**

We habitually talk about memories as though they are *things we have*. But there are no such *things* as memories; they do not exist as *entities*. They are *activities we do*. In a previous paper (*note 12*) I made the analogy with waving my hand; I can refer to ‘the wave’ but: ‘When I stop waving my hand the wave does not go somewhere.’

Thus, I engage in the act of remembering an event; when I stop doing this the memory does not go somewhere. It is not stored away like a file in a filing cabinet. Just as my hand, arm and nervous system are structured in a manner that allows me to engage in the act of waving when I choose, so my brain is structured in a manner that allows me to recall a particular event when I chose.

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**It does not appear that the processes involved in ‘directed forgetting’ or in deliberate attempts to inhibit recall match those associated with suggested posthypnotic amnesia.**

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In fact, the same can be said of thinking, imagining, dreaming, and anything that the brain does, including *the mind*: the mind is something the brain *does*.

Likewise, instead of asking ‘Is there such a *thing* as repression?’ or ‘Does repression *exist*?’, we would do better to phrase the question in the active form, something like: ‘In what ways are people able to inhibit the activity of consciously recalling an event?’

It does not appear that the processes involved in ‘directed forgetting’ or in deliberate attempts to inhibit recall match those associated with suggested PHA, although they may do in some subjects. Some authorities consider that suggested PHA is similar to functional

amnesia, notably because of the differential effects on explicit and implicit memory. Whatever the case, PHA appears to be something that highly hypnotisable individuals are adept at doing and reversing according to context (the instructions, the cues, the expectations created, and so on). There is no theoretical reason why responsive individuals should not be able to do this outside of the hypnotic context.

Is this the same as saying that we have here a mechanism for the repression of traumatic childhood substantial memories claimed by some therapists? I don't think so. Neither does Professor John Kihlstrom of the University of California, Berkeley, a leading specialist on PHA and advocate of the dissociation model, but an antagonist when it comes to the notion of the wholesale repression of traumatic memories (*note 13*). It is however consistent with the idea that some responsive individuals can, in some contexts, be temporarily amnesic for events and information that normally they are able to recall. This notion is used to describe pathological dissociative states but it could be a more everyday phenomenon.

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### Call for Contributions

If you have attended a conference or presentation, watched a programme, or read an article or book that would be of interest to readers, why not write a review of this, however brief, for the *Skeptical Adversaria* or the *Skeptical Intelligencer*?

## ARTICLES

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### **TEN YEARS AFTER: WHAT WE KNOW NOW THAT WE DIDN'T KNOW THEN ABOUT RECOVERED AND FALSE MEMORIES**

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**Daniel B. Wright, James Ost and Christopher French**

*Dr Wright and Dr Ost are at the Departments of Psychology of the University of Sussex and the University of Portsmouth respectively and Professor French is at the Anomalistic Psychology Research Unit of Goldsmiths College, London.*

In 1995 the recovered memory debate was near its most vociferous height. Hundreds of people were recovering memories of childhood sexual abuse (CSA), sometimes in therapies that used techniques geared for eliciting such memories. It was claimed that because these events had been so traumatic, memories of them had been repressed or dissociated, but that they then had to be recovered in order for the person to 'heal'. Many of the people who recovered these memories confronted the person whom they remembered abusing them, and some of these cases ended up in criminal courts with successful prosecutions. However, there were those who questioned whether all such recovered memories should be accepted as accurate reflections of events that had really taken place (e.g., Loftus, 1993). It was argued that some, perhaps even most, of such recovered memories might in fact be false memories produced, at least in part, by the therapists themselves. In response to such concerns, many psychological and psychiatric associations issued guidance to their members regarding the potential dangers of unintentionally implanting false memories in patients (e.g., American Psychiatric Association, 1993; American Psychological Association, 1994).

The argument is about the accuracy of such recovered memories and whether certain types of therapeutic techniques were associated with recovery. The argument is critical for the science of memory, but also for thousands of people who have either recovered memories or have been accused of abuse on the basis of such memories, not to mention the families and friends of all concerned. Against this backdrop, the British Psychological Society's Working Party on Recovered Memories (BPS's WPRM) published their report, recommendations, and the results of a survey they conducted with BPS accredited practitioners (Andrews, Bekerian, *et al.*, 1995; Andrews, Morton *et al.*, 1995).

Given the controversial nature of the topic, particularly in the mid-90s, it is not surprising that the WPRM report and survey attracted much criticism. We are not going to re-state any of the criticisms nor are we going re-state any of the praise given to the report. Ten years on we focus on what research has been conducted since the publication of the report that helps to inform the debate.

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#### **In 1995 there was little direct experimental evidence of the impact of so-called 'memory recovery' techniques and the relative ease with which some false reports can be created.**

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Before we begin our review it is necessary to clarify two terms. By 'recovered memory' we are referring to cases where an individual reports an event of which they claim they were previously unaware. By 'false memory' we are referring to cases where an individual reports an event that does not map accurately onto past events. We are aware that these two terms are vague and loaded concepts (Ost, 2003; Smeets, Merckelbach, Horselenberg, & Jelicic, 2005), but as they are also in common use, we use them in this article.

#### **What we know now that we didn't know then**

In 1995 there was little direct experimental evidence of the impact of so-called 'memory recovery' techniques and the relative ease with which some false reports can be created. Much of the evidence at that time was based on memory studies not specifically designed to address the recovered memory debate and case studies not specifically designed to examine the veridicality of memories. Before 1995 there was much literature showing that memories could be distorted (by misinformation, by stereotypes, and so on),

but only a couple of studies of the creation of false memories for entire events (e.g., ‘the mousetrap study’ by Ceci *et al.*, 1994; and ‘lost in the mall’, cited in Loftus, 1993) and a small literature on errors in autobiographical memory (e.g., Conway, 1990). Similarly, while there were many case reports of recovered memories, there was little documentation about whether the memories were accurate, or about whether people actually had forgotten the events. That said, there had been some case studies of memories for bizarre events (biologically impossible events, alien abduction, widespread Satanic ritual abuse) that had been reported. Since the publication of the WPRM, there have been significant efforts directed towards designing studies that are more relevant to the recovered memory debate, and more emphasis within some case studies on investigating firstly the veridicality of the memories and, secondly, whether there had indeed been a period of forgetting.

The purpose of this paper is to review the last 10 years. Due to length constraints, this is a selective review both in relation to the topics chosen and the studies cited. As humans, this selectivity is guided by our own beliefs. We believe:

- that what appear to be newly remembered (i.e. recovered) memories of past trauma are sometimes accurate, sometimes inaccurate, and sometimes a mixture of accuracy and inaccuracy;
- that much of what is recalled cannot be confirmed or disconfirmed;
- and that, because of these two beliefs, reports of past trauma based on such recovered memories are not reliable enough to be the sole basis for legal decisions.

These beliefs are not idiosyncratic to us; many people on both so-called *sides* of the recovered memory debate share these views.

Our selective review covers four areas: adding entire events into a person’s autobiography, forgetting memories, remembering forgetting and forgetting remembering, and case studies of recovered memories. Further, we focus on research with non-clinical (usually student) populations. We do not cover the large trauma/PTSD literature (see Brewin, 2003; McNally, 2003, for thorough reviews).

### **False reports of entire events**

Before 1995 there were a couple of studies showing that false events could be added to people’s memories. With the publication of the ‘lost in the mall’ study (Loftus & Pickrell, 1995), several laboratories began showing that with a little encouragement (like asking people to imagine the event, showing photographs of the event, repeated questioning, hypnosis, etc.; see Ost, 2006), it was possible for participants to come to report relatively unusual events (e.g., spilling a punch bowl at a wedding; Hyman, Husband, & Billings, 1995), events occurring in the first few days of life (Spanos *et al.*, 1999), medical procedures

(Mazzoni & Memon, 2003), and negatively charged events (e.g., being the victim of an animal attack; Porter, Yuille, & Lehman, 1999), and that this even occurs with trained interviewers (Ost, Foster, Costall, & Bull, 2005). The ease with which participants can be led to make such reports relates to aspects of both the event and the person’s beliefs about the plausibility of the event (Pezdek, Finger, & Hodge, 1997; Scoboria, Mazzoni, Kirsch, & Relyea, 2004). Within the ethical constraints of the psychology laboratory, making somebody think that they were attacked by a dog as a child (Porter *et al.*, 1999) may be about as traumatic an event as can be added. This is an important point and is a necessary limitation of laboratory tasks. However, the case studies we discuss later provide strong evidence that it is indeed possible to implant false memories of extremely traumatic events.

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**Clearly, further research is needed on the link between dissociation and false reporting, especially given the problematic finding that a tendency to dissociate is often associated with a history of abuse.**

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Many researchers have also investigated whether people differ in how susceptible they are to such false memories (Read & Winograd, 1998). One of the most researched individual difference measures in this area is dissociative tendencies, or having difficulties integrating thoughts, memories, images, and so on. In lay terms, this is ‘spaciness’ and is closely related to cognitive failures (Wright & Osborne, 2005). People who report much dissociation are likely to be the most susceptible to memory distortions in experiments (e.g., Hyman & Billings, 1998; Ost *et al.*, 2005; Wright & Livingston-Raper, 2002). Clearly, further research is needed on the link between dissociation and false reporting, especially given the problematic finding that a tendency to dissociate is often associated with a history of abuse (Brown, Schefflin & Hammond, 1998).

### **Forgetting Memories for Events**

The term ‘recovered memory’ implies that, at some point, the memory must have been inaccessible to conscious awareness (as opposed to being a ‘continuous memory’). Although this terminology is not ideal, it is clear that people often fail to report important events, for example known hospitalisations (Loftus, 1993). Several surveys of people with documented CSA have found that some of the people fail to report these events. The most recent of these surveys, by Goodman *et al.* (2003), found a non-disclosure rate of around 19%. The authors suggested that a lack of willingness to disclose, as opposed to a lack of memory, was the most parsimonious explanation for the apparent ‘inaccessibility’ of memories for these documented

episodes of abuse (see also McNally, 2003, for a comprehensive review).

However, prior to 1995, two accounts special mechanisms were generally put forward to explain the inaccessibility of memories for some events: repression and dissociation. Repression has historically been a difficult concept to define and several mutual incompatible definitions exist. This led to strong criticism of the concept and of the evidence for it (Holmes, 1990). As a result, recent investigations have focussed on more selected definitions of the concept, akin to motivated forgetting (Brewin & Andrews, 1998). The second account for explaining the inaccessibility of certain memory is dissociation, or dissociative amnesia (Brown, Schefflin & Hammond, 1998). The dissociative amnesia model suggests that, rather than people consciously or unconsciously 'repressing' memories, individuals learn to deal with traumatic events by dissociating from them. It is argued that in extreme cases this can lead to Dissociative Identity Disorder (DID, formerly called Multiple Personality Disorder, MPD). Problematically, given its relationship with memory distortions mentioned above, the most widely used measure of dissociativity is the Dissociative Experiences Scale (DES; Bernstein & Putnam, 1986). While researchers have found that people who have PTSD do indeed report higher levels of dissociation there has been a growing realization within mental health professions that DID can also result from therapy (e.g., Ross, 2001).

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**Most of the studies examining individual differences in forgetting have examined...repressor personality types. These are people who report being low anxious, but have high defensiveness.**

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There is less laboratory work on forgetting memories (i.e. factors which may reduce levels of reporting for witnessed events) than there is on creating memories (i.e. factors that may lead individuals to report events that did not occur). The two most relevant procedures are the directed forgetting task and retrieval induced forgetting, which can be related to the concepts of repression and dissociation, respectively (see papers in Wessel & Wright, 2004, for studies using both of these procedures). Due to space considerations we focus just on retrieval induced forgetting. Anderson and colleagues (e.g., Anderson & Spellman, 1995) have shown that re-presenting some associated words from lists of studied words decreases the likelihood that other studied words will be reported. They call this retrieval-induced forgetting. Like the Deese-Roediger-McDermott (Roediger & McDermott, 1995) studies showing that people falsely report semantically

related words, the applicability of these studies to memory for events may be limited (Freyd & Gleaves, 1996), but important extensions have been made. Barnier, Hung and Conway (2004) have extended the notion of retrieval-induced forgetting to autobiographical events and have found such forgetting for positive, negative, and neutral events. Wright, Loftus and Hall (2001; Wright, Mathews & Skagerberg, 2005) showed that re-presenting stories without certain critical scenes indeed lowered the likelihood that these critical scenes were recalled. They argued that this situation is analogous to the situation where a perpetrator acts as if the abuse has not occurred and that such behaviour could make memories of the abuse less accessible.

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**While the results are complex, it is clear that repressive coping style is related to the failure to report negative stimuli in many circumstances.**

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Most of the studies examining individual differences in forgetting have examined what is called repressor personality types. These are people who report being low anxious, but have high defensiveness (e.g., they state they are not anxious but show some of the signs of being anxious; Myers, 2000). Some of this research, showing that repressors are less likely to remember negative autobiographical memories (Davis, 1987) was conducted before 1995 and influenced the BPS's *WPRM*. Several laboratories are now looking at how repressors differ on different laboratory tasks (Barnier, Levin & Maher, 2004; Myers & Derakshan, 2004). While the results are complex, it is clear that repressive coping style is related to the failure to report negative stimuli in many circumstances. Further research is needed on the link between dissociation and false reporting to gain a greater understanding of the processes involved. However, as we will now show, conducting research assessing the extent of non-reporting is difficult as people generally lack a reliable metacognitive awareness regarding memory.

**Remembering Forgetting and Forgetting Remembering**

Is there any point during today where you had forgotten what you had for breakfast this morning? This is not a philosophical conundrum, but an important question about people's ability to make metacognitive judgments about their own memories. There are two aspects of these metacognitive judgments that are important for the recovered memory debate. The first aspect relates to a question some mental health professionals asked in order to help them determine whether a client might have experienced trauma as a child. They would ask if there were any periods during the client's life for which they had

few or no memories (i.e., remembering forgetting). If a client reported such gaps in their memory this could suggest, to some, that some traumatic event had caused these periods of amnesia. The use of techniques intended to uncover or access these supposed 'hidden' memories might then appear justified. However, Belli *et al.* (1998) wondered whether the way this question was asked could produce the belief that the person had memory gaps. They found that if, before asking the question about periods of amnesia, participants were asked to recall several memories from that period, this increased the chances that they would indeed report significant gaps in their memory. Thus, responses to this question are liable to bias and are an unreliable way to show whether an individual really does have atypical gaps in memory compared to the general population.

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**From biologically impossible events...to alien abduction claims...people clearly come to believe in events that never occurred.**

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The second aspect of these metacognitive judgments is that people often forget that they have previously remembered an event (Padilla-Walker & Poole, 2002; Parks, 1999). Merckelbach *et al.* (2006) have conducted one of the most relevant of these studies for the recovered memory debate. They asked people to report vivid memories for some childhood events. After either a 1-hour or a 2-day delay, they were asked if they had recently thought about any of these events and several others. Despite recalling the events either an hour or a couple of days before, many participants reported not having thought about the events for years. Critically, Merckelbach *et al.* compared people reporting continuous memories of CSA with those who reported recovered memories of CSA. The people reporting that they had recovered memories of CSA were more likely to forget remembering the recent events in their laboratory tasks. This finding has important implications. Could it be that these people had recalled the CSA continuously (or at least fairly often), but just forgot remembering it?

### **Case Studies**

Different types of case studies have been used to illustrate the different processes described above. Illustrating false memories is simple. From biologically impossible events (Wagenaar, 1996) to alien abduction claims (French, 2001, 2003), people clearly come to believe in events that never occurred. Well-documented case histories exist for some cases, like retractor cases against therapists (e.g., Bennett Braun, Roberta Sachs; see Bikel & Dretzin, 1995). These show that, without the constraints of psychology ethics committees, it is possible to create memories for truly traumatic and abusive events that did not occur. The

number of these case histories has increased dramatically since 1995.

For methodological reasons, case studies demonstrating recovered memories are more difficult to find. While a memory for space abduction can be taken as *prima facie* evidence of a false memory, to show a true recovered memory it is necessary to show that a) the event occurred, b) that the person could not remember the event for a period subsequently, and c) that the information recovered could not have been gained from other sources (Schooler, Ambadar & Bendiksen, 1997).

The largest archive of cases consists of, at the time of writing, 101 cases of 'corroborated recovered memories' (Cheit, 2005). To be included, the case must have 'strong corroboration', but this can simply mean testimony from other witnesses (which can be problematic; see Garven *et al.*, 1998). Cases can also be included on the basis of 'corroboration of significant circumstantial evidence'. In reading through the cases, it appears being found guilty in court is another form of corroboration. Of course both inclusion in Cheit's archive *and* the court decision should be based on other evidence. Critical and detailed scrutiny of many of these cases can lead to a sceptical view of the accuracy of many of these memories. Further, Cheit does not list *not* remembering the event, and evidence for this, as a criterion. This does not mean that the cases on this list are not examples of true recovered memories, only that the requirements to be in this archive are not as stringent as, for example, in Schooler *et al.* (1997). Schooler *et al.* have produced a smaller archive, but one which we feel takes more care to make sure, for example, that there is a period of non-remembering.

Still, often even surpassing Schooler's criteria does not necessarily mean that the memory is a true recovered (or 'discovered', which is the word Schooler prefers) memory. A case discussed by both Cheit and Schooler, and reported in Corwin and Olafson (1997), appeared to show a watertight case of a true recovered memory. Corwin and Olafson provided convincing evidence of the abuse, and provided no reason to doubt that it took place. However, when Loftus and Guyer (2002a, b) looked more closely at the case it was clear that Corwin and Olafson had left out information that would have been useful to most readers to decide how watertight this case was. It is worth reading these (all available on the web) to make your own mind up about this fascinating case. It is important to remember that this is just a case study. If you conclude that this case is not a watertight example of a true recovered memory, this does not show that some recovered memories are not true.

### **Summary**

Since 1995 and the BPS' *WPRM* there has been much research on reports of memories for events that have allegedly been recovered after a long period of non-remembering. The belief that some of these claims are

based on events that did occur, some are based on events that did not occur, and some a combination of the two was held by us then, and the research over the past decade has not changed this overarching view. There has been a great deal of laboratory and case study research showing that people can be led to report falsely that they remember events that never occurred. Research has also shown that people sometimes do not report events that did occur and that some people do this more than others. What we know now that we did not know then is much more about the conditions under which these situations occur.

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**Psychology as a discipline should be judged on three aspects: how well the scientific findings are used to resolve the debate, how efficiently changes are implemented in concordance with the evidence, and, if necessary, how the discipline acts to rectify any mistakes.**

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How will history judge the discipline of psychology in relation to the recovered memory debate? Debate is bound to occur in any scientific field and when science impacts on society (which it should) this is going to create controversy. Psychology as a discipline should be judged on three aspects: how well the scientific findings are used to resolve the debate, how efficiently changes are implemented in concordance with the evidence, and, if necessary, how the discipline acts to rectify any mistakes.

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## CAN WE FORGET BAD MEMORIES?

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**James Ost and Kimberley Wade**

*Dr Ost and Dr Wade are at the Departments of Psychology of the University of Portsmouth and the University of Warwick respectively.*

On Friday 13th July 2007, the *Daily Telegraph* ran a short piece entitled 'How to forget bad memories' reporting on recent research which claimed to show that, 'with enough practise, disturbing and stressful memories can be "forgotten"' (Highfield, 2007, p. 10). The experiments, conducted by Depue and colleagues at the University of Colorado, indicated that certain parts of the brain became more active when participants tried to forget emotional material. This type of research is important. It could help inform current debates over the fate of traumatic memory – from individuals who claim to have repressed memories of abuse, to combat veterans with post-traumatic stress disorder who try to distract themselves from remembering the trauma they have suffered. So does this research - as the newspaper headlines suggest - tell us how to forget bad memories?

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**There are numerous theoretical and methodological problems with brain imaging techniques.**

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Well, as it turns out, the answer is not quite as sensational as the headline. The Depue *et al.* experiments are an extension of earlier work by Anderson and colleagues. Whilst Anderson *et al.*'s work has been expertly critiqued elsewhere (see Garry & Loftus, 2004; Hayne, Garry & Loftus, 2006; Wade, 2007) we need to

first revisit their findings in order to place Depue *et al.*'s experiments in context.

In 2001, Anderson and Green published a paper in the leading journal *Nature* in which they claimed to have found a region of the brain responsible for suppressing unpleasant memories. In their study, participants were first asked to learn sets of word pairs (e.g. ordeal – roach), so that presenting the first cue word (e.g. ordeal) would lead participants to respond with the second, target, word (e.g. roach). Next, participants were presented with the first cue word of each pair (e.g. ordeal) followed by a cue to either 'remember', or 'forget' the target word (e.g. roach). This is referred to as the Think/No Think (T/NT) paradigm. In the final stage, participants were presented with all the cue words (e.g. ordeal) and asked to recall all the target words (e.g. roach). Anderson and Green (2001) found that participants recalled fewer of the target words that they had been instructed to forget than the target words that they had been instructed to remember. This, they argued, was evidence that people can learn to selectively 'block out' certain memories. In a follow up study, published in *Science* in 2004, Anderson *et al.* repeated the experiment. This time, however, whilst participants were attempting to 'remember' or 'forget' target words (e.g. roach) the experimenters used fMRI (*note 1*) to measure participants' blood flow to different parts of the brain. As a result of this new experiment, Anderson *et al.* claimed that they had discovered which areas of the brain were responsible for the suppression of unwanted material.

So had Anderson and colleagues finally found concrete evidence that the mind can block out horrific events? Whilst some journalists appeared convinced (one headline in the UK at the time read 'FREUD PROVED'), psychologists were more circumspect (Garry & Loftus, 2004; Hayne *et al.*, 2006; Wade, 2007). Garry and Loftus (2004), in an article in the *Skeptical Inquirer*, pointed to several limitations with the Anderson *et al.* findings, a few of which are summarised here. Firstly, there are numerous theoretical and methodological problems with brain imaging techniques. For example, do increases in 'metabolic' activity (e.g. blood flow) to an area of the brain necessarily indicate an increase in 'cognitive' activity? At what point, statistically, do we conclude that one part of the brain has become 'more' or 'less' active? By creating an 'average' brain scan from the scans of different participants, are we running the risk of masking important individual differences in brain structure? Can we be sure that the brain areas identified are *facilitating* the processes under investigation (e.g. suppression), or could they be *inhibiting* another response instead? Brain imaging research is still in its infancy and many of these questions are a long way from being resolved. Thus any data need to be treated with caution. (The fact that psychologists appear to have been seduced by these technological advances led William Uttal to title his 2001 critique of neuropsychological techniques, *The New Phrenology*).

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**The nature of the stimulus material used does not allow us to generalise to the kinds of traumas associated with repression or PTSD.**

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Secondly, the effects of the T/NT paradigm appears to be quite fragile. As Garry and Loftus (2004) noted, the degree of suppression in the Anderson *et al.* experiments was not particularly severe - instructing participants to 'forget' the target word (e.g. roach) led to a 10% reduction in recall. Even then, participants still recalled about 80% of the target words. Importantly, another group of psychologists have failed to replicate these findings in three separate experiments (Bulevich, Roediger, Balota & Butler, 2006; Wade, 2007). Such a fragile effect is not convincing evidence of a mechanism which would presumably be required to block entire traumatic, autobiographical episodes from consciousness.

Finally, the nature of the stimulus material used does not allow us to generalise to the kinds of traumas associated with repression or PTSD. Freudian repression allegedly results in the blocking from awareness of traumatic, threatening and emotional information. A diagnosis of PTSD requires exposure to a Category A traumatic stressor (such as witnessing someone being killed, McNally, 2003). As Garry and Loftus (2004) argued, word pairs (e.g.

ordeal-roach) hardly mirror the impact of this kind of material. However, it is this last criticism that was recently addressed by Depue and colleagues in a replication and extension of the Anderson work (Depue, Banich & Curran, 2006).

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**So does this research tell us how to forget bad memories? The stark answer, despite newspaper claims to the contrary, is no.**

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Depue and colleagues (2006) wanted to find out whether the suppression effects found for word pairs would be replicated when more emotional material was used. Thus, rather than using words as both targets and cues, as Anderson and colleagues had done, they used faces as cues, and either emotionally neutral or negative words, or pictures, as targets. Participants first practised recalling 40 face-word, or face-picture, pairs until they could recall them with a high level of accuracy (97%). They then took part in an experimental phase where they were shown 32 of the face cues. Sixteen of these face cues were paired with an instruction to 'think about' the associated word or picture targets, whilst the other 16 were paired with an instruction to 'not think about' the associated word or picture targets. For half of the face cues, these 'think' or 'no think' instructions were repeated five times and for the other half they were repeated ten times.

Depue and colleagues found that participants who were instructed to 'think' about the targets ten times, recalled more of those targets in a final test than participants who were given the 'no think' instructions (importantly, the 'no think' instructions led participants to recall fewer word or picture targets compared to baseline word or picture targets for which they had been given no instructions). The emotional nature of the stimuli also seemed to magnify the effect. Participants recalled more of the emotional word or picture cues after ten 'think' instructions, than they did of the neutral word or picture cues. Similarly, participants recalled fewer of the emotional, compared to neutral, word or picture cues after the 'no think' instructions. Thus, according to Depue and colleagues, there is a 'cognitive control' process in the brain which deals differently with emotional and non-emotional memories. When emotional material is repeatedly processed (or thought about) it becomes more accessible than neutral material, but when emotional material is repeatedly suppressed (not thought about) it becomes less accessible. In a follow-up fMRI study, the authors found evidence of two neural mechanisms which appear to be implicated in the suppression process (Depue, Curran & Banich, 2007).

So does this research tell us how to forget bad memories? The stark answer, despite newspaper claims to

the contrary, is no. We've all had the experience of cringing, and trying to distract ourselves, when a memory for an embarrassing event suddenly comes to mind. Most of us try, often with limited degrees of success, not to think about events that upset us. This is called *suppression* – and psychological research shows that we are not very good at it. This is mainly because the rule ('I must try not to think about X') contains the thing one is trying to forget. Thus, most of us cannot help but picture a white bear when explicitly instructed not to (Wegner, Schneider, Knutson & McMahon, 1991). Likewise, survivors of trauma generally find it difficult *not to think* about the events they have witnessed (McNally, 2006). Nothing in the Depue *et al.* experiments suggests that people can be trained to do this more effectively. If it *could* be shown that people could learn to effectively 'suppress' traumatic memories – would this offer hope for PTSD sufferers? Again – contrary to the newspaper article – the answer is no. Most treatments for PTSD do not involve helping survivors to 'forget' their experiences. Rather they aim to help sufferers change the way they react to, and cope with, their traumatic experiences (although PTSD itself is highly controversial; see Rosen, 2004, for excellent discussions).

More worrying, however, is the claim made in the opening lines of the Depue *et al.* (2007) article. They state that whilst there is evidence that people actively try to suppress memories, 'others claim that memory repression or suppression is a clinical myth in search of scientific support' (p. 215). This sleight of hand, in which *suppression* and *repression* are conflated, is problematic – the two terms are not interchangeable. *Suppression*, as noted above, refers to cases where people actively try *not to think about* something, usually with very limited degrees of success (Anderson & Green, 2001; Depue *et al.*, 2007). *Repression*, on the other hand, is when an individual is allegedly *unable to remember* something because the mind has unconsciously blocked out any memory of the event. Years of psychological research have indicated that this is indeed a 'clinical myth' (Hayne *et al.*, 2006; Kihlstrom, 2002; note 2).

Yet the work of Anderson and colleagues, and Depue and colleagues, has somehow been seized on as providing evidence that it exists. This is probably because important qualifications – such as the distinction between *suppression* and *repression* – are of little interest to non-psychologists and headline writers. The accurate reporting of scientific findings is critical if we are to prevent further confusion in an area already plagued with misunderstanding and therapeutic folklore. Unfortunately, media reports of the Anderson and Depue work have provided the 'take home' message that the latest advances in technology are showing that people can, consciously or unconsciously, block out memories of traumatic events. In fact, the evidence actually supports the opposite conclusion – survivors of trauma

generally have difficulty forgetting the experiences they have been through (McNally, 2006).

### Notes

1. (*Editor's note*) fMRI is functional magnetic resonance imagery
2. See Loftus and Guyer (2002a and 2002b) for discussion of the case of Jane Doe in which an allegedly 'repressed' memory was 'recovered' during a videotaped interview.

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## BOOK REVIEWS AND COMMENTARIES

### ON 'GOD-BASHING' BEST-SELLERS AND OTHER PUBLICATIONS

#### Brian Robinson

*Brian Robinson is a retired Consultant Psychiatrist living in Milton Keynes.*  
(Website: <<http://musicweaver.users.btopenworld.com/>>)

The physicist Paul Davies has recently pointed to 'the recent spate of God-bashing bestsellers'.<sup>1</sup> I wrote a slightly sceptical note<sup>2</sup> about one of them, Richard Dawkins' *The God Delusion* for the *Skeptical Intelligencer* recently: 'sceptical' not because I disagree with Dawkins (I very much agree with him) but because I wondered whether that kind of approach helped to stem the tide of unreason threatening to engulf our world, or whether it merely entrenches reactionary attitudes.

#### **Dawkins is a cuddly pussycat compared to the claws of Hitchens' tiger.**

Readers are probably familiar with the books by the philosopher and neuroscientist Sam Harris critiquing religion, *The End of Faith: Religion, Terror and the future of reason*, and *Letter to a Christian Nation: A challenge to the faith of America*, as well as the recent book by the American philosopher of evolution Daniel Dennett, *Breaking the Spell: Religion as a natural phenomenon*. Dennett's book is rather different in approach: he doesn't attack religion so much as seek, as the subtitle implies, to explain its universality and persistence through history.

I want here to mention two more recent ones, and to link them with a lecture that I attended a few weeks ago. The first<sup>3</sup> is by the brilliant, controversial and fearlessly pugnacious Christopher Hitchens, which he has titled in typically provocative fashion, *god is not Great* (*sic*, although the lower case use becomes clear only after the Contents page; until then the title is wholly in capitals). The publishers rather unspontingly changed the subtitle for the British edition to 'The case against religion', whereas in the USA it was 'How religion poisons everything', which not

only sounds more like the Hitch we know and love, but is also a phrase that is echoed several times in the early chapters of the book.

A religious acquaintance told me how upset he'd been by *The God Delusion*, and I had to tell him that Dawkins is a cuddly pussycat compared to the claws of Hitchens' tiger. A selective list of the latter's chapter headings will give you an idea: 'Religion Kills, A short digression on the pig, or why heaven hates ham', 'The metaphysical claims of religion are false', 'The nightmare of the 'Old' Testament', 'The 'New' Testament exceeds the evil of the 'Old' one', 'The Koran is borrowed from both Jewish and Christian myths', and 'Is religion child abuse?'. But his last chapter, which offers a bit of hope after all the warnings is, 'The need for a new enlightenment'.

#### **Opposing religion is not merely an academic exercise. The manifestations of religious fundamentalism threaten our very survival as a species.**

In this note I can't really convey the wit and erudition that Hitchens deploys. And the humour (I laughed out loud several times) does not disguise the sheer horror of some of the religious cruelty he describes. Hitchens is such a wonderful phrase maker that his vivid depictions made me react with sudden disgust and once or twice left me with an almost overwhelming sadness at some of the things done by human beings to other human beings, often with the most absurd rationales, yet always in the name of a god of one kind or another.

He mentions religion's ceaseless brooding on 'eschatology' and 'the passing away of all earthly things'. He writes:

'This death cult refuses to abate, even though we have every reason to think that 'earthly things' are all that we have, or are ever going to have. Yet in our hands and within our view is a whole universe of discovery and clarification, which is a pleasure to study in itself, gives the average person access to insights that not even Darwin or Einstein possessed, and offers the promise of near-miraculous advances in healing, in energy, and in peaceful exchange between different cultures. Yet millions of people in all societies still prefer the myths of the cave and the blood sacrifice.'

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**'Faith is what I die for;  
dogma is what I kill for'.**

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What all these new 'God-bashing' books have in common is that opposing religion is not merely an academic exercise. The manifestations of religious fundamentalism threaten our very survival as a species. And the theme is picked up by the next book, small in size but packed with ideas, by the philosopher A.C. Grayling<sup>4</sup>. Sixty-four pages of fairly large print, called *Against All Gods: Six polemics on religion and an essay on kindness*.

To read Grayling is to enter a very different tone-world from that inhabited by Hitchens. This is sober, closely-argued, measured stuff, but no less entertaining for that:

'It is time to reverse the prevailing notion that religious commitment is intrinsically deserving of respect, and that it should be handled with kid gloves and protected by custom and in some cases law against criticism and ridicule. ... Everyone is free to believe what they want, providing they do not bother (or coerce, or kill) others; but no one is entitled to claim privileges merely on the grounds that they are votaries of one or another of the world's many religions.'

Grayling clears up all the woolly thinking around words and phrases like 'fundamental atheist', 'secularist', 'humanist' and 'atheist'. He quotes some astonishing, and alarming, findings, such as 'An Opinion Panel Research survey conducted in July 2006 found that more than 30 per cent of UK university students believe in creationism or intelligent design'. What makes this especially troubling is that it is

'a symptom of a wider corrosion, the spread of a more virulent cancer of unreason, which is affecting not just

the mental culture of our own country but the fate of the world itself.'

Grayling sees this as resulting from a combination of under-investment in education, a dumbing down of the exam system, and fashionable post-modern relativism, the last of which has encouraged

'teachers to accept, and even promote as valid alternatives, the various superstitions and antique belief systems constituting the multiplicity of different and generally competing religions represented in our multicultural society.'

In addition, the increased licence given to 'faith-based schooling' has meant

'the ghettoisation of intellectually defenceless children into a variety of competing superstitions, despite the stark evidence, all the way from Northern Ireland to the madrassahs of Pakistan, of what this does for the welfare of mankind.'

Thus 'enquiry is no longer premised on the requirement that belief must be proportional to carefully gathered and assessed evidence'. For a worryingly significant number of people, faith 'is enough to legitimate anything from superstition to mass murder'. Grayling quotes 'Faith is what I die for, dogma is what I kill for'.

Yet in the end, despite the assertions from many quarters that religion is having a resurgence, Grayling argues that the deceptive manifestations of this are in reality its death throes. Political events over the last half-century gave rise to heightened Muslim activism, which in turn prompted a rise in activism by other religious groups 'not wishing to be left behind'. The response of politicians and the amplifying effect of media reportage make what is really a reaction to decline appear to be a resurgence. Grayling backs up his case with reference to church attendance figures in Britain (and even those who believe vaguely in 'Something', are functionally secularist) and by citing historical precedent. His last chapter deals with 'The alternative: Humanism', and the very last word in his book is, fittingly, 'reason'.

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**In many ways the contemporary 'left' has come to believe that its solidarity should be transferred from the traditional working class to oppressed groups throughout the world.**

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All the aforementioned books are highly relevant in considering a lecture I attended a couple of weeks ago given by the journalist Nick Cohen, who last year published his book<sup>5</sup>, *What's Left?: How liberals lost their way*. So highly did I rate this book that my wish to hear him speak about it in person brought me to a synagogue for

the first time in rather more decades than I'm willing to reveal here; well, actually, to be more accurate, it was the synagogue communal centre next door, but it felt almost the same thing. It's rather beside the point, but in view of the context, perhaps I should mention that Cohen isn't Jewish, and he'd been invited to deliver the 5<sup>th</sup> Annual Sir Isaiah Berlin Lecture.

Cohen, as with many who have not lost their sense of what it has always meant to be on the left, is greatly influenced by the writings of Paul Berman, in particular his book *Terror and Liberalism*.<sup>6</sup> Although many of the things that progressive politics fought for have been achieved in terms of better living and working conditions for working people, in many ways the contemporary 'left' has come to believe that its solidarity should be transferred from the traditional working class to oppressed groups throughout the world, in particular aligning itself with groups perceived to be in revolutionary opposition to America.

Those same forces described by Grayling and Hitchens especially, are now seen in the light of 'My enemy's enemy is my friend': if they're against America they're OK by us. And this has meant that instead of, as you'd have expected, the left lining up to support progressive individuals and groups within Islamic society, they have joined up with the most reactionary elements.

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**People on the left can get so used to condemning what's wrong with our own country, they can't see that what's happening abroad is worse.**

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For example, we have the spectacle of a prominent politician, supposedly on the progressive left, who could have chosen to support those highly courageous people working from within Islamic communities in Britain for a non-political, progressive, reformed expression of Islamic belief and practice. Instead he chose to embrace (literally and figuratively) a rightwing bigot who has supported suicide bombings in Israel, ('It's not suicide, it is martyrdom in the name of God'<sup>7</sup>), defended the death penalty for gays, supports female genital mutilation and says it's OK for a man 'in certain circumstances' to beat his wife 'lightly ... avoiding her face and other sensitive areas'.<sup>8</sup>

As Cohen sees it, people on the left can get so used to condemning what's wrong with our own country, they can't see that what's happening abroad is worse, and this can also make them unable, or reluctant, to acknowledge faults in those they identify as victims of western imperialism. It is, in effect, a kind of racism.

He drew some challenging conclusions. One is that it's very difficult today to define what it is to be leftwing.

Socialism as it was has gone, and this frees people up a lot: the left has had an easy time of it because America has been so easy to be against, and being 'against' has obviated the need to elaborate a true leftwing programme of its own.

So free, in fact, are the so-called liberal left, that they can, seemingly without noticing, go along with the kind of people whom they would once have considered utter taboo. This amounts to what Cohen calls a consumerist view, and as he writes in his book,

'Like a shopper walking through a mall, you have no loyalties and no duties and can breeze into any store that takes your fancy. All you must be is against your own Western government and against America.'

Having said all that, despite Grayling, Hitchens, Harris, Dawkins and the rest, will we ever be able to stop being moidered by gods? Nick Cohen believes that fear is a greatly underestimated factor in politics. The psychoanalyst, sociologist and humanistic philosopher Eric Fromm, in the darkness of the early 1940s, wrote what may perhaps be the most relevant of his books for us today, *The Fear of Freedom*<sup>9</sup> in an attempt to explain the enduring human willingness to be seduced by, and submit to, authoritarian power.

Bertrand Russell once referred to man's 'cosmic loneliness' and the philosopher Colin McGinn, in conversation<sup>10</sup> with Jonathan Miller, said he thought that human consciousness made for a great sense of isolation that fuelled the need for some kind of personal god. Perhaps, in the end, some of our best thinkers, such as Dawkins, Grayling, Harris and the rest, need to stop implicitly denying, or at least explicitly minimising, the enormity of our existential loss and its associated psychological pain. Whilst they are certainly right to celebrate the defeat of unreason where they find it, they pay too little attention to our most urgent need amidst the terrors of today's world, namely how to learn to live, at ease and in tranquillity, with the god-shaped vacuum at the very heart of our being.

## References

<sup>1</sup> Paul Davies: 'Yes, the universe looks like a fix. But that doesn't mean that a god fixed it' *Guardian Comment*, June 26, 2007,

<<http://www.guardian.co.uk/comment/story/0,,2111345,00.html>>.

<sup>2</sup> On Richard Dawkins and *The God Delusion*, *Skeptical Intelligencer* 9, 2006.

<sup>3</sup> Christopher Hitchens: *god is not Great: The case against religion*. London: Atlantic Books, 2007.

<sup>4</sup> A C Grayling: *Against All Gods: Six polemics on religion and an essay on kindness*. London: Oberon Books, 2007.

<sup>5</sup> Nick Cohen: *What's Left?: How liberals lost their way*. London: Fourth Estate, 2007.

<sup>6</sup> Paul Berman: *Terror and Liberalism*. New York and London: W.W. Norton & Company, 2004.

<sup>7</sup><http://news.bbc.co.uk/1/hi/programmes/newsnight/3875119.stm>.

<sup>8</sup><http://www.indymedia.org.uk/en/regions/world/2005/02/305315.html>.

<sup>9</sup> Published in America as *Escape from Freedom*.

<sup>10</sup><http://www.bbc.co.uk/bbcfour/documentaries/features/atheism-tapes.shtml>.

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## *The Science of the Dogon*

by Laird Scranton, Inner Traditions, Rochester VT, USA, 2006, xv + 208

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### Mark Newbrook

*Mark Newbrook is a researcher in linguistics, currently affiliated with Monash University and the University of Sheffield. Email: <morcusporcus@btinternet.com>.*

In this book, originally published in 2002, Scranton develops further the ideas of Griaule and Temple (see *The Sirius Mystery*, 1976; 2<sup>nd</sup> edition 1998) about the cosmological knowledge of the Dogon tribe in Mali. Like Temple but more systematically, he argues that the conceptual and symbolic cosmological system of the Dogon is largely shared with that of ancient Egypt. He goes on to claim links with Buddhist thought and symbolism and with other non-local ideas on these matters. Still more dramatically, he holds that spoken Dogon word-forms (the Dogon are illiterate), corresponding Egyptian words, Dogon symbolism considered as a system and aspects of Egyptian hieroglyphic writing collectively demonstrate awareness of cosmological truths discovered only recently by modern science (e.g. string theory). Scranton believes that the observed similarities are too close, numerous and systematic to have arisen by chance. If he is right, the upshots are obviously major.

The upshots of Temple's claims about the Dogon were also major (notably, extrasolar visitation in ancient times); but sceptical reviewers were able to explain the symbolic and mythological data in other less dramatic terms. One of the sceptical reviews was prepared by me in conjunction with Groves (*The Skeptic* [Australia] **19:4**, 1999, pp 56-60; short version on Amazon.com). Among other objections, Groves and I showed that Temple's linguistic evidence, specifically, is much weaker than he suggests. He argues extensively for contact/shared origins on the basis of superficial and unsystematic similarities between words in Dogon, Egyptian and other languages not generally believed to be linked (notably Greek). This method is very typical indeed of work in this area by non-linguists but is the best part of 200 years out of date in terms of linguistic scholarship. **All** qualified/well-informed historical linguists,

whatever their differences on other issues, would agree that this method can easily be shown to be wholly unreliable. The likelihood of accidental unsystematic similarity is much greater than such writers realise, and in cases where the cross-linguistic correspondences between phonemes are incorrigibly unsystematic (no specific explanation for the lack of systematicity) the likelihood that they involve common origin or contact is actually very small. I have rehearsed this argument in detail in many publications; see e.g. 'Linguistic reconstruction and revisionist accounts of ancient history', *The Skeptical Intelligencer* **7**, 2005, pp 22-33.

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### **Unlike Temple, who apparently believes for some reason that he understands historical linguistics, Scranton proceeds as if the discipline did not exist.**

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Scranton argues in similar ways, and the same objections apply again. He also treats as authoritative earlier authors such as Higgins (pp 174-175) who made the same mistake or wrote before these matters were understood. And he compounds his problems by insisting on using Budge's older transliterations of Egyptian words, which – as he well knows – are deemed outdated but which suit his case marginally better than those transliterations which are now accepted by Egyptologists. (His case is still impossibly weak, however, even using Budge.)

And, unlike Temple, who apparently believes for some reason that he understands historical linguistics, Scranton proceeds as if the discipline did not exist. In fact, whether or not he has a case firmly grounded in semiotic and other



non-linguistic similarities, the linguistic evidence *per se* offers him no support at all. And, in some cases, he himself admits in so many words that his equations are speculative, e.g. where he discusses some Hebrew roots on p 25.

In fact, Scranton's amateur view of cross-linguistic relationships between words is particularly obscure and strange. Without actually being aware of the objections that a linguist would raise to his equations, but perhaps concerned about this aspect of his case, he covers himself (p 3) by saying that when he proclaims an equation between e.g. a Dogon and an Egyptian word for (allegedly) related concepts he is not necessarily referring to 'a strict linguistic lineage for the words'. But it is not at all obvious how any **other** kind of relationship might be valid. Scranton goes on to say that he regards such pairs of words as related within a larger symbolic system. Now that might sometimes be the case, and as I note below his viewpoint might be somewhat strengthened thereby; but, even if this were so in a given case, that would not absolve him of the need to explain in what other (non-'strict'?) way the words of a pair are themselves related to each other. Neither would it absolve him of the need to demonstrate that they themselves (as opposed to their senses) really are related.

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**When apprised of some of my objections, Scranton was gracious enough and talked as if he was taking them on board, but in fact he recanted nothing.**

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Scranton also has a somewhat strange perception of contemporary mainstream scholarly views on the written Egyptian language itself. For instance, he seems to think (correspondence) that he is being radical in regarding some hieroglyphs as logographic (read for meaning, not for sound). (He then goes on to link this idea with his own specific theories.) But the script as a whole clearly developed out of a logographic script, and Egyptian language scholars already know very well that in dynastic times some glyphs – including e.g. one referring to periods of time, upon which Scranton focuses – were still read for their meanings in various contexts.

When apprised of some of my objections, Scranton was gracious enough and talked as if he was taking them on board, but in fact he recanted nothing. He holds that his overall case is overwhelmingly strong regardless of such objections. I will comment here mainly on the linguistic material, where my own expertise lies. I do not myself find many of Scranton's cross-cultural equations of non-linguistic items persuasive either, but I leave it to scholars of semiotics and the cosmologies of early cultures to deal authoritatively with those aspects of his work. Overall, my view is that the individual equations, of all kinds, are too

weak to sustain a case, however numerous they may be. And this is certainly the case for the linguistic equations.

There are several more specific objections to Scranton's linguistic equations:

1) Many of the pairs of words which he regards as significant are very short; this obviously increases further the chance of accidental similarity. Examples are his discussion of words beginning with *te-* (p 87), Dogon *po* and Egyptian *pau-* (p 93), and Dogon *dada* and Egyptian *dd* (p 97).

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**A linguist's provisional conclusion must be, as noted, that there is no worthwhile specifically linguistic evidence for a link between Dogon and Egyptian.**

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2) Many of the alleged similarities are very approximate only. Without a significant degree of systematicity across many such pairs, cases of this kind cannot be taken seriously. For example, Egyptian *hpr*, or even Budge's version *khepr*, and Dogon *ke* (which in any case have only roughly similar meanings) share only the vowel (reconstructed in the case of Egyptian; vowels are not represented in the Egyptian hieroglyphic script) and a very approximate area of oral articulation for the initial consonant (p 7).

3) In comparing linguistic forms, it is necessary to examine known etymologies and to use the forms (known or reconstructed) from the relevant period. In this case the relevant period is clearly ancient. This has the consequence that Spanish or German forms (and indeed, very probably, current Dogon forms) cannot be used in comparison. Spanish *ojo* and German *Auge* (p 154; both mean 'eye') must be traced back at least to their Latin and Proto-Germanic origins, respectively, and if possible further back, before any comparison with Dogon or Egyptian words can be usefully carried out. Similarities between Dogon or Egyptian and current Spanish or German words (especially if approximate only, as in the case of Egyptian *aakhu* and *ojo/Auge*; see above) **cannot** be of any significance.

4) In some of Scranton's examples, it is actually **known** that the words in question are unconnected and the (unsystematic/approximate) similarities accidental. For example, Spanish *dios* and *diez* are unconnected (p 25); the former is from Latin *deus*, the latter from Latin *decem*, and the Latin words have separate Indo-European sources.

5) Like most non-linguists, Scranton focuses almost entirely on word-forms (lexical phonology and spelling); he ignores not only phonological systems but also grammar. Unless the posited links between e.g. Dogon and Egyptian are supposed to involve **only** contact between the peoples, not 'genetic' relationship, these more structured aspects of

the languages must be considered in assessing claims of relatedness.

If it were clear that many of the Dogon and Egyptian words are really implicated, systematically, in cosmologies that take distinctly similar forms, I grant that the linguistic aspect of Scranton's case would thereby be **somewhat** strengthened (I do not think it would be **decisively** strengthened). But, as I have noted, it does not seem to be at all certain that this is so. As things stand, unless linguists are persuaded that these words **are** implicated in cosmologies with significant similarities, they will regard Scranton's approach not merely as 'not a strictly linguistic approach' (as he calls it) but as an approach to correlating words which is not sound in **any** way.

In face of all this, a linguist's provisional conclusion must be, as noted, that there is **no** worthwhile specifically linguistic evidence for a link between Dogon and Egyptian.

Scranton is publishing a second book in which he will make (and try to defend) further dramatic non-standard claims about the Egyptian language and script (including the notion that written Egyptian was somehow prior to spoken Egyptian) and the links between Egyptian and Dogon. I have corresponded with Scranton on these matters and so far I find his case unconvincing and his view of the Egyptian language and script implausible; but serious comment must await publication.

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## ***Why People Believe Weird Things***

by Michael Shermer, Souvenir Press Ltd, London, 2007, 384pp.

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### **Alan Sausse**

*Alan Sausse is a postgraduate student in Philosophy at the University of Durham. He holds an MA (Mathematics) from Cambridge University and formerly worked as an actuary in The City of London.*

Michael Shermer is undoubtedly an important figure in the sceptical movement. He founded *The Skeptics' Society* in the USA and edits its magazine, *The Skeptic*. He is also a regular columnist for *Scientific American*, has written several books, and appeared on various television programmes (including *Oprah*, *Donahue*, Penn & Teller's *Bullshit!*, and *Larry King Live*). Earlier in his life Shermer was a devout Christian but is now an atheist. He holds a PhD in the History of Science from Claremont Graduate University.

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**Just as the credulous may mistake pseudoscience for true science, it is all too easy for the aspiring sceptic to mistake pseudoscepticism (sniggering, sneering, dismissive *ad hominem* attacks, etc.) for true scepticism.**

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*Why People Believe Weird Things* was first published in the USA in 1997 but was not available in the UK until 2007. Arguably, the title of the 313-page book is slightly misleading. It is written in 5 parts. Parts 1 and 5 do indeed explore the reasons for 'weird' beliefs, but parts 2, 3 and 4 tend instead to focus on particular categories and instances of these beliefs – not so much 'why?', as 'what?'. The book

resembles a collection of extended essays rather than a single, coherent whole, and its ending is rather abrupt for that reason – there is no concluding summary.

However it does not attempt to be a compendium of the mystical, the supernatural, etc. There are other books that fulfil that role, such as Robert T Carroll's *The Skeptic's Dictionary* and James Randi's *An Encyclopedia of Claims, Frauds and Hoaxes of the Occult and Supernatural*.

In terms of the categories of 'what' discussed, the book's scope is rather wider than Carroll's and Randi's, since it deals not only with the supernatural but also contains detailed explorations of issues such as the evolution/creation debate, historical revisionism, prejudice and discrimination, near-death experiences and the Ayn Rand cult.

I found Shermer's writing style to be enjoyable and easily digestible. It is not (and does not claim to be) a weighty academic tome, but is none the worse for that. That the author has a powerful grasp of his subject is evident, however, and he conveys his thoughts eloquently and wittily. I particularly welcomed the patient, sympathetic way in which he approaches those who, in his opinion, hold 'weird' beliefs. I suspect that his own deeply religious past allows him to empathise with such people unusually well, even if he clearly does not share their beliefs. It is an important point for us all to consider. Just as the credulous may mistake pseudoscience for true science,

it is all too easy for the aspiring sceptic to mistake *pseudoscepticism* (sniggering, sneering, dismissive *ad hominem* attacks, etc.) for true scepticism. Shermer largely avoids this pitfall.

His conversion to scepticism is explored in an amusing section of chapter 1 where he describes his former career as a professional ultra-marathon cyclist. During this period he tried a large number of rather outlandish-sounding training techniques, approaches to diet, health, etc. and, predictably, came to the conclusion that the only things that really made a difference were 'long hours in the saddle, dedication to a consistent training schedule and a balanced diet.'

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**Scientific 'facts' are only *provisional* – the truth, until a better truth comes along – and can never be known with absolute certainty.**

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Shermer is a passionate defender of the scientific method as a discipline with some unique properties: progressiveness, falsifiability, self-correction, rigorous peer review and the testing of theories against observed data. The first 3 chapters of the book deal with these issues in some detail, and chapter 3 ('How Thinking Goes Wrong'), which gives a description of 25 common fallacies in thought, is a very worthwhile read and is one of the highlights of the book. Nevertheless, he has the wisdom to understand the limitations of science.

Firstly, he sensibly points out that scientific 'facts' are only *provisional* – the truth, until a better truth comes along – and can never be known with absolute certainty. Any sceptic worth his or her salt needs to understand this point. He also emphasises that scepticism is a *method* and not a *position*. It is, correspondingly, a *provisional* approach to claims, a view that our beliefs should be formed on the basis of evidence, and modified if necessary, as new evidence emerges (a quasi-mathematical explanation for this process in terms of Bayesian inference is given in a short but very interesting paper by Robert Matthews with the uncannily similar title *Why do People Believe Weird Things?* which appears in *Significance (The Journal of the Royal Statistical Society)*, Dec 2005, pp 182-184). It is important, therefore, not to confuse scepticism with cynicism or dogmatism.

Secondly, he notes that the practical uses of science and technology have of course caused huge problems: overpopulation, pollution, water shortages, climate change, war, terrorism, etc. He wisely observes that 'our...progress has...given us many ways of causing the extinction of our own species. This is neither good nor bad. It is simply the outcome.' Science *is what it is*, in other words – nothing more, nothing less.

The middle sections of the book are given over to the exploration of three major areas of debate and

disagreement: science/ pseudoscience; evolution/ creationism; and history/ pseudohistory. Arguably these are the weakest sections of the book, since some of the discussions are overlong (particularly the section dealing with Holocaust denial) and do little to answer the question posed by the book's title. Nevertheless, they contain some interesting observations.

Chapter 15, 'Pigeonholes and Continuums', gives an interesting slant on the logical mistakes that humans make in attempting to assign individuals to artificial categories for the purposes of measurement, judgement, etc., and on some of the problems (discrimination, prejudice, etc.) that can result. He quotes Alfred Kinsey (from *Sexual Behaviour in the Human Male*):

'Males do not represent two discrete populations, heterosexual and homosexual. The world is not to be divided into sheep and goats. Not all things are black nor all things white. It is a fundamental of taxonomy that nature rarely deals with discrete categories. Only the human mind invents categories and tries to force facts into separate pigeonholes. The living world is a continuum in each and every one of its aspects. The sooner we learn this concerning human sexual behaviour the sooner we shall reach a sound understanding of the realities of sex.'

It is easy to see how this recognition of the limitations of taxonomy (i.e. the practice of classification) can illuminate our thinking not only with respect to (male) sexuality but also with respect to many other imperfect classifications of individuals such as gender, race, religion and politics.

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**Any suggestion that intelligence is a bar to credulity seems groundless. A more nuanced understanding is needed here.**

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Chapter 17, 'Why Do People Believe Weird Things?', comes closest (as its name suggests) to answering the question posed by the title of the book but, at only 6 pages, is surprisingly short. The key observation, which isn't terribly surprising, is that often people believe what they *want* to believe. They are able to believe that which they find comforting and reassuring, sometimes even in the presence of strong evidence to the contrary.

Shermer makes the point that sceptics are vulnerable to this also. It is a natural human response. There is a metaphysical point at issue here: the *true* nature of reality, time, space, the formation of the universe, etc. is deeply mysterious. In the face of such overwhelming mystery, science may suddenly begin to seem rather feeble, and it could be argued that *all* knowledge (scientific, pseudoscientific or otherwise) involves major leaps of faith. Shermer gives a rather surprising quote from (the sceptic) Martin Gardner, suggesting that leaps of faith can be

justified, if they are not sharply contradicted by science or reason, if one has a strong emotional attachment to a particular belief and if one finds that it provides sufficient satisfaction.

Clearly other forces are at work, as Shermer points out. Simplicity, instant gratification, brainwashing, parental influence, desires to conform to norms, etc. all clearly have a part to play. We can believe what we *want* to believe but we can also believe what we are *told* to believe!

Chapter 18, the final chapter of the book, 'Why *Smart* People Believe Weird Things', is particularly important. It is all too easy for the pseudosceptic to answer 'because they're not *really* smart at all!'. This seems, however, to be a rather flippant response. There are many intelligent, highly educated individuals in the world who nevertheless hold what seem – to the sceptic – to be outlandish beliefs. Any suggestion that intelligence is a bar to credulity seems groundless. A more nuanced understanding is needed here. Shermer states that:

'Smart people believe weird things because they are skilled at defending beliefs they have arrived at for *non-smart* reasons.'

He puts forward a number of possible explanations for this. Firstly, the controversial idea of *domain-specific* intelligence - i.e. that smart people may only be smart in limited number of areas. That they may excel at mathematics or science but still have a 'blind spot' when it comes to critical reasoning is but one example of this.

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**Shermer observes that educated people may be better at defending their pre-existing beliefs and that this may lead them to be more closed-minded than we might expect.**

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Other relationships are examined in order to see if, for example, there are strong links between belief and factors such as age and gender. There appears to be no convincing evidence for such links.

Shermer observes that educated people may be better at defending their pre-existing beliefs and that this may lead them to be more closed-minded than we might expect. He also pithily points out that 'where evidence is lacking, the mind fills in the gaps, and smart minds are better at gap filling.'

He claims that there are two particularly important cognitive biases that make it difficult for humans to evaluate evidence: *Intellectual Attribution Bias* and *Confirmation Bias*.

*Intellectual Attribution Bias* describes the process whereby we falsely attribute the causes of our own and

others' behaviour to either a *situation* or a *disposition*. For example, humans have a tendency to take the credit for their successes (a *dispositional* attribution) but to blame bad luck, circumstances, etc. for their failures (a *situational* attribution).

*Confirmation Bias* is the term used to describe the tendency of humans to overemphasise the importance of evidence that supports their prior beliefs, while at the same time playing down the significance of opposing evidence. It is a familiar and well-understood phenomenon, and it is easy to understand how it can lead to distorted beliefs that bear little relation to the available evidence.

As I mentioned above, there is no real conclusion to the book. However, Shermer comes closest to this, perhaps unwittingly, in chapter 17, where he makes two very telling observations. Firstly, he states that:

'We can never know all of the contingencies and necessities guiding history at any given point in time, let alone the initial conditions of any historical sequence, and from this methodological weakness comes philosophical strength. Human freedom...may be found not only in our inability to process all the data of the past and present but also in our ignorance of the initial conditions and conjunctures of events that shape our actions. We are free in our ignorance, free in the knowledge that most of the causes that determine us are lost to the past, forever.'

He goes on to observe that:

'Humans are...a forward-looking species always seeking greater levels of happiness...unfortunately, the corollary is that humans are all too often willing to grasp at unrealistic promises of a better life [i.e. the next life]...and sometimes, by focusing on a life to come, we miss what we have in this life.'

This outlook will be familiar to the reader of John Gray's intriguing and unsettling book *Straw Dogs* (a flawed but nonetheless extraordinary book). It's not obvious that these observations are pertinent to the title of the book, but I think they are very important lessons nonetheless.

The book, as a whole, is good but not exceptional. It strays from its title at times, and parts of it are overlong. Nevertheless, Shermer writes with authority and sincerity, and those parts of the book that 'stick to their brief' are very well written, provide many insights, and come highly recommended.

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