6º Simpósio da Fundação Bial

Aquém e Além do Cérebro

Behind and Beyond the Brain

Casa do Médico - Porto • 29 de Março a 1 de Abril de 2006

FUNDAÇÃO Bial

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O livro "Aquém e Além do Cérebro" contém as actas do 6º Simpósio da Fundação Bial, realizado na Casa do Médico, de 29 de Março a 1 de Abril de 2006, tendo como membros da Comissão Organizadora os Senhores Professores Nuno Grande, Alexandre Castro-Caldas, Caroline Watt, Fernando Lopes da Silva, Mário Simões e Rui Mota Cardoso.
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SESSÃO DE ABERTURA
HOMENAGEM A ROBERT MORRIS
OPENING SESSION AND
THE ROBERT MORRIS TRIBUTE

DISCURSO DO PRESIDENTE DA FUNDAÇÃO BIAL

Luís Portela

Senhor Bastonário da Ordem dos Médicos, Dr. Pedro Nunes, Senhora representante do Reitor da Universidade do Porto e Vice-Reitora da Universidade do Porto, Professora Isabel Azevedo, Senhor Presidente da Comissão Executiva da Agência Portuguesa para o Investimento, Dr. Costa Lima, Senhor Presidente da Comissão Organizadora deste Simpósio, Professor Nuno Grande, demais autoridades presentes, minhas Senhoras e meus Senhores.

"A Fundação Bial, espera contribuir para que o Homem se esclareça a si próprio como ser espiritual e como ser físico, de grande sensibilidade e de grandes capacidades de raciocínio e de realização, libertando-se de atrofiantes dependências de terceiros e de práticas místicas entorpecedoras.

A realidade existe. As coisas são como são, embora possam estar envolvidas pelo véu da ignorância. A todos aqueles que se dedicam a levantar as pontas desse véu, procurando descobrir a realidade, a Fundação Bial deseja os maiores sucessos."

Com estas palavras terminei a minha intervenção de abertura do nosso primeiro simpósio, em 29 de Março de 1996, faz hoje precisamente dez anos. O Prémio Bial - um dos maiores prémios europeus na área da Saúde - tinha sido criado em 1984 e esta Fundação em 1994, a que logo se seguiu o primeiro pacote de Bolsas de Investigação Científica, nas áreas da Psicofisiologia e da Parapsicologia.

Hoje - vinte e dois anos depois da nossa primeira iniciativa mecenática de cariz científico e dez anos depois do primeiro simpósio - penso que posso manifestar-lhes a minha satisfação pelo caminho percorrido por esta Fundação sem fins lucrativos, constituída e administrada em conjunto pelo Conselho de Reitores das Universidades Portuguesas e pelos Laboratórios Bial, e considerada de utilidade pública pelo Governo português.

O Prémio Bial tem vindo a distinguir alguns dos mais notáveis investigadores portugueses e mesmo alguns europeus. Vinte e cinco das

obras premiadas foram editadas em livro e distribuídas gratuitamente aos médicos portugueses e de alguns outros países.

O Júri do Prémio tem sido constituído por representantes dos Conselhos Científicos de cinco Escolas Médicas portuguesas, sendo a grande maioria das vezes dos seus mais notáveis membros. Funciona com total autonomia e é sempre presidido por uma personalidade convidada para o efeito pela Fundação Bial. O Senhor Prof. Manuel Sobrinho Simões dá-nos a honra de presidir ao Júri da edição deste ano.

As Bolsas de Investigação Bial, lançadas de dois em dois anos, apoiaram até agora seiscentos e quarenta e oito cientistas de vinte e um países diferentes, agrupados em duzentas e cinco equipas de trabalho. Poderemos dizer que cerca de metade das bolsas foram atribuídas na área da Psicofisiologia e as restantes na área da Parapsicologia.

A generalidade dos projectos apoiados tem o resumo dos seus resultados, provisórios ou definitivos, na nossa página da Internet, que pode ser consultada em alguns computadores disponíveis na galeria aqui ao lado. Também nessa galeria encontram-se trinta e quatro posters com resultados definitivos de projectos recentemente terminados e subsidiados por esta Fundação.

Dezoito desses projectos foram seleccionados pela Comissão Organizadora deste simpósio e serão apresentados nas sessões da tarde de quinta e de sexta-feira. Prestamos assim contas da actividade científica dos nossos bolseiros, realizando um dos propósitos deste simpósio.

Satisfeitos com o balanço das actividades desenvolvidas, entendemos procurar dar-lhes continuidade. Por isso tenho o gosto de informar que a Fundação Bial vai promover um novo pacote de bolsas, cobrindo as mesmas áreas e com características semelhantes às anteriores. O regulamento e os formulários para concurso estarão disponíveis, a partir da próxima segunda-feira, no nosso espaço www.bial.com e o prazo para entrega das candidaturas terminará em 31 de Agosto próximo. Cada projecto poderá ser realizado num período máximo de três anos e poderá beneficiar de um subsídio pecuniário entre 5.000 e 50.000 euros.

Outro objectivo deste simpósio é o de reunir a maioria dos nossos bolseiros, outros investigadores e alguns dos maiores nomes mundiais

nestas áreas, durante alguns dias, em agradável convívio e troca de ideias. Gostaria de lhes manifestar a nossa satisfação por termos podido reunir um tão valioso conjunto de palestrantes; e de agradecer a presença de todos.

O rico programa de que dispomos só foi possível graças ao entusiasmo e à dedicação da Comissão Organizadora, que aqui saliento, manifestando a nossa gratidão aos Senhores Professores Nuno Grande, Alexandre Castro-Caldas, Caroline Watt, Fernando Lopes da Silva, Mário Simões e Rui Mota Cardoso. Muito obrigado. Bem hajam! Os nossos agradecimentos também aos membros dos nossos órgãos sociais, aos membros do Júri do Prémio Bial, aos Conselhos Científicos das Escolas Médicas Portuguesas, ao Conselho de Reitores das Universidades Portuguesas, à Ordem dos Médicos e à Câmara Municipal do Porto, por toda a colaboração prestada.

Uma saudação especial ao Senhor Bastonário da Ordem dos Médicos que, apesar de ter um dos pés engessado, viajou de Lisboa de propósito para estar aqui connosco. Muito obrigado Dr. Pedro Nunes pelo seu tão amável gesto.

Finalmente, cabe-me lembrar o grande cientista, o grande homem e o grande amigo Professor Robert Morris, que infelizmente deixou o nosso convívio em 12 de Agosto de 2004, quando tinha 62 anos. Abro um espaço de homenagem à sua memória, que continuará com uma intervenção a ele exclusivamente dedicada, do Professor Richard Broughton.

Conheci o Professor Robert Morris, há dez anos, quando da realização do nosso primeiro simpósio, em que ele foi palestrante. Fiquei impressionado pela forma serena, rigorosa e simultaneamente apaixonada como fez a sua intervenção, intitulada *Recent developments in Experimental Parapsychology*. No contacto pessoal simples e fácil percebi rapidamente que a Fundação Bial poderia ter nele um excelente colaborador, o que de facto veio a acontecer. Mais do que isso, compreendi que estava perante um ser humano de uma dimensão extraordinária, o que o tempo me veio a confirmar amplamente.

O Professor Robert Morris ofereceu-se para colaborar connosco, tendo participado em todas as restantes Comissões Organizadoras dos nossos simpósios e sendo desde Abril de 1998 membro do nosso

Conselho Científico. Viajou frequentemente de Edimburgo ao Porto para participar em reuniões. Analisou e deu pareceres de muitos projectos concorrentes a bolsas. Aconselhou-nos em múltiplos aspectos. Recebeu-nos em Edimburgo, para melhor analisarmos e desenharmos os nossos apoios à Parapsicologia, área que estudou desde 1964 e na qual se tinha tornado um dos maiores especialistas mundiais.

Disponibilizou-se ainda para receber no seu departamento universitário algum ou alguns estudantes de doutoramento portugueses, o que veio a acontecer. Também se disponibilizou para fazer palestras e aconselhar instituições universitárias portuguesas que se pudessem interessar pela Parapsicologia. Lembro-me de ele ter estado em algumas das nossas Universidades, como por exemplo a Universidade do Minho e a Universidade Fernando Pessoa.

Orientou vários dos projectos de investigação por nós apoiados e ele próprio foi nosso bolseiro como investigador. Apresentou-nos muita gente da área das Neurociências, algumas das quais estão hoje aqui connosco. Foi efectivamente um muito valioso colaborador da Fundação Bial, o que sempre fez graciosamente.

No campo pessoal, fizemos todos nós na Fundação um grande amigo. Para mim, o Bob - como rapidamente me habituei a chamar-lhe - tornou-se um dos meus maiores amigos.

Dotado de superior inteligência, era um homem de uma sensibilidade invulgar e de um trato absolutamente distinto. Discreto na sua postura, tinha uma permanente preocupação de qualidade e de rigor naquilo que fazia.

Perspicaz e sereno, cultivava uma cuidada gestão dos recursos e uma elevada eficácia nos resultados. Mantinha uma permanente boa disposição, aliada a um impressionante entusiasmo com que se dedicava às suas tarefas e, por vezes, mesmo às tarefas dos outros.

Lembro-me da forma delicada como se tentava expressar a quem não sabia inglês. Recordo as suas gargalhadas sonoras quando conseguia compreender as anedotas que o Rui Mota Cardoso ou o Mário Simões por vezes nos contavam durante as refeições feitas em conjunto. Nunca mais poderei esquecer a forma forte como me abraçava quando nos encontrávamos, com a ternura de um verdadeiro amigo.

Admirei profundamente o Professor Robert Morris como cientista. Mas fascinou-me ainda mais a sua dimensão espiritual. Em toda a minha experiência terrena terei conhecido poucos, muito poucos seres da sua dimensão. Fascinava-me a sua perspectiva existencial. Fascinava-me a sua postura harmoniosa, tolerante, construtiva, incansável e sempre, sempre muito simples.

A Fundação Bial, muito grata por ter tido uma tão elevada colaboração do Professor Robert Morris e tendo tomado conhecimento de algumas dificuldades financeiras do seu departamento, após a sua passagem, entendeu - a título absolutamente excepcional - apoiar financeiramente durante os próximos três anos a manutenção da cadeira de Parapsicologia da *Koestler Parapsychology Unit* da Universidade de Edimburgo. Fazemo-lo em homenagem à obra científica de Robert Morris e de todos os que trabalharam com ele, pretendendo, assim, apoiar aqueles que estão a procurar dar continuidade ao excelente trabalho realizado, que fez da Universidade de Edimburgo uma das principais referências a nível mundial da Parapsicologia de carácter estritamente científico. Por nossa sugestão, esta cadeira passa a chamar-se *Robert Morris Chair in Parapsychology*.

Bob, expresso-me em nome do Conselho de Administração da Fundação Bial: no do Nuno Grande, no do Daniel Bessa e no meu próprio, mas também no do nosso querido Manuel Duarte Baganha, igualmente já partido da nossa convivência física, mas sempre teu admirador e amigo.

Obrigado Bob pela radiosa simplicidade com que soubeste iluminar o teu e o nosso caminho na tua passagem pela Terra.

Obrigado Bob pela forma serena e amiga com que sempre soubeste relacionar-te com todos e com cada um de nós.

Obrigado Bob pela inocência com que sempre soubeste mostrar-nos a pureza das tuas intenções, como só os Mestres sabem fazer; mais do que mestres do conhecimento, Mestres de Vida. Foste, de facto, para muitos de nós, um verdadeiro Mestre.

Muito e muito obrigado por tudo e até sempre, Bob.

SPEECH OF THE CHAIRMAN OF THE BIAL FOUNDATION

Luís Portela

Mr. Chairman of the Portuguese Medical Association, Dr. Pedro Nunes, Ms. Representative of the Rector of Oporto University and Vice-Rector of the Oporto University, Professor Isabel Azevedo, Mr. President of the Executive Committee of the Invest in Portugal Agency, Dr. Costa Lima, Mr. President of the Organizing Committee of this Symposium, Professor Nuno Grande, other bodies present, ladies and gentlemen.

"The Bial Foundation hopes to contribute to man emerging as a spiritual being and as a physical being, with great sensitivity and a great capacity for reasoning and realization, freeing himself from an atrophying dependence on others and numbing mystical practices.

Reality exists. Things are as they are, although they may be enveloped in a veil of ignorance. The Bial Foundation wishes the greatest success to all those dedicated to lifting the edge of that veil, seeking to discover reality."

With these words I ended my introduction to the opening of our first symposium, on March 29, 1996, exactly ten years ago. The Bial Award - an important European award in the field of health - was created in 1984 and then this Foundation in 1994, soon to be followed by the first package of Scientific Research Grants in the fields of psychophysiology and parapsychology.

Today - twenty two years after our first venture into scientific's sponsorship and ten years after the first symposium - I think I can tell you of my satisfaction with the route that has been followed by this non profit-making Foundation, established and administered jointly by the Council of Rectors of the Portuguese Universities and the Bial Laboratories and classified as a public utility by the Portuguese government.

The Bial Award has distinguished some of Portugal's and Europe's most eminent researchers. Twenty five prize-winning projects have been published and distributed free of charge to doctors in Portugal and other countries.

The award panel is made up of representatives of the scientific boards of five Portuguese medical schools, on the vast majority of occasions comprising their most eminent members. The panel operates with total autonomy and is always chaired by a leading figure in the field, invited by the Bial Foundation. Prof. Manuel Sobrinho Simões is giving us the honour of chairing the panel for this year's award.

The Bial Research Grants, awarded every two years, to date have supported six hundred and forty eight scientists from twenty one different countries, grouped in two hundred and five work teams. We can report that around half the grants were awarded in the area of psychophysiology and the rest in parapsychology.

For most of the projects supported, their results - whether provisional or final - are summarized on our Internet site which can be looked up on the computers available in the gallery outside. Also in the gallery are thirty-four posters showing the final results of recently completed projects supported by this Foundation.

Eighteen of these projects have been selected by the Organizing Committee of this symposium and will be presented during the afternoon sessions on Thursday and Friday. We will therefore be giving an account of the scientific activities of the recipients of our grants - one of the purposes of this symposium.

Satisfied with the balance of the activities that have been taking place, we intend that they should continue. I am therefore delighted to announce that the Bial Foundation will be promoting a new package of grants, covering the same areas and with features similar to the previous ones. From next Monday, the regulations and forms for the competition will be available on-line at www.bial.com and the deadline for submitting applications will be August 31. Each project must be completed within a maximum period of three years and may benefit from a grant of between EUR 5.000 and 50.000.

Another aim of this symposium is to bring together for a few days, the majority of our Fellows, other researchers as well as some of the best-known names in these fields worldwide, to enjoy each other's company and exchange ideas. I would like to say how pleased we are to have been able to bring together such a valuable group of speakers and thank everyone for coming.

The splendid programme that we have planned has been possible only thanks to the dedication of the Organizing Committee and in particular, I would like to express our gratitude to Professors Nuno Grande, Alexandre Castro-Caldas, Caroline Watt, Fernando Lopes da Silva, Mário Simões and Rui Mota Cardoso. Thank you very much for your efforts. Well done! Our thanks also go to the members of our executive bodies, the members of the award panel, the scientific boards of the Portuguese medical schools, the Council of Rectors of the Portuguese Universities, the Portuguese Medical Association and the Porto City Council for all the assistance they have given.

My special greetings to the President of the Portuguese Medical Association who, although having a fractured foot, travelled purposely from Lisbon to be with us tonight. Thank you so much Dr. Pedro Nunes for your kind gesture.

Finally, it falls to me to remember the great scientist, the great man and our great friend, Professor Robert Morris, who sadly left us on August 12, 2004, at the age of 62. I would like to begin this tribute to his memory, which will continue with a speech dedicated exclusively to him, by Professor Richard Broughton.

I met Robert Morris ten years ago when we held our first symposium at which he was a speaker. I was impressed by the calm, uncompromising and simultaneously impassioned way in which he delivered his speech, entitled *Recent Developments in Experimental Parapsychology*. From his unaffected and easy-going manner I soon realized that in him the Bial Foundation could have an excellent supporter - which is in fact what happened. More than that, I became aware that I was in the presence of a human being of an extraordinary dimension, which in time was fully confirmed.

Professor Morris offered to work with us, participating in all subsequent Organizing Committees for our symposia and, in April 1998, he became a member of our Scientific Board. He frequently travelled from Edinburgh to Porto to take part in meetings. He analysed and gave his opinion on many projects competing for grants. He advised on many matters. He welcomed us to Edinburgh, enabling us to examine and formulate our support for parapsychology, an area he had been

studying since 1964 and in which he had become one of the world's leading authorities.

He also found time to welcome Portuguese postgraduate PhD students to his university department. In addition he made himself available to give talks and advise Portuguese university institutions showing interest in parapsychology. I remember him visiting some of our universities, such as the University of Minho and the Fernando Pessoa University.

He supervised several research projects funded by us and he was also awarded some grants as a researcher. He introduced us to many people in the field of neuroscience, some of whom are with us here today. He was in fact, an extremely valuable contributor to the Bial Foundation and always gave his time most graciously.

On the personal point, he was a great friend to all of us at the Foundation. Bob - as I soon came to call him - became one of my best friends.

Endowed with superior intelligence, he was a man of unusual sensitivity and a manner that was highly distinguished. The soul of discretion, he always paid great attention to quality and precision in relation to all that he did.

Astute and unruffled, he cultivated the careful management of resources and a high level of efficacy in terms of results. He was blessed with a sunny disposition combined with an impressive enthusiasm with which he devoted himself to his own work and sometimes even to the work of others.

I remember the polite way he tried to express himself to those who could not speak English. I remember his loud guffaws when he managed to understand jokes told by Rui Mota Cardoso or Mário Simões during the meals we shared together. I shall never forget the bear-hug whenever we met, given with the tenderness of a true friend.

I deeply admired Robert Morris as a scientist but what fascinated me even more was his spiritual dimension. In my entire earthly experience I have known very few with the extent of his dimension. His existential perspective also fascinated me as well as his captivating harmonious attitude - tolerant, constructive, untiring and always, always very unaffected.

Out of gratitude for having benefited from such a high level of cooperation with Robert Morris and being aware of financial difficulties experienced by his department after his death, the Bial Foundation - as an exception - has agreed to provide financial support over the next three years for the Chair of Parapsychology at the Koestler Parapsychology Unit at the University of Edinburgh. We are doing this as a tribute to the scientific work of Robert Morris and all those who worked with him, with the aim of continuing the excellent work carried out which has made the University of Edinburgh one of the main points of reference for parapsychology in a strictly scientific sense. At our suggestion, this chair is to be called the Robert Morris Chair in Parapsychology.

Bob, I speak on behalf of the Board of Directors of the Bial Foundation, Nuno Grande, Daniel Bessa and myself, and for our dear Manuel Duarte Baganha, who as you left us in a physical sense and was always your friend and admirer.

Thank you Bob for the glorious simplicity with which you illuminated your and our paths during your journey through earth.

Thank you Bob for the serene and friendly way in which you were able to relate to each and every one of us.

Thank you Bob for the innocence with which you were able to show us the purity of your intentions, as only true Masters know how; more than masters of knowledge, Masters of life. For many of us, you were in fact, a true Master.

Thank you so much, Bob for everything and for always.

REMEMBERING BOB: THOUGHTS ON THE LIFE AND LEGACY OF PROFESSOR ROBERT L. MORRIS

Richard Broughton*

On August 19th 2004, the day of Bob Morris' funeral, the flags on the buildings of the University of Edinburgh throughout the city flew at half-mast, and the first person to be introduced to speak at his funeral was none other than the Principal of the University, Professor Timothy O'Shea. Such was the esteem in which he was held by his academic colleagues and the institution that he served for nearly two decades. How was it that this man from a small town in Pennsylvania, who spent his career championing a field of inquiry not widely respected by mainstream academia, would be mourned in such a fashion by Scotland's most prestigious university? Of course, Bob was mourned not just by the university and his family, but by countless friends throughout the world.

Bob was born in 1942 in a small town in the coal mining and steel making region of western Pennsylvania. As an only child until age 17 he was raised amongst the extended families of both parents, which seems to have helped develop his well known characteristic of always being willing to listen to both sides of an argument, and his abilities as a peacemaker. His interest in parapsychology emerged while he was still quite young. Apparently his parents had an interest in J.B Rhine's research on extrasensory perception and even had a game for testing ESP, which Bob discovered in a closet. The family dentist is also thought to have given Bob a home-made device for testing psychokinesis. Perhaps most important was that his parents thought that science was the way to understand these strange phenomena, and they conveyed that belief to Bob.

Bob did his undergraduate study in psychology at the nearby University of Pittsburgh. While he was there, he assisted with

* University of Northampton, Northampton, UK.

psychokinesis experiments that physicist and parapsychologist Robert McConnell was conducting, and he also began corresponding with the famous J.B. Rhine at Duke University. This led to Bob deciding to go to Duke University in Durham, North Carolina, for graduate school. His intention was to get a thorough grounding in scientific method and at the same time he would be able to see what Rhine's team was doing in the Parapsychology Laboratory. In the former, he acquitted himself very well, earning a PhD in 1969 for his dissertation on pair bonding in ring doves, which he always described as "how birds kiss."

It was the parapsychology, however, that won Bob's heart and his mind. The mid-1960s was an exciting time at the Duke University Parapsychology Laboratory. After some years of relative quiet, a new generation of bright and enthusiastic young researchers had gathered around Rhine, spending summers at the parapsychology lab while they pursued their graduate study. At the same time, Rhine was facing retirement from Duke, and he had established a new private research foundation so he could carry on leading the research effort. Bob joined this group, which included Jim Carpenter, Rex Stanford, John Palmer, and Chuck Honorton. They did not know it at the time, but these "youngsters" would go on to shape the field of parapsychology to this day. What drove them was the excitement of building a field of science that seemed about to make great leaps forward. They worked hard, played hard, nurtured ideas and nurtured friendships that were to endure.

But the latter half of the 1960s was also a time of rebelliousness, intellectual and social. Rhine's new generation of researchers were finding themselves drawn to the latest findings and research techniques emerging in psychology, as psychology itself emerged from the straightjacket of behaviourism. Rhine, however, was inclined to think that the card-guessing methods that he had established decades earlier were still to good enough to be the method of choice in his lab. The intellectual tensions that developed, and other factors, eventually led to a rupture as Rhine's new generation, Bob included, walked out on him.

As the rebels went off in different directions, Bob began his formal career in parapsychology at William Roll's Psychical Research Foundation, which was also in Durham at the time. As Research

Coordinator he explored the abilities of exceptional individuals and carried on research using animals as possible detectors of psychic influence. In some of the papers that he published in those days he began considering psychic ability as fundamentally a form of communication that might be found in other species in addition to humans. This effort to embed psi in the wider context of human and possibly animal behaviour was to be a theme throughout his career.

From Durham, in 1974, Bob, now with his wife Joanna, moved to California where he took up positions in the University of California, first in Santa Barbara and later, in 1978, in the School of Social Sciences on the Irvine campus. There Bob was very much the teacher, covering a wide range of psychology topics in addition to his main interest in parapsychology. Two themes emerged in this period. He became increasingly convinced that parapsychology research as a discipline had to be integrated into the wider science of human behaviour, and he began to see the dangers of uncritical acceptance of parapsychological and other anomalous claims. Whether this was due to the "laid back" life style of California, or the zeitgeist in those days, students seemed much too ready to *believe* rather than to *examine*. Bob found he needed to devote more of his time to teaching the need for rigorous testing of claims of psychic ability, and for critical examination of experimental evidence.

In 1980 Bob and his wife, and now with their twin daughters Lila and Vanessa, moved to Syracuse, New York, where Bob joined Syracuse University as a Senior Research Scientist in, of all places, the School of Computer and Information Science. But it was a good fit, as Bob taught courses on man-machine interactions, both of the normal kind (human factors) and the paranormal kind. In his writings he developed more thoroughly his ideas of psychic ability as exceptional human performance in the domain of communication. With his students he explored innovative ways of testing psychic abilities through computer technology.

Bob's last move, of course, was to the University of Edinburgh. The author and philosopher Arthur Koestler died in 1983 and the bequest from him and his wife was to be used for the establishment of a chair of parapsychology at a British university. After several of Britain's

leading universities either declined the money or wished to impose unreasonable conditions, the chair was eventually established at the University of Edinburgh. This may have had something to do with the more adventurous spirit of the Scots, but it was most likely because Edinburgh's psychology department had already established a tradition of graduate research on parapsychological topics under John Beloff. Edinburgh welcomed the Koestler Chair and began a worldwide search to fill it. It was no surprise to anyone in the field when Bob Morris was offered the chair in 1985.

Thus began that chapter of Bob's life that most of us know best, and one of the most remarkable periods in the history of parapsychology. In accepting that post, Bob recognized that he was being presented with both an unparalleled opportunity to advance the science that he loved, but also an enormous challenge. As he was to confide to his friend Stephan Schwartz, his plan was to take the long view; to be patient, not to make waves, and to keep a low profile. He would "...try to *work* with, not *argue* with, the academic establishment..." He would "plant seeds" in the form of young graduates with an interest in an academic career who would go on to spread careful, scientific parapsychology to universities in the UK and Europe.[1] That is where he would invest his time and resources. He thought the process might take about 20 years. In the end he had 19, and he accomplished his goals superbly.

By the nature of the post, it was initially surrounded by considerable interest by both the scientific and the popular press, so keeping a low profile was not possible. Yet, Bob managed to keep a very modest profile. Toward his goal of working with the academic establishment Bob's efforts and natural talents paid handsome dividends. He almost never turned down an opportunity to speak at an academic gathering if he could fit it in to his busy schedule. After only a few years in the post, Bob told me that he had spoken at nearly every university in Great Britain, sometimes more than once. His unassuming manner, his often

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[1] Stephan Schwartz kindly shared his memories of Bob's plans (aided by contemporaneous notes) with colleagues on an electronic discussion list and they were subsequently quoted in James Carpenter's obituary for Bob Morris (Carpenter, 2004).

self-deprecating humour, and most importantly his willingness to engage in truly constructive dialog frequently turned critics into supporters-not believers, but allies in the quest for a scientific understanding of the phenomena of parapsychology. For many of his academic colleagues it was Bob's caution and relentless empiricism toward the phenomena that won the day, for in Bob it was genuine. It was not a pose for the audience. The degree to which he succeeded in working with the establishment was demonstrated when he was elected to serve as the President of the Psychology Section of the British Association for the Advancement of Science.

At home, in Edinburgh, Bob quickly became a key member of the large psychology department and the school of which it was part. In addition to his large PhD supervision load, he took on administrative and teaching responsibilities far beyond what would be expected of a chair holder. The way in which he managed the Koestler Parapsychology Unit – his team of researchers and graduate students – brought a freshness and vigour to the entire department. One member of the department remarked to Jim Carpenter that Bob had made the department "an enormously friendlier and healthier place."

Of course, Bob was active within his professional field as well, publishing numerous papers and lecturing widely. The concerns that first manifest in California were addressed by a range of papers and co-authored books that examined the issue of "what looks like psi, but isn't" and the requirements of effective testing of so-called "psychic claimants" (people who claim to have psychic abilities). He wrote on the psychology of deception, volitional competence and performance enhancement. He served on the Board of the Parapsychological Association, and on the Council of the Society for Psychical research, and his colleagues recognized his achievements in several contexts. He was awarded the prestigious Frederic Myers Medal by the Society for Psychical Research and the Outstanding Contribution Award from the Parapsychological Association. From the British Psychological Society Bob received the C S Myers Award.

Undoubtedly Bob's most important achievement was in the sowing of seeds. He supervised to completion 32 PhD students who did research in parapsychology. Bob's students didn't work solely in

parapsychology. Recall his conviction that parapsychological research had to be embedded in the wider field of psychology. Bob insisted that his graduate students formulate their research projects in ways that spoke to issues in psychology as well as parapsychology. Twelve of his students now have permanent positions in universities, some having risen to senior positions, and all have brought a healthy, academic interest in parapsychology to their institutions. In turn, Bob's students have supervised their own PhD students, so that the Bob Morris family tree of graduates is growing wide indeed. Moreover, a culture not just of tolerance for parapsychology, but genuine respect for that science as he embodied it has spread across the country.

All in all, Bob's was a most remarkable career forged in a field that does not make it easy to have a career. But I do not think we would be commemorating our friend and colleague so long after his death if it were just his professional career that was special. It was really Bob, the man, who was also special. It was a unique personality that enabled him to enter a frustrating and often maligned field of science and turn its difficulties into assets and its shortcomings into surmountable challenges. What was it about Bob that his friends and close associates remember?

Bob was an educator in the basic sense of the word, to "lead out". All who spent time in conversation with him benefited from his patient questioning and probing that enabled one to "lead out" and develop one's own ideas. His graduate students saw him as the ideal supervisor. He was amongst parapsychology's best conceptual analysts and when one shared ideas with him they always came back sharper and more focussed, and he let you think that you had done all the work. He was genuinely interested in what other people had to say, and one always knew he was really listening. Countless professional colleagues have cherished memories of long evenings in Bob and Joanna's home, or in hotel rooms at conventions, where conversations ranged long into the night on parapsychological issues great and small, and when they finally ended one felt stimulated and invigorated by his interest in your ideas and the expanded possibilities that usually emerged.

Bob was an extremely generous person-generous with his time, his home life, his expertise, just about anything he could share. Bob and

Joanna's home was always open to friends and visitors, and there were many. Frequently Bob and Joanna provided accommodation for impecunious graduate students in their spare flat. Bob spent countless hours on committees, and boards for various organizations, and on official visits to organizations and universities from one end of the world to the other. These were not things he had to do for his job, or to enhance his own career. He did this to advance parapsychology, and to help his friends advance the science wherever they were. One of his colleagues in Edinburgh and the former Head of the psychology department, Peter Caryl, recalled a time some years ago when the department found itself with a major gap in the teaching provision for final year students. Bob, who was already teaching two final year options, offered to develop a new option in sport psychology, yet another interest of his. He started it from scratch and it drew a large class. He continued to teach it until Edinburgh brought a sport psychology specialist on board.[2] Just about anyone who ever worked with Bob can recount similar incidents of Bob's willingness to help out wherever and whenever he could.

No remembrance of Bob Morris can be complete with reference to his sense of humour. Variously described as nutty, bizarre, surreal, it was never far below the surface and could burst forth at the most unexpected moments. This is one aspect of Bob's personality that is impossible to capture for those who did not know him, but for those who did, I suspect at this moment they are quietly thinking of some favourite encounter with that side of Bob. He used his sense of humour to great effect to disarm potentially hostile audiences, or capture the attention of students, and he let it loose to entertain his friends, but most of the time I think he simply just couldn't help himself. He just had a knack for seeing life's incongruities and drawing attention to them. I won't attempt to recreate any example, but I can describe the effect. About 10 years ago I had the honour of presenting him with the Parapsychological Association's Outstanding Contribution Award. This was after a banquet, and I struggled to keep everyone's attention as I introduced the award. Bob joined me at the podium and turned to the

[2] Recalled by Dr. Caryl in his tribute to Bob at the funeral.

audience to say a few words of acceptance. Within a sentence or two he had the entire crowd convulsing with laughter, and after about 2 or 3 minutes tears were streaming down faces and people were gasping to catch their breaths between fits of laughter. And the whole time he never so much as cracked a smile, until he turned and gave me a wink, and walked back to his seat, leaving me to carry on, except that I couldn't. I was literally breathless. For what seemed like ages, I stood at the podium trying to start a sentence, only to break up laughing again at the thought of something Bob had just said. Years after that people still recall that incident, and not one of us can remember a word that Bob had said, just that it was insanely funny.

We also should remember Bob's family at this time. To lose a father and a husband so prematurely is tragic, and I need not belabour that point. What I can say is that Bob and Joanna's twin daughters are doing well. Vanessa is confronting the challenges of being a struggling artist in the animation field while Lila is traipsing the fields and forests of Great Britain as a young ecologist. Joanna continues as the administrative anchor of Edinburgh University's Management School. A few weeks ago my wife and I visited Joanna in Edinburgh, and the conversation turned to how she was coping with Bob's absence. That she is doing as well as can be expected, but one comment summed up something about Bob that we all knew to be true. She said that, in a way, she always felt that Bob didn't just belong to her, but he really belonged to everyone.

Finally, I must make a little confession. For most of the time I have been talking about, I really did not know Bob other than as a professional colleague who was always a delight to see at a convention. It was not until the summer of 1985, when I had invited Bob to come down to Durham to spend a few days at the Rhine Institute that I really began to know him as a friend. The Koestler Chair had just been announced. Bob and his family were over at our house one evening, and I was shocked to discover that Bob wasn't planning to apply for the chair. He explained that he didn't think that he and Joanna would like living overseas, and wasn't the weather pretty awful in Edinburgh, and it was just a professor post (which in the American system is just another rung on the academic ladder).

There began a long discussion in which I impressed upon Bob the importance of a named chair in the British system-and the particular importance of this chair for parapsychology. And I spoke of how my wife and I enjoyed our life in Edinburgh some years earlier, during my postgraduate days. I assured him it did not really rain every day (which may have been stretching the truth a bit). It was the same sort of conversation that I later came to expect from Bob, probing, questioning, and always supportive, because one of the issues was that we would be competing for that post. But we both had no doubt that the chair required the best person our field could put forward, and I knew that was Bob. Later that evening, as we stood in the driveway with the night-time cacophony of North Carolina insects all about us, Bob assured me he would "give it some thought."

As they say, the rest is history. Bob Morris, once reluctant to move out of the States, went on to become perhaps the most international of any of us in the field. He went on nurture students and colleagues not only in Edinburgh and Britain, but in all the places that he visited. His breadth of knowledge about the field, his fairness and his generosity made him the confidant and consultant to some of the most influential individuals and organizations in the world for our field. The Bial Foundation is one of those, of course, and he was enormously pleased that the Bial Foundation Symposia represented the merging of mainstream neuroscience and psychology with parapsychology that he saw as essential for parapsychology to progress. Others included the Institute for Border Areas of Psychology and Mental Health in Germany, the Rhine Research Center in the United States and organizations in Scandinavia, Japan, in the Carribean and in South America. Bob was welcomed as the representative of parapsychology in universities around the world. Bob Morris, once reluctant to leave the home shores, had become parapsychology's ambassador to the world.

It may come as a surprise to some, given the field that Bob laboured in, that he did not have a belief in an afterlife. He had no expectation of living on in some disembodied existence. Instead, I think he expected to live on in the science that he led, and in the hearts and minds of the students, colleagues and fellow scientists whom he touched. Tonight we commemorate Bob Morris in words and in

memory, but those of us who continue to confront the challenges of parapsychology can best commemorate our friend by insuring that he does live on; that he lives on in the excellence of the science that we practice and in the quality of our dialog with fellow scientists.

Reference

Carpenter, J. C. (2004). Obituary: Robert L. Morris. *Journal of Parapsychology, 68*(2), 423-431.

DISCURSO DA REPRESENTANTE DO REITOR DA UNIVERSIDADE DO PORTO

Isabel Azevedo

Em nome do Reitor da Universidade do Porto agradeço o convite para estar nesta sessão e digo-vos do gosto que tenho, pessoalmente, em testemunhar este ambiente tão vivo de gente interessada em discutir, eventualmente avançar, no conhecimento de um dos fenómenos indiscutivelmente mais interessantes deste mundo, que é o pensamento.

Dou, portanto, as minhas felicitações, muito vivas, aos promotores desta organização, em especial ao Dr. Luís Portela, o Presidente da Fundação Bial. Não resisto a uma pequena nota pessoal, peço-lhe desculpa: como colaborador da Universidade do Porto e como meu ex-aluno - é sempre bom ver os ex-alunos serem bem sucedidos -, haveria muitas razões para cumprimentar e para me regozijar com os sucessos do Dr. Luís Portela, mas gostaria de salientar a sua esperteza, inteligência de perceber que para se fazer avançar um conhecimento em que se tem muito interesse, não há nada como garantir directamente os meios para que se possa fazer esse progresso e fazer essa investigação.

Desejo que esta reunião seja um grande sucesso, bem assim como as futuras realizações da Fundação e a própria Fundação.

DISCURSO DO BASTONÁRIO DA ORDEM DOS MÉDICOS

Pedro Nunes

Senhora Vice-Reitora da Universidade do Porto, Senhor Presidente da Comissão Executiva da Agência Portuguesa para o Investimento, Senhor Professor Nuno Grande, Senhor Dr. Luís Portela, individualidades presentes, minhas Senhoras e meus Senhores.

Agradeceu-me o Dr. Luís Portela ter-me deslocado apesar de um pé engessado. Não é um pé engessado que evita, a menos que se tenha de conduzir, estes pequenos quilómetros que separam Lisboa do Porto. É com muita honra e muito prazer que aqui estou e sinto, para além do mais, como minha obrigação aqui estar.

É a primeira vez que tenho oportunidade de colaborar neste Simpósio, apesar de já ter estado na entrega dos prémios Bial no ano passado. Mas, Dr. Luís Portela, a Fundação a que preside, e porque não dizê-lo, a empresa que subjaz a essa Fundação, são simbólicos para a Ordem dos Médicos, muito particularmente, de alguns aspectos são simbólicos de como um médico pode, ao leme de uma indústria farmacêutica, congregar pessoas de todas as formações: da Matemática à Engenharia, à Medicina, à Farmácia, e fazer uma empresa que, na inclemência do nosso clima social e político que nem sempre é viçoso para o crescimento de empreendimentos desta natureza, consegue atrever-se a investigar o cérebro, a produzir, a inovar e a premiar quem trabalha.

Ensina-nos muito. Ensina-nos que hoje a Medicina é um acto de colaboração. Que hoje a Medicina não se exerce se não for exercida neste contexto grupal, de todos os saberes e de todas as ciências. Que a Medicina não existe se não investigar e não melhorar, e portanto, como a Ordem dos Médicos, não faço mais do que a minha obrigação em estar presente. Agradeço-lhe o que tem feito pelos médicos, pela Medicina e pelo país.

As minhas palavras, como calcularão, não serão mais do que de circunstância, neste sentido de que não vos farei perder muito tempo.

Só me resta agradecer e envolvê-los a todos, quaisquer que sejam as vossas formação, licenciatura e saberes, neste abraço dos médicos que vos dirige como colegas e que vos deseja as maiores felicidades neste simpósio que hoje começa. Muito obrigado.

PALESTRAS
LECTURES

INTRODUÇÃO À CONFERÊNCIA INAUGURAL

Prof. Doutor Nuno Grande

É para mim, um privilégio apresentar o eminente professor e cientista Howard Eichenbaum que irá proferir a conferência inaugural do sexto Simpósio "Aquém e Além do Cérebro" promovido pela Fundação Bial.

Esta conferência denominada "The Neurobiology of Recollection" será a tradução de uma vida dedicada ao ensino e à investigação dos mecanismos relacionados com a memória.

O Prof. Howard Eichenbaum é Professor do Departamento de Psicologia da Universidade de Boston onde dirige *The Center for Memory and Brain*.

Tem dirigido e participado em cursos de graduação e pós-graduação em neurociências, comportamento animal, aprendizagem e memória, bases anatómicas e fisiológicas da cognição e da fisiologia sensorial.

É director do programa em Neurociências da Universidade de Boston que iniciou em 1969 após a licenciatura em Biologia Celular na Universidade de Michingan, onde se doutorou em Psicologia em Maio de 1975.

De 1969 a 1971 investigou no coelho o condicionamento do sistema nervoso central pelos estimulantes.

Em 1971 iniciou o estudo do comportamento socializante em aves prematuras.

Em 1973 estudou o papel do colículo superior na orientação visual em macacos.

De 1973 a 1975 estudou a função do lobo frontal nos processos inibitórios em coelhos.

Iniciou o estudo da memória e percepção visual em macacos e das bases químicas e anatómicas da memória no rato.

Desde 1977 até ao presente desenvolve processos de registo incluindo radiometria de multicanais de grupos de microeléctrodos.

Tem feito elevado processamento da informação olfactiva, o estudo do comportamento e da plasticidade electrofisiológica, da representação da memória no hipotálamo e no córtex cerebral.

Não surpreende que seja o editor chefe da revista *Hypocampus* e membro do conselho editorial de várias outras publicações de grande exigência editorial

MEMORY AND THE BRAIN

Meeting Report

Howard Eichenbaum[1], Menno Witter[2], John Aggleton[3], Guilén Fernandez[4], Alcino J. Silva[5], Fernando Lopes da Silva[6]

What are the cerebral processes responsible for memory functions? This multi-disciplinary Meeting was attended by about 350 scientists and students from all over the world to discuss the Mind-Brain problem following diverse perspectives. The Symposium focused on novel experimental approaches to study memory in its several dimensions. This is a timely issue, since advances both in animal experimentation, using tools derived from molecular biology, and human studies, using novel techniques of functional brain imaging, are leading to new concepts of how memories are formed and organized in the brain. Furthermore, strategies are emerging that may allow the repair of memory deficits in patients, until recently a most unlikely proposition.

In his opening key-note lecture *Howard Eichenbaum* from the Center for Memory and Brain, Boston University (USA), talked in an entertaining and scholarly way about the neurobiology of recollection. In his talk he focused on three essential features of recollection, namely that I) items are remembered in context of experience, II) vivid memories contain a flow of events, and III) the fact that memories are interleaved by common elements. These three characteristics are

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2 VU University Medical Center, Amsterdam, The Netherlands.
3 School of Psychology, University of Wales, Cardiff, UK.
4 F.C. Donders Center for Cognitive Neuroimaging, Radboud University, Nijmegen, The Netherlands.
5 Department of Neurobiology, Psychiatry and Psychology, University of California, Los Angeles, USA.
6 University of Amsterdam, The Netherlands.

obvious in human recollection, but in order to understand the underlying networks in the brain and the processes involved we need to have access to relevant animal models. Therefore, a critical question is whether animals show recollection. This is why in his lecture he talked mainly about the rat.

After introducing the role of the human hippocampus in recollection and some of the historical arguments to focus on the hippocampus, he reviewed a number of animal studies relevant to the three essential requirements of recollection as put forward. In a first series of experiments addressing the issue that events are represented as items in a context of experience, he introduced the difference between *familiarity* and *recollection* showing that signal detection analysis is a powerful approach to differentiate between the two phenomena (Yonelinas 2001). He showed that rats, like humans, have the capacity for both familiarity and recollection by summarizing experiments in which animals were trained to discriminate between odors related to rewards. Moreover, similar to humans, rats with complete bilateral hippocampal lesions only depend on familiarity to solve the task, thus showing that *recollection* in rats, similar to the situation in humans, *depends* on an intact *hippocampus* (Fortin et al., 2004). He also made the point that in aged humans and rats, in whom the hippocampus is likely less functional, a clear shift occurs such that familiarity becomes the more relevant process used (Daselaar et al., 2006; Eichenbaum own results). In a subsequent series of experiments the question was addressed what information is represented by hippocampal neurons. He introduced a task in which odors were associated with different places and the rat had to be able to discriminate between new or familiar combinations of odor and place (Wood et al., 1999). During the behavior recordings were made in the hippocampus and although about equal numbers of cells in the hippocampus were coding for odor, place or match, the majority of neurons coded for odors in context.

In the second part of the talk, Howard Eichenbaum focused on the representation of the flow of events in particular episodes. Again using experimental data in rats, taken from a study by Wood et al (2000) he argued that rats do remember sequences. In this particular experiment, using so-called T-maze alteration, rats were shown to be able to

remember the previous trial to guide their behavior on the next one. And as illustrated in Figure 1, in a delayed version of this paradigm, the level of firing of single hippocampal neurons predicted whether the rat would show the correct choice or would make an error.

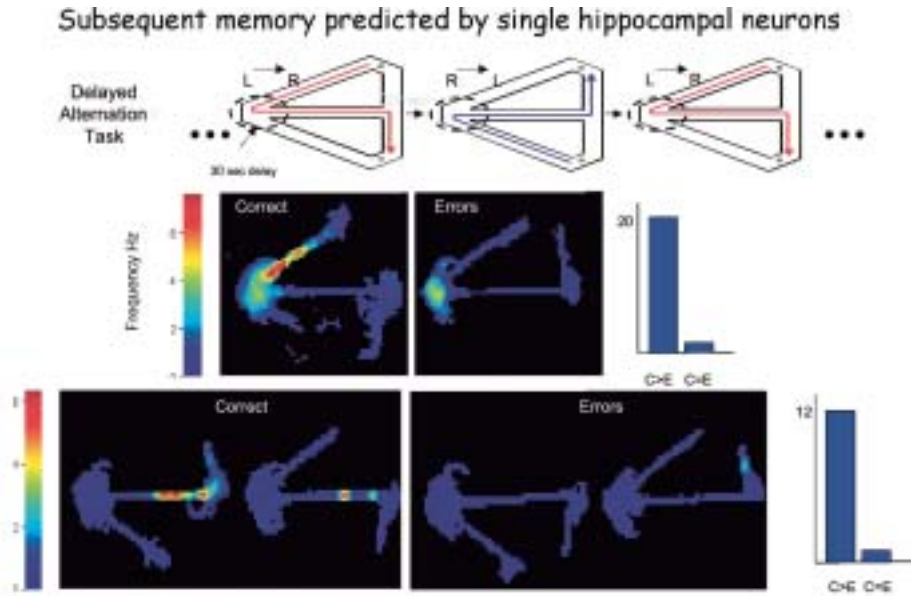

Figure 1 - The activity patterns of hippocampal neurons in a rat performing a spatial memory task. In this task rats can take either of two routes though a maze. During each route, the rat is restrained for 30 sec at the outset of the ambiguous segment of the maze, during which they must remember the preceding route and after which they must take the opposite route to obtain a reward. The top panel shows the routes animals take through the maze on alternating left-to-right (L->R in red) and right-to-left (R->L in blue) trials and the place where they are restrained (dashed oval line). The middle panels show intense activity (red and yellow colors) of one neuron just before the rat is restrained (blue color indicates the route). This activity is present just before the animal sustains the delay and predicts correct choices (left panel); there is no activity when the animal makes an error (right panel). The bottom panels show intense activity of another hippocampal neuron that fires in the midst of the right-to-left route, but not on the left-to right route, when the animal will make a correct choice (left panel); it does not fire on either route when the animal makes an error (right panel). These findings indicate that hippocampal neurons represent parts of the routes animals take in a memory task, and their activity predicts memory performance.

In the third part, he addressed the question whether rats can indeed solve a paradigm in which an associative inference is required, on the basis of overlapping paired associates. He presented data from a study published in 1996 showing that normal rats can indeed solve such a paradigm whereas animals with hippocampal lesions can learn and recall the premise pairs but cannot make the associative inference (Bunsey and Eichenbaum 1996).

In conclusion, rats do show all the three requirements for recollection and, as in humans, all three kinds of memory binding: remembering items in context, in sequence temporal organization and in associative inference (networking). All are supported by the hippocampus. Therefore the final question posed by Howard Eichenbaum was where in the hippocampus these types of binding may occur. On the basis of converging evidence from a large number of groups he proposes that *items in context and inference depend on CA3 whereas the sequence is coded in CA1 hippocampal sub-fields.*

With this opening lecture, Howard Eichebaum also provided the audience with a reference frame for the series of lectures of the following session on Neuroscience of Memory which was opened with the lecture of *Menno Witter* from the VU University medical center (VUmc) in Amsterdam, The Netherlands. In particular the suggestion put forward by Howard Eichenbaum that different parts of the hippocampus might play complementary, yet functionally unique roles in episodic memory, is strengthened by anatomical findings in rats and non-human primates. Indeed there are striking differences between, for example, the hippocampal sub-fields CA3 and CA1 with respect to their input-output connectivity. In his talk Menno Witter focused on the organization of the reciprocal connections between the entorhinal cortex and the hippocampus making the claim that there are at least two functionally different pathways that connect the hippocampus with the rest of the cortex, and that the entorhinal cortex is the pivot in these networks. In concordance with the ideas of Howard Eichenbaum, the entorhinal cortex can be subdivided with respect to its connections into a medial and a lateral subdivision, MEC and LEC respectively (Figure 2). *Whereas context or space information is most likely mediated through MEC, items or object information is most likely dealt with by LEC.* Convergence of these two pathways most likely takes place in the

dentate gyrus/CA3 system, which subsequently conveys a representation of convergent information to all neurons of CA1. However, selected groups of CA1 neurons receive either MEC input or LEC input as well. Anatomical as well as electrophysiological data support that single CA1 cells receive convergent input from both sources (Kajiwara et al., 2006; Otmakhova et al 2002; Arrigoni and Greene 2004).

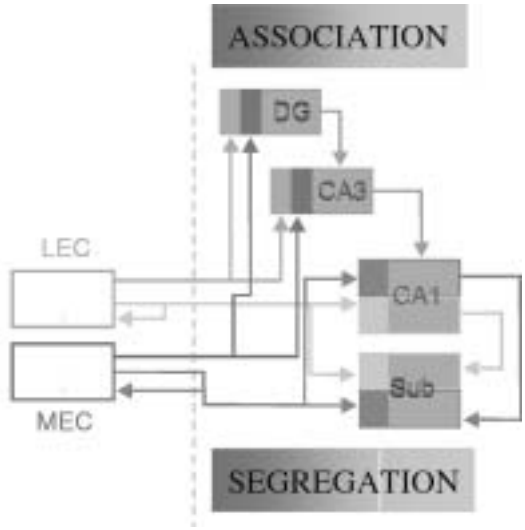

Figure 2 - Parallel pathways in the entorhinal-hippocampal circuits showing convergence and divergence in respectively dentate gyrus/CA3 and CA1/Subiculum (reproduced with permission from Witter et al., 2000).

The functional consequences of this anatomical configuration which implies that CA1 neurons receive convergent information were tested in disconnection experiments by which the effect of removing CA3 inputs on the spatial firing characteristics of CA1 neurons was investigated. On the basis of these data (Brun et al, 2002) it was concluded that the *CA3 input is needed to recollect items in context, whereas the spatial code in CA1 depends on the direct inputs from the entorhinal cortex*. This notion has been substantiated in a number of more recent papers (Steffenach et al., 2005; Brun et al., 2006).

An anatomical feature of the hippocampal system that is likely relevant regarding its role in memory functions, was put in evidence by Menno Witter, namely that the entorhinal-hippocampal circuits may function as reverberatory circuits. He presented anatomical as well as functional measures made in *in vitro* slices using voltage-sensitive dye imaging that support the notion that the connectivity and network characteristics of these circuits may be instrumental to realize fast, short term storage of information (Naber et al. 2001, van Haeften et al., 2003; Iijima et al., 1996).

With this view of cortico-entorhinal-hippocampal circuits as the relevant hardware in memory processes, the importance of the traditional notion that a main output flow of the hippocampus runs through the fornix to parts of the thalamus and hypothalamus must be taken into consideration. The subsequent talk of *John Aggleton*, from the School of Psychology, Cardiff University precisely focused on new and exciting findings that the fornix and related parts of the thalamus are vital for memory. With respect to the thalamic targets, the presentation focused on the anterior thalamic nuclei and the mammillary nuclei in primates and in rodents. After providing anatomical data to support this choice (Aggleton et al., 1986; 2005; Saunders et al., 2005) he made the point that in humans neurodegeneration in the anterior thalamic nuclei differentiates the amnesia of alcoholic Korsakoff syndrome from the closely related non-amnesic Wernicke's encephalopathy (Harding et al., 2000). Both groups, however, show marked atrophy in the mammillary bodies (Harding et al., 2000). Interestingly there is convincing evidence that *in cases with diencephalic amnesia the critical lesion disconnects the mammillary bodies from the anterior nuclei by inclusion of the so-called mammillothalamic tract* (van der Werf et al., 2000; Dusoir et al., 1990). The prediction would thus be that fornix lesions would lead to a dramatic memory problem. In a study of patients with colloid cysts (for example patient KN) in/adjacent to the fornix that have been surgically removed, it was possible to compare patients with intact and lesioned fornix. A striking feature of the patients with fornix lesions was that their memory function shows the ability to perform at *near normal levels on some tests of recognition*, despite their consistent *deficits on tests of recall* (Figure 3).

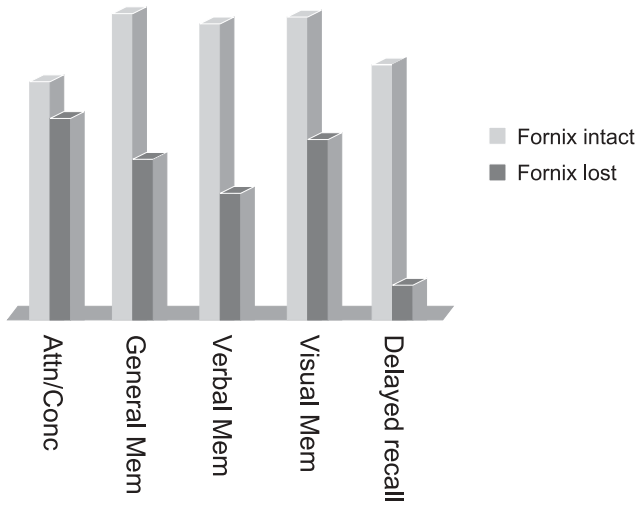

Figure 3 - Performance on the Weschsler Memory Scale -Revised by patients who have undergone surgery for removal of a colloid cyst. The fornix was broken bilaterally in three cases ('Fornix lost') but intact in seven cases ('Fornix intact'). Considerably lower scores result from fornix damage.

A larger study on such patients is currently being carried out. Interestingly John Aggleton scrutinized more precisely the concept of recognition memory. Tests were devised to distinguish between two kinds of recognition memory: the "remember procedure", i.e. the subject can recall an event that is fully recognized, and the "know procedure", i.e. the subject knows that event is familiar but cannot consciously remember anything more about the event. The patient KN displayed evidence for a *selective sparing of the familiarity component of recognition* (Aggleton et al., 2005. Aggleton & Brown 1999). The dissociation within recognition memory supports dual-process models of recognition, and also supports proposals that *anatomically linked regions within the medial temporal lobe make qualitatively different contributions to specific aspects of memory retrieval.*

Thus the extended hippocampal-diencephalic system is needed for learning and subsequent recall of new episodic information, but is not needed for the familiarity feature of recognition. Accordingly selective damage of this system can relatively spare recognition memory.

Furthermore he addressed the issue that the fornix is known to not only project to the mammillary bodies but also to thalamic structures and a number of other cortical and subcortical aeas such as prefrontal cortex, ventral striatum, cholinergic basal forebrain, and formulated the question of how can one determine whether the thalamic sites play also a critical role in memory. For this he turned to animal studies and summarized a number of lesion studies convincingly showing that the interaction between hippocampus and the anterior thalamus is essential for different types of behavior depending on learning spatial information (Warburton et al., 2001). In addition, he took his argument one step further by showing data on the effects of lesions in the anterior thalamic complex on immediate early gene expression (c-fos), a method to evaluate induced changes in cell activity. Significant changes in expression occurred in areas of medial prefrontal and cingulate cortex as well as in the hippocampus. From these experiments he concluded by arguing that the *extended-hippocampal system is the key player in episodic memory* in which not only the hippocampus and diencephalic structures but also the posterior part of the cingulate cortex, the retrosplenial cortex, play a role.

An important new dimension in studies of memory functions in human is being obtained by combining neuropsychology and functional neuroimaging methodologies. In this context the contribution of *Guillén Fernandez*, of the F. C. Donders Center for Cognitive Neuroimaging, Radboud University Nijmegen, The Netherlands, was particularly relevant. He showed two sets of data that enrich our concepts about the neurophysiology of the processes underlying the formation of declarative memories. The first set consists of the results of an exploration of declarative memory in epileptic patients with indwelling electrodes, used to detect the site of origin of epileptic seizures with the aim of removing this epileptogenic area by a surgical intervention. Using this methodology his group was able to record neural activity (local EEG signals) of the hippocampus and the rhinal cortex, brain structures of the Medial Temporal Lobe (MTL) that are part of the declarative memory system. The main question was how these structures encode new declarative memories (Fernández et al. 1999). They focused the analysis of these EEG signals on the gamma-frequency

range (32 - 48 Hz) since it has been shown that this gamma activity may mediate transient coupling of neural assemblies. They found successful *declarative memory formation to be accompanied by two factors: an early increase and a later decrease in gamma synchronization between rhinal and hippocampal recording sites*, and a transient reduction of gamma power at both locations partly within the same time window (Fell et al. 2001). This transient reduction of gamma oscillations might be explained by the necessity to suppress noise-like ambient gamma activity unrelated to specific study items. It may be speculated that in an event of unsuccessful encoding, ongoing background gamma activity interferes with item related activity and distorts the process of memory formation. Thus, reduced gamma power, as assessed in these studies during successful encoding, might be a correlate of a higher specificity of local assembly activation. These findings support the concept that *formation of new declarative memories requires a direct cooperation between rhinal cortex and hippocampus.*

In a second set of studies Guillén Fernandez and collaborators analyzed the neural correlates of memory recognition, dissociating familiarity from recollection and memory consolidation in normal subjects using event-related fMRI (Weis et al. 2004a,b).

In this way subjects were scanned while they memorized real-world photographs and subsequently tried to recognize them within a series of new photographs. The results showed that declarative memory formation is correlated with activity in the medial temporal lobe (MTL) and inferior prefrontal cortex, as revealed by the subsequent memory effect, where responses are stronger to subsequently remembered than forgotten items. Regarding recognition memory the group showed that a strong correlation with activity in specific regions within the parietal lobe, anterior prefrontal cortex, anterior cingulate and cerebellum correlate as measured by the conventional old/new paradigm: the fMRI responses were stronger for recognized old items (hits) than for correctly identified new items (correct rejections). In order to make a finer analysis of the recognition process, Guillén Fernandez and collaborators introduced two recognition tasks by comparing fMRI responses to hits and to old items misclassified as new (misses). The positive recognition effect (hits > misses) revealed prefrontal, parietal

and cerebellar contributions to recognition, and in line with electrophysiological findings, the negative recognition effect (hits < misses) revealed an anterior medial temporal contribution.

The question that was also raised by John Aggleton's group of the dissociation between a precise recollection of events and a vague recollection (familiarity) in their studies of lesioned patients, was also approached using fMRI by the group of Guillén Fernandez. The latter employed a source memory task in an event related fMRI study to dissociate MTL processes associated with either contextual retrieval or item recognition. To introduce context, stimuli (photographs of buildings and natural landscapes) were presented in one of four single-color-scales: red, blue, yellow, or green. In the subsequent old/new recognition memory test, all stimuli were presented as gray scale photographs, and old-responses were followed by a four-alternative source judgment referring to the color (context) in which the stimulus was presented during the study. In this test, recognition of an item that is accompanied by contextual information is truly episodic memory, whereas recognition unaccompanied by contextual information can rely upon a sense of familiarity (Figure 4).

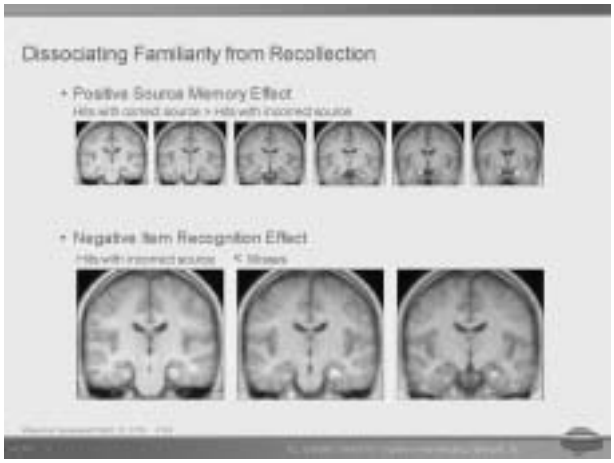

Figure 4 - Recognition Effects. Upper row: Regions activated more for hits as opposed to misses (positive recognition effect). Lower row: Regions activated less for hits as opposed to misses (negative recognition effect). Activation maps are shown superimposed onto selected coronal slices.

The results suggest a clear-cut process dissociation within the human MTL. While an activity increase of the BOLD signal accompanies successful retrieval of contextual information, an activity decrease provides a familiarity signal that, however, is sufficient for successful item recognition. These results are in line with previous event-related fMRI studies that suggested that less anterior MTL activity is related to the amount of familiarity. Thus, different functional processes, being either based on an activity increase or an activity decrease, might be involved with contextual retrieval and item memory in neighboring or overlapping MTL subregions. These findings indicate that an increase of activity of the hippocampus, which is reciprocally connected to numerous parts of associative cortex, is essential for the process of linking an item to contextual information during retrieval. Thus, *the MTL seems to support truly episodic memory by an activity increase during the successful retrieval of contextual information* (Takashima et al., 2006).

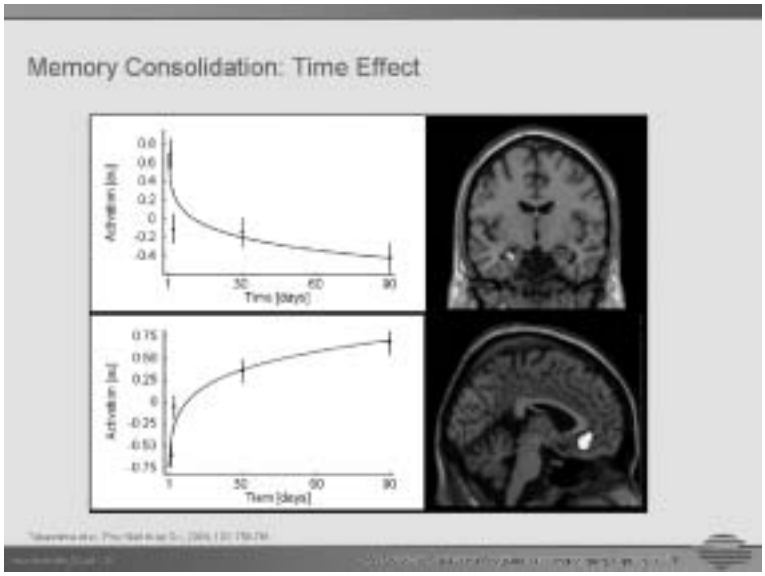

Figure 5 - The hippocampus showed a decline in activity related to memory retrieval over time. In contrast, the ventral medial prefrontal region showed an increase in activity related to memory retrieval over time

Interestingly the process of *memory consolidation is associated with a decrease of the strength of hippocampal activity, whereas the contribution of the inferior Pre-Frontal Cortex (PFC) becomes stronger* (Figure 5).

The key-note speaker of the memory Symposium, *Alcino J. Silva* of the Department of Neurobiology, Brain Research Institute, University of California, Los Angeles, USA, gave an overview of the progress made recently in understanding molecular and cellular mechanisms of cognitive functions in order to develop strategies to reverse cognitive deficits, and even to enhance cognition, in human. The observation that patients with Neurofibromatosis type I (NF1, or von Recklinghausen's disease), suffer from learning disabilities led to a search of the underlying molecular mechanisms. This proved to be relevant to understand molecular processes underlying memory and learning. NF1 is an inherited autosomal dominant neurological disorder that has complex clinical manifestations, such as benign neurofibromas and also learning disabilities that occur in 30% to 45% of the patients. The learning disabilities may include a depression in mean IQ scores, visuoperceptual problems and impairments in spatial cognitive abilities. Neurofibromatosis type I (NF1) is caused by mutations in the NF1 gene. Studies using mouse models of NF1 are beginning to unravel the mechanisms that underlie the cognitive impairments associated with the disease (Silva et al 1997).

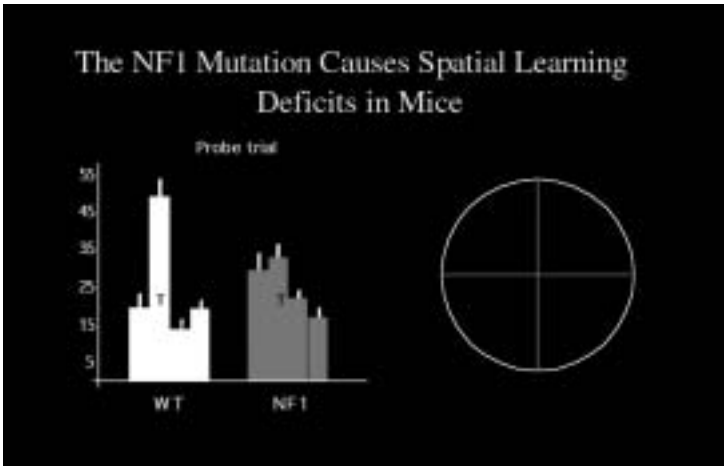

Figure 6 - Behavioral scores of mice in the water maze during the probe trial. The histograms show the percentage of time that the mice spend swimming in the 4 quadrants. During a training period the animals learned to find an escape platform in the quadrant indicated by the letter T. Later on, in the probe trial, where the platform had been removed, the wild type (WT) mice tend to spend most time in the quadrant where the platform had previously been located, indicating that they remembered the location of the platform; the NF1 mutants, on the contrary, show no preference for any of the quadrants, what indicates a deficit in spatial memory.

The NF1 Mutation causes impaired motor learning and spatial learning deficits in mice (Figure 6). This observation strongly motivated Alcino Silva's Lab to investigate the biochemical and molecular mechanisms by which NF1 may be associated with cognitive and neurological deficits (Costa et al 2002). NF1 encodes Neurofibromin, a p21Ras GTPase Activating Protein (GAP). Several experiments led to the conclusion that the *learning and memory deficits of a mouse model of NF1 (nf1+/-) appear to be caused by excessive p21Ras activity* such that p21Ras/Mitogen Activated Protein Kinase (MAPK) activity in the brain is enhanced. The latter leads to impairments in synaptic plasticity as revealed by a decrease of long-term potentiation (LTP), a cellular mechanism of learning and memory. The finding that excessive p21Ras/MAPK causes the learning deficits in nf1+/- mice led to question by means of which mechanisms does this learning impairment come about?

The researchers found that mice with enhanced Ras/MAPK activity showed enhanced inhibition mediated by GABA-A receptors and that decreasing GABA-A inhibition rescues the learning-deficits of NF1 mice. Using mice genetically manipulated to create cell-specific deletions of NF1, the researchers revealed that the cellular loci of the NF1 deficit are inhibitory but not excitatory neurons or astrocytes (Figure 7).

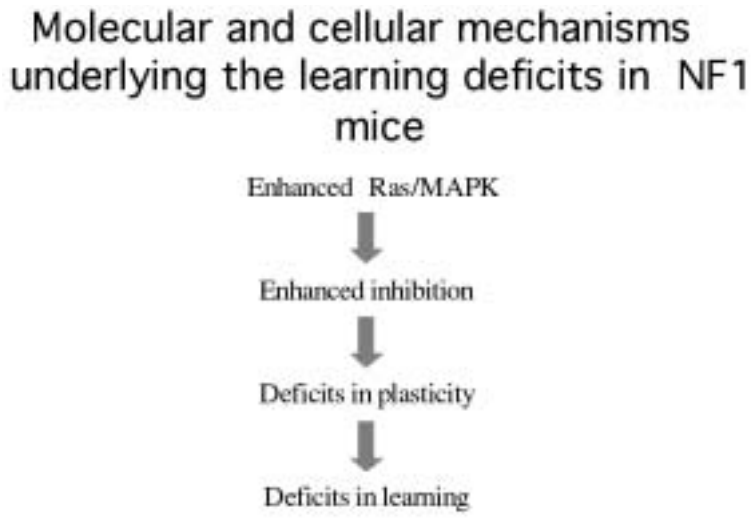

Figure 7 - Summary diagram of the molecular and cellular mechanisms underlying learning deficits in NF1 mice.

Alcino Sliva's group looked then to drugs that might counteract this excessive p21Ras/MAPK activity. They found that *Lovastatin* (Li et al 2005), a drug commonly used in the treatment of hypercholesterolemia, *decreases the enhanced brain p21Ras/MAPK activity of the nf1+/- mice, and reverses the spatial learning deficits of Nf1 mutant mice* (Figure 8).

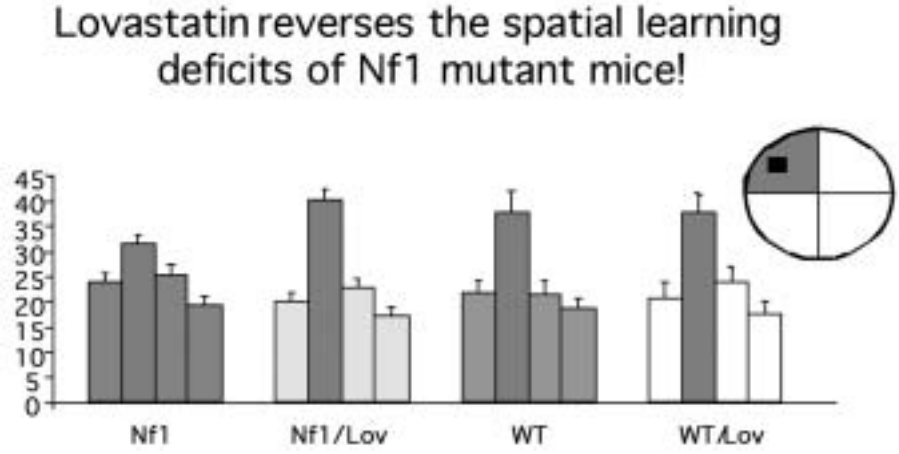

Figure 8 - Behavioral scores of the water maze test during the probe trial as shown in figure 6. The NF1 mutants show no preference for any quadrant indicating the spatial learning deficit as in the data of figure 6. The treatment with lovastatin treatment rescues the spatial memory. For comparison the behavioral score of the wild type (WT) mice without and with lovastatin. The drug has no effect on the WT.

This finding prompted a series of human studies in order to determine whether statins might also improve cognitive functions in NF1 patients and other human subjects, particularly their learning and memory capacities. Currently clinical tests are being carried out in Los Angeles, Washington and Rotterdam in this context.

In *conclusion, the symposium provided a diverse and detailed overview of the different networks in the MTL relevant for recognition and recollection and how they interact with other cortical and subcortical structures in the brain. Also insight was provided in some of the molecular and genetic underpinnings of memory.*

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PHYSIOLOGY OF "DÉJÀ-VU" AND DREAMY STATE EXPERIENCES

Patrick Chauvel*

The epileptic seizures which are named partial or focal are able to cause perceptive, cognitive, emotional, affective, autonomic, motor, gestural and/or behavioural changes, which are paroxysmal, and develop themselves in the same time-dimension as the normal "functions", thus producing a construction of abnormal phenomena (sensory or motor, conscious or unconscious), which can often be viewed as a caricature of normal but for the patient a breakdown of self-control. This production of signs and symptoms of epileptic fits has been carefully studied since the origins of modern neurology [1]. Some of them appeared so close to normal functioning that the pioneers were immediately aware that they were studying the effects of a sort of natural stimulation of brain areas provided by the disease, and able to give insight into the way the human brain is working.

One of the most demonstrative illustration of this was the phenomena of déjà-vu and “dreamy state”. Some patients, being conscious of their seizures or at least of their onset, can describe their attacks as follows:

“I’m reliving something…but I can see you clearly.... It’s as if what is happening now has already happened to me, it’s like an old memory that I am in the middle of living out” or “I see myself playing the drums, with people from my family listening to me” or ”It’s exactly the same as the last time, it’s all come back to me…it’s behind, it’s always thoughts from childhood, it’s always visual, it’s a place behind the house, the field where my father put his car, near a lake… It’s not always the same countryside; I’ve forgotten the story of this countryside…” [2].

These illusions of familiarity with the present experience (déjà-vu) or hallucinatory recall of past experience had been observed and

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reported by J. Hughlings Jackson at the end of the XIXth century [3]. In fact, his attention towards this phenomenon had been attracted by a physician who suffered from it, and had published himself his own observation. His epilepsy manifested first as “déjà vu” episodes, but their increasing recurrence finally got him worried. He consulted Jackson, who quoted his observation in an article published in 1888. At this time, in addition of too frequent déjà vu, he experienced abnormal reminiscences that he called “feelings of recollection”. Jackson noticed that “the psychical condition, therefore, is a very complex one”. During these attacks, the patient remains aware of the incoming sensory stimuli, so there are remains of normal consciousness and in addition a quasi-parasitical state of consciousness (dreamy state). This double consciousness was named “mental diplopia”. The dreamy state, for Jackson, corresponds to the activation of centers liberated from the control of higher centers impaired by epileptic activity. It arises from an excessive activity that is not epileptic, and thus is different than auras. Having the opportunity to perform autopsy in some of these patients, he concluded that this phenomenon was caused by medial temporal lobe (uncus) « discharging lesions ».

W. Penfield at the Montreal Neurological Institute inaugurated the modern era of epilepsy surgery in the 1920s. He based the delineation of cortical resections on direct stimulation of the cerebral cortex performed in the awake patient in the operating room [4]. At his great surprise, he could reproduce impressions of familiarity or strangeness or even evoke simple or complex scenes with visual or auditory components like arising from the memory of the patient: « on the first occasion, when one of these « flashbacks » was reported to me by a conscious patient (1933), I was incredulous. On each subsequent occasion, I marvelled. For example, when a mother told me she was suddenly aware, as my electrode touched the cortex, of being in her kitchen listening to the voice of her little boy who was playing outside in the yard. She was aware of the neighborhood noises, such as passing motor cars, that might mean danger to him. » [5]

Penfield did not view these paroxysmal symptoms as a continuum and distinguished between a feeling of familiarity (déjà vu or déjà vécu) and an elaborate visual hallucination. He analysed feelings of familiarity

and strangeness as illusions of recognition, classing these together with other sensory illusions (such as visual or auditory) and “emotional” illusions under the denomination “interpretive illusions” (misrepresentation or altered interpretation of present experience, [6]). The phenomena described as defining the “dreamy state” by Jackson were considered as hallucinations by Penfield and were thought of as being the replaying of a past experience, which he termed “experiential hallucinations”, all of which were presumed to be based only upon memories of experiences that the patient had personally lived through. The cortical sites where feelings of familiarity and visual hallucinations might be produced by electrical stimulation were widely distributed over the temporal neocortex [7]. Therefore, the MNI experience differed in anatomical location of the brain structures determinant for producing such phenomena, even though the temporal lobe seemed to be in cause anyway. However, due to the surgical approach used which privileged the exposed cortex, the access to the deeper areas of the medial temporal lobe was certainly uneasy [7].

The development of stereotaxic methods [8] then allowed to apply stimulations directly in these MTL structures through multiple lead intracerebral electrodes. Certain authors then found that the dreamy state could be provoked by stimulation of the MTL [9], [10], [11]. Halgren et al. [12] grouped together the mental phenomena provoked by stimulation of the hippocampal formation and amygdala. They noted that obtaining a response depended more upon whether or not an after-discharge occurred, than upon the location of the stimulation site. In other words, they found that the dreamy state is not a symptom specific to a single but needs recruitment of several structures, and that its content represents rather an idiosyncratic response that also takes into account the personality of the subject. Gloor et al., [13] could elicit experiential phenomena only by stimulation of the medial limbic areas, and especially of amygdala, which seemed to have the lowest threshold. Halgren and Chauvel [14], and Bancaud et al. [15] analyzed the anatomo-electro-clinical of 57 spontaneous seizures recorded in 16 patients with multiple intracerebral electrodes (Stereoelectroencephalography, SEEG), all of them characterized by déjà vu or dreamy state at their onset, and compared to the same phenomena induced by

direct electrical stimulation. They did not follow the classification of Penfield, considering that the illusions of recognition were not at the same level of perceptual/cognitive integration as the unimodal sensory illusions, and observed that if feelings of familiarity/strangeness, feelings of recollection, and reminiscences of elaborated past scenes could be recorded separately in the same patient, they were most of the time reported as successive or even simultaneous in the same experience. Their data were clearly in favour of a paroxysmal alteration of memory processes, sometimes intermingled with negative (more frequent than positive) emotions. They discussed in depth the anatomo-functional basis for their occurrence. These phenomena could be elicited by paroxysmal activation of either amygdala, hippocampus, or temporal neocortex, reconciling Jackson's and Penfield's views. But this was in fact the *co-activation* of MTL structures (amygdala and/or hippocampus) with areas of the lateral and posterior temporal cortex which represented the condition for production of the dreamy state components. So, the idea of activation of cortical networks where memories are embedded through the efferents of the hippo-campal/parahippocampal structures driven by the ictal discharge represented the new hypothesis for paroxysmal recollection and reminiscences. This hypothesis was based on the fact that the content of the experiential hallucinations is determined more by the structure which receives the hyperactivated efferences of the MTL rather than by the MTL itself. As a matter of fact, a large majority of the MTL stimulations evoked no psychical phenomenon at all, even when they are intense enough to induce after-discharges [12], [13], [16], [14]. In addition, surgical removal of the MTL structures alone does not abolish the occurrence of these manifestations. Therefore, activation of MTL neurones is able to produce these memory experiences, but other data indicate that this activation is neither sufficient, nor necessary. Another line of arguments suggests that the neurones whose activity directly underpins experiences evoked by the MTL stimulation are located outside the MTL. This holds to the diversity of the experiences, and the nature of the factors which select them. What is determining the category of the evoked experiences is not the anatomical situation of the stimulating electrode in amygdala, hippocampus or parahippo-

campal gyrus, but which personality is stimulated and the inter-personal situation at the time of the stimulation. The personal traits and inter-personal relations represent factors susceptible to modify the context in which a remote structure interprets the efferent message from the MTL. Another argument against a sole and direct role of the MTL in elaborating these complex experiences is the fact that, at the very moment when they are occurring its neurones are engaged in an after-discharge which renders them very likely unable to participate in a so specific and complex activity. It seems more reasonable to consider that these experiences are produced by a functional "inhibition" of the MTL associated with a paroxysmal disorganisation remotely induced by its efferent discharge. The main hypothesis proposed and discussed in the literature since Jackson are schematically illustrated in fig.1.

The anatomical organisation of the MTL efferences gives a plausible explanation why a MTL stimulation can synchronize vast territories in the supramodal temporal and temporo-parieto-occipital cortex. The type of discharge and the speed of its propagation are likely to play an important role, because awareness is preserved and the current sensory

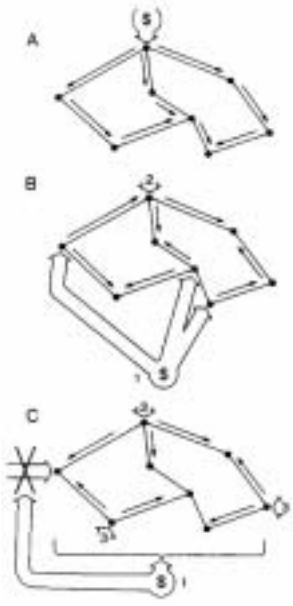

Fig.1: Schematic representation of the main hypothesis on the mechanisms of paroxysmal recollections (after [14]). A. Penfield's hypothesis: cortex stimulation reactivates memory traces embedded in the interpretive cortex (temporal lobe). B. Bancaud et al.'s hypothesis: MTL stimulation synchronizes remote cortical areas, and associates the pieces of the distributed network where memory traces are stored ; direct stimulation applied to the cortex can also be successful. C. Jackson's hypothesis: the "dreamy state" corresponds to the activation of "nervous centers" liberated from the control of higher "centers" impaired by epileptic activity; it arises from an excessive activity that is not epileptic by itself. The nodes connected by lines with arrows represent activated cortical networks; broad arrows with $ and numbers indicate the sites of stimulation.

stream access to consciousness maintained in parallel (“mental diplopia”).

This working hypothesis was strengthened by recent data obtained in a series of temporal lobe epilepsy patients investigated with a new strategy of electrode implantation. In order to get recordings directly in dysplastic lesions situated in the basal temporal cortex, new trajectories of orthogonal multiple lead electrodes were studied to reach this region in addition to the “classical” trajectories targeting amygdala and anterior and posterior hippocampus line. Under these conditions of recording, evoked déjà vu and dreamy state manifestations were obtained by stimulation performed in the course of presurgical investigations [17]. The anatomical situation of the basal temporal electrode allowed stimulation of the rhinal (entorhinal and perirhinal) cortices, in addition to the amygdala and hippocampus. Déjà vu and reminiscences were reported more often by the patients after stimulation of the rhinal cortices than by stimulation of amygdala or hippocampus. Moreover, the most sensitive site to obtain the illusion of familiarity was the entorhinal cortex, and that for reminiscences was the perirhinal cortex. These data are interesting by reference to recent developments in research on anatomo-functional basis of episodic memory. A double system (hippocampal and perirhinal) with distinct but cooperative functions has been hypothesized from convergence of experimental animal and human studies [18]. The perirhinal system is rapid and automatic, and contains familiarity and recency discrimination components of recognition memory; it is also part of a high-level perceptual system coding for visual feature ambiguity [19]. The hippocampal system is slower and associational, more specialised in spatial cognition, and gives access to episodes; it is the system for recollective aspects of recognition memory [18]. The former “knows” that the present event has previously occurred, and the latter “remembers” the previous experience. This dual organisation of declarative memory could underlie the mechanisms of déjà vu and dreamy state in epileptic patients.

However, the results of rhinal cortices stimulation do not appear in full agreement with experimental data. As a matter of fact, the stimulation of perirhinal rather than entorhinal cortex should induce

déjà vu. Two lines of arguments might be developed against a too focal view of these phenomena. First, such stimulations applied intracerebrally with macroelectrodes are considered to inactivate functionally the area close to the electrode and to activate its efferences [14]. Second, clinical observations of the patients presenting with these ictal symptoms show that the two components (déjà vu and recollection) cannot be easily dissociated, and if déjà vu may occur in isolation, recollection of memories is, obviously, felt in a context of familiarity [2, 15]. The network hypothesis [14-16] linking medial temporal limbic structures with associative sensory unimodal and polymodal areas remains the most plausible.

Using signal analysis we have studied in one patient functional coupling between the structures discharging after stimulation of the perirhinal region evoking a dreamy state-like hallucination [20]. A déjà vu illusion and experiential phenomena consisting of visual memories could be obtained by stimulation of the same site localised in the perirhinal area, but at distinct levels of intensity, the threshold for déjà vu being the lowest. These visual hallucinations were reported as objects or pieces of objects, from which a familiar scene is reconstructed. So, a priming effect was exerted by some visual features from which the patient was inferring a complete memory. Interestingly, this associational process seemed to be underlain by significant increase in functional coupling (as measured by cross-correlation) between perirhinal cortex and hippocampus on the one hand, between these two structures and the visual cortex on the other hand. During the stimulation inducing a déjà vu only, there was no correlation with the visual cortex, but synchronisation between the rhinal cortices and the hippocampus. Therefore, déjà vu phenomenon could be related to the abnormal synchronisation between the two systems, i.e. the perirhinal and the hippocampal ones, whereas the “dreamy state” would need an associational mechanism between perceptual/cognitive cortical areas mediated by the hippocampus (and the amygdala, adding the emotional tone of the experience) (fig.2).

The content of the memories pathologically recollected during the dreamy state was especially analysed in 16 subjects submitted to intracerebral recordings prior to epilepsy surgery [2]. Semiological

analysis showed a clinical continuity between déjà vu and visual hallucinations, the latter often consisting of a personal memory that was "relived" by the subject; such memories could be recent, distant or from childhood. With one exception, the particular memory evoked differed from one seizure to another, but were always drawn from the same period of the subject's life. Therefore, these paroxysmal experiences should be viewed as illusions of autobiographic memory. This study also confirmed the mediation of large neural networks that produce recall of recent or distant memories via activation of the hippocampus, amygdala and rhinal cortices.

Finally, the modernity of the Jackson's view of the Dreamy State deserves to be underlined. The fact that the abnormal discharge arises from the MTL network, but the reassembled episode emerges from a remote (normally functioning) cortical network was prefigured : "the elaborate state I call "dreamy state" arises during but slightly raised activities (slightly increased discharges) of *healthy* nervous arrangements" [3]. Reassembling pieces of a familiar visual scene (as described in most of the stimulation data reports) from basic elements through a priming mechanism might suggest a self-organizing process activated by a loss of normal control from "higher centers impaired by epileptic activity", a novel hypothesis in accordance with Jackson's visionary concepts.

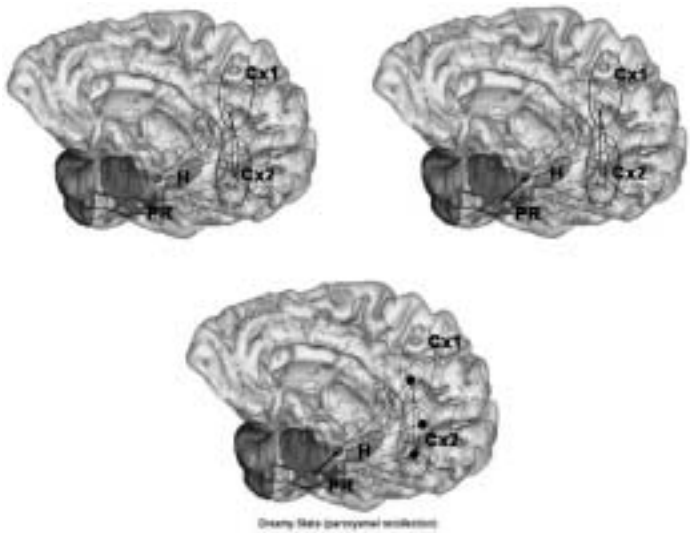

Fig.2: Déjà Vu and paroxysmal Recollections of memories : hypothetic mechanisms. An infero-medial aspect of the right hemisphere is represented. Top left (epoch 1): perirhinal cortex (PR) is functionally linked with some areas of the visual cortex (Cx2), whereas hippocampus (H) is in reciprocal relation with two distinct cortical networks (Cx1 and Cx2) representing distributed perceptual/cognitive elements of the current experience. Top right (epoch 2): low-level stimulation applied to rhinal cortex generates a feeling of déjà vu by synchronising the two systems (dark arrow), perirhinal at the tip of the ventral stream and hippocampal tuned to retrieval; an alternate possibility is that PR stimulation simply signals familiarity to hippocampus. Bottom (epoch 3): a stronger stimulation induces an after-discharge synchronising the two systems (dark arrow) and cortical associative sensory areas through activation of hippocampal efferences; as a result of this efferent discharge, a distributed cortical network rebuilding memories (Cx1, Cx2) is reactivated (dark dots) because of potentiated synaptic connections.

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HEARING THE NEWS OF THE DEATH OF PRINCESS DIANA AND SEPTEMBER 11: HOW SPECIAL ARE FLASHBULB MEMORIES?

*Lia Kvavilashvili**

Abstract

Some events produce vivid and detailed memories lasting for many years whereas others are less detailed and easily forgotten. What makes some events more memorable than others? What is the role of emotional arousal or trauma in the formation of these vivid memories? Most importantly, if something is remembered very vividly and in considerable detail does this necessarily mean that the memory is veridical? One area of research that has addressed these fundamental questions over the past 25 years is the research on flashbulb memories. These have been defined as particularly vivid and long lasting (autobiographical) memories "for circumstances in which one first learned of a very surprising and consequential (or emotionally arousing) event" (Brown & Kulik, 1977, p. 73). However, a question about the special status of flashbulb memories has remained controversial with some studies showing a good test-retest consistency and others showing substantial distortion and forgetting.

In this talk I will describe a series of studies that examined the flashbulb memories of the death of Princess Diana and the terrorist attack on New York on September 11, 2001. The results of these studies show that the phenomenological characteristics of flashbulb memories show virtually no forgetting. For example, 4- and 7-year old memories of the death of Princess Diana were as detailed, specific and vivid as memories of September 11 assessed after only few days from the attack. The special status and the accuracy of flashbulb memories was examined by investigating the test-retest consistency of flashbulb memories of September 11 and comparing it to the consistency of

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control memories about hearing some unimportant personal news. The results showed that flashbulb memories are not totally immune to forgetting as the consistency dropped reliably from September 2001 to July/August 2003. However, major distortions occurred in only 8% of cases, and they were not total confabulations. Rather, they referred to another occasion in which one heard of September 11 again later on the same or next day. Most importantly, flashbulb memories were significantly more consistent than memories of the control event even though their test-retest delay was almost twice as long (23-24 months) than for control memories (11-12 months). Taken together, the pattern of results supports the notion that flashbulb memories are special and different from ordinary non-flashbulb memories in terms of mechanisms that may be involved in their encoding and long-term retention. The theoretical and methodological implications of these findings will be discussed.

Introduction

Flashbulb memories have been defined as particularly vivid, detailed and long lasting autobiographical memories that are accompanied by high levels of confidence in their accuracy. We all have such vivid and detailed memories of personally important, emotionally arousing and/or unusual events from our personal life like for example a car accident we were involved in, the first romantic kiss or the day we failed an important exam, etc. However, in psychological research it has been customary to study these memories by asking people to remember their personal circumstances in which they first heard of the news of very important or tragic public event.

Major public events that have been used for studying flashbulb memories over the past 30 years include the assassination of President John F. Kennedy, the explosion of space shuttle Challenger and the resignation of British Prime Minister Margaret Thatcher. More recent events include the Death of Princess Diana and the terrorist attack on New York. One important point that needs to be stressed in relation to these studies is that participants are not asked to recall the details of the event itself (for example, what exactly happened in New York on 11 September). Instead, they are asked to recall their personal

circumstances in which they heard of the news such as where they were, what they were doing or who told them, etc.

For example, in a seminal study of Brown and Kulik (1977), who coined the term "Flashbulb Memories", 99% of participants were able to recall several details about their personal circumstances in which they first heard the news of the assassination of President Kennedy even though 13 years had passed from this event at the time of their testing. The results of this and subsequent studies indicate that remembering personal circumstances of hearing news of public events is a good analogue for studying flashbulb memories of personal events.

In order to explain the long-lasting nature of these memories Brown and Kulik (1977) postulated the existence of special encoding brain mechanism, the so called "print now" mechanism. According to Brown and Kulik (1977) this mechanism switches on when the levels of surprise and importance/consequentiality exceed certain threshold which will then result in a detailed and permanent memory trace.

The aim of the research that I am going to report today was to examine the hypothesis about the special status of flashbulb memories by studying two related issues: the stability of these memories and the accuracy of these memories. The stability concerns the question about how permanent these memories are over long time delays in terms of their phenomenological characteristics irrespective of their accuracy. In other words, will flashbulb memories of important public event that happened many years ago be as specific, detailed and vivid as flashbulb memories of an event that happened only a few days ago? This is an important research question that has not been directly addressed in previous studies (but see Kvavilashvili, Mirani, Schlagman & Kornbrot, 2003).

On the other hand, the issue about accuracy involves asking a different question, namely, if someone has a very vivid, detailed and specific memory of an important flashbulb event does it mean that this memory is accurate? This is a very important question especially in the light of recent debates on the recovered memories of childhood sexual abuse or the accuracy of eyewitness testimony. In the first half of this talk I will describe the study that examined the issue of stability of flashbulb memories irrespective of their accuracy and in the second part

of the talk I will describe a study addressing the issue about the accuracy of flashbulb memories.

In both studies participants were administered the standard flashbulb memory questionnaire which consists of three parts. First, participants are asked to recall their personal circumstances in which they first heard of the news of important public event (i.e., free recall). Next, they are asked five specific questions about what time did they hear the news, where they were, what they were doing, who told them, and whether there were others present. This can be regarded as a probed recall. Finally, participants are asked to provide ratings of their surprise, emotion, importance of event as well as the rating of vividness of their memory image. A 10-point rating scale was used with 1=not at all and 10=extremely.

The phenomenological characteristics of memories can be assessed by the number of details provided in memory descriptions and the specificity of participants' responses to five questions as well as the ratings of vividness. However, the accuracy of memories can only be assessed by administering the Flashbulb Memory Questionnaire twice, and comparing participants' responses given immediately after the event to their responses given after a long delay.

Study 1

The aim of Study 1 was to assess the stability of flashbulb memories by examining the following three phenomenological characteristics: The number of details mentioned in memory descriptions, the specificity of responses to 5 questions and the ratings of vividness. The basic idea was to compare these phenomenological characteristics of memories of two different but comparable public events, one with long and another with a shorter delay interval. In fact, we wanted to stretch this comparison to its limit by comparing flashbulb memories of September 11 that were only few days old to the flashbulb memories of the death of Princess Diana that were several years old.

In total, we had 5 different groups of young British participants (see Table 1 below). Two groups of 45 and 39 participants were tested in September 2001 for their flashbulb memories of terrorist attack on New York on September 11, 2001. One group was tested on 12 and 13

September and another group on 20 and 21 September. This means that these flashbulb memories were potentially as detailed, specific and vivid as one can get because the memories were very fresh, only 1-2 days old in Group 1 and 10-11 days old in Group 2. We then tested three additional groups of young British participants at different time points for their memories of the death of Princess Diana. One group was tested in December 2001 at which time their flashbulb memories of Princess Diana's death were 4 years and 3 months old (the fatal car crash happened on 31 August, 1997). The remaining two groups were tested in July/August 2003 and July/August 2004 which means that at the time of the testing their flashbulb memories of the death of Princess Diana were 6 and 7 years old respectively.

Table 1 - Information about participants, events, time of testing and age of memories in Study 1

Participants	Event	Time of testing	Age of memory
Group1 (N=45)	September 11	12-13 Sept. 2001	1-2 days
Group2 (N=39)	September 11	20-21 Sept. 2001	10-11 days
Group 3 (N= 65)	Princess Diana	Dec. 2001	4 years
Group 4 (N=89)	Princess Diana	July/Aug. 2003	6 years
Group 5 (N=47)	Princess Diana	July/Aug. 2004	7 years

The results are presented in Table 2 and show the means of the three phenomenological characteristics of memory as a function of group. The mean ratings of vividness of memory image were made on a 10-point scale. The end points of this scale ranged from 1=very vague, almost no image at all to 10=extremely vivid, almost as normal vision. It is important that participants rated the vividness of image of their personal circumstances and not the images of actual events broadcasted by television. As one can see there was no group effects ($F<1$) so that 7-year old memories of the death of Princess Diana were rated as vivid as only 1-2 days old memories of September 11. There was also no statistically significant difference between the groups in the mean number of details provided by participants in their memory descriptions

(F<1). Finally, we looked at the specificity of participants' responses to 5 questions about the time, location, activity, source and others present. For each question the specificity score could vary from 0 to 2 depending on the specificity of the response. The total specificity score could vary from 0 to 10. As one can see from Table 2, the mean specificity scores are generally very high in all four groups with a slight drop in Group 5. One way between group ANOVA revealed a significant main effect of groups. However, the follow up analysis showed that this effect was due to a significant difference between Group 1 and Group 5 only. Thus, one can conclude that there is very little drop in the specificity of memories at least within the first 6 years of the event.

Table 2 - Mean ratings of vividness, number of details in memory descriptions and specificity scores as a function of group. The results of 1-way between subject ANOVAs are presented in the final column.

	Group 1	**Group 2**	**Group 3**	**Group 4**	**Group 5**	**F (4,279)**
Vividness	8.13	7.85	7.75	7.84	7.53	F<1
No of Details	3.44	3.95	3.49	3.74	3.83	F<1
Specificity	9.09	8.72	8.55	8.84	7.79	F=4.55

In conclusion, the results of Study 1 provide convincing evidence in support of the idea that there is very little forgetting in flashbulb memories in terms of phenomenological characteristics of memories. Thus, 6-7year old memories of Princess Diana were as detailed specific and vivid as memories of September 11 that were only few days old. However, the next important question that we wanted to address was the question about the accuracy of these memories.

Study 2

As pointed out earlier, the accuracy or the consistency of flashbulb memories can be studied with the test-retest method where participants fill in the flashbulb memory questionnaire twice, once immediately or very soon after the public event and second time after a considerable delay. This allows to assess the consistency of participants' responses at the initial test and at the re-test.

The drop out rate in the test-retest studies of flashbulb memories is notoriously high. There are only two published studies with a delay interval longer than two years and, interestingly, both studies report significant distortions and forgetting in flashbulb memories. In Study 2, we therefore wanted to assess the test-retest consistency of flashbulb memories by using a long delay interval of 2 years and such an important and tragic public event as terrorist attack on New York on September 11, 2001. The enormous impact that this event had on international community gave us a unique opportunity to assess the special status of flashbulb memories. Indeed, if the terrorist attack on New York can not produce long lasting and consistent memories then the hypothesis of special status of flashbulb memories will be seriously compromised.

However, in order to properly assess this hypothesis one needs to compare the consistency of flashbulb memories to that of non-flashbulb control memories. This raises an issue as to what can count as an appropriate control event. Brewer (1992) has suggested that the most appropriate control will be asking participants to remember circumstances in which they heard of some personal but unimportant news, for example hearing that you did not win a prize in the competition. Therefore, a unique feature of the present study was that we staged a control event for the participants and then assessed the test-retest consistency in the same way as for the flashbulb event.

Thus, in the first study (Study 2a) young British participants were administered the Flashbulb Memory Questionnaire in September 2001. Half of the participants were tested on 12 and 13 September and half were tested on 20 and 21 September. Then all participants were re-tested in July/August 2003 after a delay of 23/24 months.

In the second study (Study 2b), a new sample of young participants was recruited in summer 2003 to take part in a study of memories of personally experienced events. They were warned that there would be several telephone interviews and that by taking part they had a chance to win £100 in a prize draw run by the researchers at the beginning of the study. After an initial introductory interview they were warned that the next interview would take place some time soon and were asked to keep their mobile phones switched. In few days, they indeed received

a call from a different researcher who told them that the interview was cancelled and additionally informed them that they did not win the prize in the prize draw. This was the staged control event and in subsequent interviews participants were administered the Flashbulb Memory questionnaire and asked to remember their personal circumstance in which they first heard of this unimportant personal news. Like in the flashbulb study (Study 2a), half of the participants were tested 1 or 2 days after hearing the news of not winning the prize and the other half was tested after 10-11 days. All participants were re-tested in summer 2004, 12 months after the event. Unfortunately, it was not possible to have a delay of 24 months for the control event. However, we reasoned that if flashbulb memories are indeed special and different from control memories then their test-retest consistency should still be significantly better after almost twice as long delay.

In Study 2a, there were 84 young participants (mean age 32 years, range 20-56) and in Study 2b - 89 young participant (mean age 37 years, range 22-50). The drop out rate after 24 and 12 months respectively was remarkably low so that the final number of participants who were tested on both occasions was 64 and 79 participants in Study 2a and 2b, respectively.

Having conducted the study the next stage was to code participants' responses on the Flashbulb Memory Questionnaire for the consistency. This was done separately for participants' answers to 5 specific questions in the probed recall and to participants' memory descriptions in free recall. When coding for the consistency of 5 questions we used the scoring system devised by Neisser and Harsh (1992). In this system, participants' responses to each question at the re-test are compared to their responses at an initial test and assigned a score of 0 to 2 depending on the match between the two responses. For example, a score of '0' is assigned if, at the re-test, participant does not remember where he was or provides a completely different location to the Place question. A score of '1' is assigned if the participant's response is less specific or only slightly incorrect (for example if initially the participant said at home in my bedroom but at the re-test they say at home or at home but in the kitchen). A score of '2' is assigned if the participant's response is exactly the same or even more specific than at an initial test. The so called Weighted Attribute Score proposed by Neisser and Harsh

(1992) puts particular emphasis on Place, Activity and Source and regards Time and Others Present as less important attributes of flashbulb memories. The scoring system takes this into account by assigning the maximum of 2 points for Place, Activity and Source and only one extra point if participant obtains a score of 3 or above for the Time and Others present. The resultant Weighted Attribute Score varies from 0 to 7 with higher scores indicating higher levels of consistency.

The mean Weighted Attribute Scores were entered into a 2 by 2 between subjects ANOVA with the event and delay interval as independent variables. The delay interval (i.e., whether participants were initially tested after 1/2 or 10/11 days) did not have any effect on memory consistency. However, there was a highly significant effect of event ($F(1,284)=91.50$, $p<.0001$, effect size-partial eta squared=.26). The consistency scores were reliably higher for flashbulb memories of September 11 ($M=5.13$, $SD=1.60$) than for the control event ($M=3.20$, $SD=1.73$) even though the test-retest interval was 24 months for September 11 and only 12 months for the control event. Importantly, the consistency score for September 11 is also reliably higher than the consistency scores reported in two previous flashbulb memory studies with long test-retest delays (2.95 in the study of Neisser & Harsh, 1992, and 3.30 in the study of Scmolck, Buffalo, & Squire, 2000).

However, one problem with the Weighted Attribute scores is that they do not distinguish the 'don't remember' response from major distortions. Consider the following memory description provided by one male participant on 21 September, 2001: "I was returning from a shopping trip and I put on the car radio and heard something on the news and it was very early and the news story was just breaking and they interrupted Radio 4... As I arrived home I just sat and watched it". Now compare it to this description provided in July 2003, two years after the initial test: "We were standing in a queue (Jenni and I) waiting to, check in for a flight to Nimes the day after - the 12th of September (a Wednesday or Thursday). We were quite horrorstruck and surprised. We had been packing up in the night before and we were subject when we checked in to the most vigorous and thorough luggage and body search to Nimes".

The problem with Weighted Attribute Score is that this participant will get a score of 0 together with the participants who acknowledge at

the re-test that they simply do not remember. In order to take this distinction into account and to get a better idea about what was really happening with participants' memories we separately examined participants' memory descriptions and coded them into the following six categories:

Don't remember
Major distortion (as in the above case)
Minor distortion (if the memory description was mostly correct but one or two details were incorrect)
Less specific (if the description was same but contained less details)
More specific (if the description was the same plus contained additional details)
Same (if the description at re-test was exactly the same as at initial test).

Table 3 presents the percentage of participants whose memories were classed into these six categories as a function of event: flashbulb versus control. The chi-square test on this data revealed a highly significant effect of event ((2=62.40, N=143, df=5, p<.0001). However, the table shows that the difference is not in the categories of major or minor distortions. The number of memories classed into these categories are surprisingly low for both types of events. The major difference between the two events lies in the categories of "Less specific" and "More specific". While 62% of control memories became less specific after 12 months only 14% of flashbulb memories became so after 24 months. Moreover, while 44% of flashbulb memories became more specific only 1% of control memories became more specific.

Table 3 - The percentages (raw numbers in brackets) of participants whose memories were classed into six categories as a function of event (flashbulb vs. control)

Consistency of Memory Description							
	Don't Remember	**Major Distortion**	**Minor Distortion**	**Less Specific**	**More Specific**	**Same**	**Total**
Flashbulb	0% (0)	8% (5)	22% (14)	14% (9)	44% (28)	12% (8)	100% (64)
Control	9% (7)	11.5% (9)	11.5% (9)	62% (49)	1% (1)	5% (4)	100% (79)

In conclusion, several interesting findings emerged from this study. First finding is that the consistency was markedly better than in previous two studies with long delays by Neisser and Harsh (1992) and Schmolck et al. (2000). This superior consistency was obvious both in terms of Weighted Attribute Scores and especially in terms of the percentage of memory descriptions classed as major distortions. Thus, in the study of Neisser & Harsh (1992), 25% of memories were classed as major distortions and in the study of Schmolck et al (2000) this percentage was as high as 40%. In contrast, in our study only 8% of memories were classed as major distortions.

However, the mere fact that there was 8% of major distortions and 22% of minor distortions indicates that flashbulb memories are not totally immune to forgetting as originally suggested by Brown and Kulik (1992). This is also obvious from the mean Weighted Attribute Score for September 11 that it was not at ceiling, the mean score was 5.13 out of possible 7 after two years from the event.

However, by far the most important and novel finding that emerged from the study is that the retention rate was significantly better for flashbulb memories than for memories of the staged control event. If we had not compared flashbulb memories to control memories we would have concluded that flashbulb memories are not special and different from other autobiographical memories of more ordinary events. However, the results clearly show that flashbulb memories are retained in quantitatively and qualitatively different ways than non-flashbulb memories.

Conclusions

The overall conclusion, based on the results of our studies, is that flashbulb memories are indeed special and different from ordinary autobiographical memories. Although they may not be totally immune to forgetting they do show remarkable permanence in phenomenological characteristics over as many as 6-7 years as shown in Study 1 and significantly better consistency than memories of control events as shown in Study 2.

The present findings contrast the currently prevailing view held by flashbulb memory researchers who claim that flashbulb memories are

prone to distortions and forgetting like ordinary, non-flashbulb memories. For example, according Talarico and Rubin (2003) the only difference between the two is that flashbulb memories are accompanied by high levels of confidence in their accuracy but are no more accurate than ordinary memories. However, the results of our study show that flashbulb memories are far more consistent than ordinary non-flashbulb memories.

In fact, I believe that some distortions in flashbulb memories found in our study are due to research methodology and that flashbulb memories of personal events are even more accurate than the ones studied via important public event. The problem with public events is that they are constantly televised and talked about by everyone so that each person would hear the news again many times during the day from different sources. Therefore, over the time it may become difficult to distinguish between different memories of hearing the news and to know which one is when you heard the news very first time. In other words, the memory distortions are not complete confabulations but they represent a real but different occasion in which the participants heard about the news. For example, there is no doubt that the participant whose memory descriptions I showed you earlier did indeed hear the news again on the next day at the airport on his way to Nimes. However, he incorrectly remembered this as the first time he heard this news. This would also explain why people are so confident in their memories classed as "major distortion". Therefore, my prediction is that the percentage of distortions will be much lower if it was possible to study the consistency of flashbulb memories via personal events rather than public events.

It is also interesting that recent studies on the brain mechanisms involved in the formation of emotional and especially traumatic memories provide further support for the idea that flashbulb memories are special. In particular, studies by Cahill and his colleagues have shown the involvement of noradrenergic system and amygdala in the formation of emotional memories. For example, several studies have shown that the administration of beta-adrenergic receptor antagonist propranolol selectively impairs memory for emotionally salient film episode but does not affect the memory for non-emotional episodes

(Cahill & McGaugh, 1998). O'Caroll and colleagues have also pointed out the adaptive value of such brain mechanism which helps the organism to better recall potentially dangerous situations (O'Caroll et al., 1999).

Finally, I wish to acknowledge the support of Economic and Social Research Council in conducting research on flashbulb memories and the contribution of my collaborator Diana Kornbrot as well as numerous research assistants who helped me to collect and code the data. Also, to finish on a lighter note, it appears that even animals have flashbulb memories as portrayed by this cartoon. Apparently all forest animals can remember to this day exactly where they were and what they were doing when they first heard about the news that Bamby's mother was shot by an evil hunter.

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INCREDIBLE MEMORIES: HOW ACCURATE ARE REPORTS OF ANOMALOUS EVENTS?

Christopher French*

Abstract

Reports of unusual experiences of an ostensibly paranormal nature are surprisingly common throughout all known societies, both historically and geographically. This might be taken as strong evidence that paranormal forces really do exist but we must bear in mind that in attempting to evaluate such evidence we are dealing with reports of these experiences and not directly with the experiences themselves. The issue of the actual degree of accuracy of anecdotal accounts is therefore central in assessing such evidence. Psychologists have studied the accuracy of eyewitness testimony for many decades and, more recently, there has been a great deal of research carried out on the topic of false memories. An overview of research in these two areas is presented with a particular focus upon the accuracy of reported memories for anomalous experiences. It has been shown that eyewitness accounts of faked sÈances and other pseudo-psychic demonstrations are often highly inaccurate. Recent research has also considered memory conformity effects whereby the account of an ostensibly paranormal event provided by one witness can be shown to have an effect upon the accuracy of the report of a co-witness. It is often the case that the degree of memory distortion in such studies is related to the level of paranormal belief. Paranormal belief and the tendency to report ostensibly paranormal experiences have been shown to be correlated with a number of psychological variables which themselves correlate with susceptibility to false memories. These variables include dissociativity, absorption, fantasy proneness, hypnotic susceptibility, and reports of childhood trauma. This raises the possibility that at least

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some reports of ostensibly paranormal experiences may be based upon false memories. The results of recent studies supporting such a claim will be presented. Various possible interpretations of the link between paranormal belief and experience, childhood trauma, dissociativity and fantasy proneness will be presented and evaluated.

Introduction

Ever since records began and in all known societies, people have reported unusual experiences which, taken at face value, would suggest that the current conventional Western scientific world view is at best incomplete and at worst seriously in error. Many of these experiences would today be labelled as 'paranormal' and it is recognised that they are a major factor in explaining the high levels of paranormal belief found even in modern societies (e.g., Blackmore, 1984). The ubiquitous nature of such claims might be taken as evidence that paranormal forces really do exist, but it must always be borne in mind when dealing with such reports that they are almost always mainly dependent upon the memory of the claimant. The issue of the actual degree of accuracy of anecdotal accounts is therefore central in assessing such evidence. French (2003) presented a comprehensive review of the relevance of research into eyewitness testimony and false memories for reports of anomalous experiences. It is the purpose of my paper today to summarise and update that review, including the presentation of recent, as yet unpublished, findings from my own research unit. The first part of this talk will describe findings from recent studies of eyewitness testimony relating to ostensibly paranormal events, including studies of the effects of verbal suggestion, and misinformation effects, including so-called memory conformity effects. The second part will discuss the relevance of false memory research in assessing reports of anomalous experiences, and discuss various possible interpretations of the empirical link between paranormal belief and experience, childhood trauma, dissociativity and fantasy proneness.

Verbal Suggestion and Eyewitness Testimony for Anomalous Events

A vast body of experimental literature demonstrates that eyewitness testimony for crimes and other events can often be extremely unreliable even when no deliberate attempt is made to distort the memories of witnesses (e.g., Loftus, 1979). The same is true of witnesses to ostensibly paranormal events. French (2003, p. 157) highlights several factors often associated with such events that would serve to undermine the reliability of honest witnesses including "poor viewing conditions (e.g., darkness or semi-darkness), altered states of consciousness (e.g., due to tiredness, biological trauma, engaging in particular rituals or drug abuse), emotional arousal, and either the ambiguous and unexpected nature of the event on the one hand (in spontaneous cases) or a high level of expectation and will to believe on the other (e.g., in a séance)".

More recent research by Richard Wiseman and colleagues has explored the effects of verbal suggestion on the reliability of eyewitness accounts of séances and other ostensibly paranormal events, taking their cue from the fact that fraudulent mediums and mentalist conjurors have often described how powerful simple verbal suggestion can be in influencing witnesses' accounts of an event. Wiseman, Greening, and Smith (2003), for example, carried out a fake séance in which an actor suggested that a stationary table was moving. In response to a memory questionnaire, around one third of the observers incorrectly reported that the table had indeed moved, with this tendency being stronger for believers in the paranormal than for disbelievers. In a second experiment, Wiseman *et al.* systematically varied whether the verbal suggestions provided were consistent or inconsistent with the observers' stated attitude towards the paranormal. Believers were found once again to be more susceptible to the effects of suggestion than disbelievers but only when the suggestion was congruent with their stated belief. Overall, around 20% of those taking part in these fake séances reported believing that genuine paranormal phenomena had taken place in the darkened séance room.

Wiseman and Greening (2005) explored the effects of verbal suggestion in another ostensibly paranormal context. Many eyewitnesses to alleged psychokinetic metal-bending attest that not only

did they see a metallic object (typically a key or a piece of cutlery) bend while in the hands of the alleged psychic, but that it continued to bend right before their very eyes even when it had been placed on the table in front of them. The claim that the metal continues to bend even when not in contact with the psychic is offered as compelling evidence that a genuinely paranormal effect had taken place, not simply some form of sleight of hand. Wiseman and Greening provide convincing evidence that such a conclusion would be inadvisable given the unreliability of eyewitnesses in this context. They presented participants with a video clip showing a skilled conjuror playing the part of an alleged psychic claiming to use psychokinetic ability to bend a key, although in fact using sleight of hand to achieve the effect. Having bent the key, the psychic then placed it back on the table and the video clip ended with a long close-up of the bent key. Half of the participants then heard the psychic say that the key was continuing to bend while the other half saw the same footage but without the verbal suggestion. Although the key did not in fact continue to bend, around 40% of the participants in the suggestion condition reported that it did. Only one participant out of 23 reported that the key continued to bend in the no-suggestion condition. A second study replicated this general pattern of results as well as showing that those who reported that the key continued to bend were more confident in their testimony and also less likely to recall the actual verbal suggestion from the fake psychic. Surprisingly, no differences were found between believers in the paranormal and non-believers in terms of their susceptibility to verbal suggestion in this context.

These studies provide an interesting insight into the effects of the hitherto neglected factor of verbal suggestion on eyewitness reliability in an ostensibly paranormal context. Despite the attractiveness of obtaining such results in relatively naturalistic settings, questions remain regarding the mechanisms that underlie the reported effects. It is possible that the verbal suggestion affected either the perception of the event or the observer's memory of the event or both. It is even possible that neither of these explanations is correct and that instead the participants were influenced by the demand characteristics of the situation, i.e., they were simply giving the responses that they believed

the investigators wanted to receive. Future investigations should be directed at attempting to determine which of these explanations is correct. However, the visible surprise upon the faces of many witnesses in response to such simple verbal suggestions would suggest that at least some of them do perceive the events in line with the verbal suggestion.

Post-Event Misinformation and Eyewitness Testimony for Anomalous Events

If no attempt is made to influence an eyewitness's memory for an event until after the event has taken place, we can be sure that any effects reported cannot be due to any direct effect upon the perception of the event itself. A number of different techniques have been developed that show the distorting influence of misinformation presented after an event has been witnessed. Memory researchers have been studying such misinformation effects for over 30 years (e.g., Loftus, 1979). What these techniques have in common is that witnesses first observe a complex event such as a staged crime or accident. Half of the participants are then exposed to misleading information about the event, while the other half are not so exposed. Finally, all participants are tested upon their recall for the original event. Typically, a higher degree of memory distortion is observed amongst the participants exposed to the misinformation.

This approach was employed in a recent study of memory for psychic readings in which we demonstrated that believers in the paranormal are more likely to misremember a psychic reading in such a way that they recall the information provided by the psychic as being more specific than it actually is (Wilson & French, in preparation). Participants were presented with a video clip of an alleged psychic giving a reading to a sitter, followed by another clip in which the sitter comments upon the accuracy of the reading. In fact, both the reading and the post-reading interview were entirely scripted. Two different versions of the video were prepared. Both versions showed identical readings and almost identical post-reading interviews with the sitter, apart from one crucial statement. In one version of the interview the

sitter correctly asserts that the psychic "mentioned the name Sheila, and that is my mother's name. In the other version, she incorrectly asserts that the psychic "said my mother's name was Sheila", making it appears that the psychic was more specific in his utterance than he actually was. We had hypothesised that this post-event misinformation would lead to greater memory distortion on the part of believers than non-believers, given that greater accuracy on the part of the psychic would be congruent with their general belief in psychic ability. In fact, somewhat surprisingly, we found that believers showed a strong tendency to misremember this part of the reading as being more accurate than it actually was *whether or not* they received the post-reading misinformation. Non-believers tended to remember the reading more accurately than believers if no misinformation was supplied but, interestingly, their memories were as distorted as the believersí in the misinformation condition.

We are as interested in investigating possible memory biases on the part of non-believers as we are on the part of believers. We therefore intend to carry out a follow-up experiment using a similar methodology, i.e., scripted reading and post-reading interview. However, in addition to the conditions described above, we would also include a condition in which the sitter makes an incorrect assertion that makes the psychic appear to be *less* accurate and specific than he actually was. The main foci of interest would be (a) to replicate effects found in the first study using this methodology and (b) to investigate the possibility that non-believers would show greater memory distortion than believers when the post-reading misinformation is congruent with their beliefs.

When attempting to ascertain the reliability of eyewitness accounts in either a forensic context or in an anomalistic context, highly similar accounts from multiple witnesses are understandably taken as being more reliable than either a single uncorroborated account or an account which differs from that of another witness. While such an assumption is probably justifiable, it should always be borne in mind that such accounts may well be influenced by a particularly insidious form of misinformation effect known as memory conformity. When multiple witnesses observe an unusual event such as a crime or a possible sighting of a UFO, a ghost or the Loch Ness Monster, they will be very

likely to discuss the event between themselves prior to any formal questioning by investigators. Memory conformity refers to the phenomenon whereby the testimony of one eyewitness directly influences the testimony provided by a second eyewitness (Gabbert, Memon, & Allan, 2003). For example, if pairs of participants are asked to view a video recording of a staged crime and are led to believe that they have both viewed the same video clip when in fact the video clips are subtly different, it can be shown that, following discussion, one witness's account can have a direct influence on that of the co-witness. Thus the first witness may report directly observing actions (such as someone stealing a purse) that were in fact only directly observable on the co-witness's video clip.

As already described, Wiseman and Greening (2005) showed participants a video clip of a key being bent by an alleged psychic who was in fact using sleight of hand. They reported that around 40% of the participants reported that the key continued to bend after it was placed on the table if the psychic simply said, "Look. It's still bending." In the absence of such a suggestion from the psychic, virtually no one reported that the key continued to bend. Wilson and French (submitted) replicated this basic effect, but also found, in contrast to Wiseman and Greening, that believers were more strongly affected by the verbal suggestion than non-believers. However, we also went one stage further by adding a memory conformity component to the original experimental design. In addition to the conditions used by Wiseman and Greening, we also included conditions with a stooge present. The stooge either indicated that he did see the key continue to bend or that he saw that the key did not continue to bend. We found that the stooge's expressed belief about whether the key continued to bend also had an effect on the reports of the genuine participants. Clearly, although the original verbal suggestion may have a direct effect on the witnesses' perception, the subsequent influence of the stooge's expressed belief must be explained in terms of either an effect on memory or demand characteristics. Further experiments investigating memory conformity are planned, using a wider range of ostensibly paranormal events.

False Memories and Reports of Anomalous Events

Studies of the unreliability of eyewitness accounts of observed events have been carried out since the early days of scientific psychology and it has long been recognised that details of witnessed events may be lost or distorted in memory. A great deal of research over the last couple of decades, however, has focussed upon the fact that some apparent memories appear to be entirely false; that is to say, they are not based upon any actual event directly witnessed by the claimant at all. French (2003) also considered the relevance of this body of research with respect to reports of anomalous events, especially reports of past-life memories and alien abduction claims (see also, French, 2001; Holden & French, 2002).

A wide range of experimental paradigms have been developed to investigate the factors that lead to the development of false memories although a comprehensive review of these techniques is beyond the scope of the current presentation (see, e.g., Garry & Gerrie, 2005; Loftus, 1997, 2001, 2003; McNally, 2003; Ost, in press; Smeets, Jelicic, Peters, Candel, Horselenberg, & Merckelbach, in press). One of the issues that has been the focus of a great deal of attention is the identification of psychological variables that correlate with susceptibility to false memories. A number of such variables have been identified, although it should be noted that there is considerable variability in the findings across studies, possibly reflecting the variation in experimental paradigms employed and other factors. Amongst the variables that appear to correlate with susceptibility to false memories (at least in certain contexts) are fantasy proneness (Spanos, Burgess, & Burgess, 1994), hypnotic suggestibility (e.g., Barnier & McConkey, 1992), dissociativity (e.g., Hyman & Billings, 1998), absorption (e.g., Eisen & Carlson, 1998), and vividness of visual imagery (e.g., Winograd, Peluso, & Glover, 1998). As French (2003) points out, these variables have also been shown to correlate with paranormal belief and/or tendency to report paranormal experiences (fantasy proneness: e.g., Irwin, 1990, 1991; hypnotic suggestibility: e.g., Kumar & Pekala, 2001; dissociativity: e.g., Wolfradt, 1997; absorption: e.g., Irwin, 1985; vividness of visual imagery: e.g., Diamond & Taft, 1975). This raises the possibility that at

least some reports of anomalous experiences may be based upon false memories and that those who report such experiences may be more susceptible to false memories.

Until recently, direct attempts to test the hypothesis that there is a link between susceptibility to false memories and the tendency to report anomalous experiences had met with only limited success (French, 2003), possibly reflecting the use of inappropriate techniques to measure susceptibility to the type of false memories that one might expect to correlate with the tendency to report anomalous experiences. Intuitively, one might expect that techniques that attempt to produce detailed false memories for entire episodes (e.g., Loftus & Pickrell, 1995) might be more relevant than techniques which attempt to produce false memories for, say, individual words in word lists (e.g., Roediger & McDermott, 1995).

It is somewhat surprising then that up until recently one of the few studies to produce results supporting this hypothesis had used the latter type of technique. Clancy, McNally, Schacter, Lenzenweger, and Pitman (2002) used a technique which involves presenting lists of word to participants. Within each list, all words are strongly semantically related to a critical non-presented word. For example, the words *bed, pillow, snore, dream, snooze,* and so on might be presented, but not the critical word *sleep.* Subsequently, many participants would incorrectly recall or recognise the word *sleep* as having been on the original list. Clancy *et al.* used this technique to demonstrate that people with conscious memories of having being abducted by aliens were more susceptible to false memories than either people who believed that they had been abducted by aliens but had no conscious memory of the event or people who did not believe that they had ever been so abducted.

A strong case can be made that false memories are indeed likely to be the explanation for reports of alien abduction and contact, although a number of other factors may also be involved (Clancy, 2005; French, 2001; Holden & French, 2002). Results of a recent study by French, Santomauro, Fox, Hamilton, and Thalbourne (2005) generally support this claim insofar as a group of participants reporting memories of alien contact were found to score more highly than a control group on a number of variables known to correlate with susceptibility to false

memories, including dissociativity, fantasy proneness, tendency to hallucinate, and absorption. However, no differences were found between the experiencers and the control group in this study in terms of susceptibility to false memories as assessed by the same measure as used by Clancy *et al.* (2002).

Many theoretical models of how false memories are formed would predict that believers in the paranormal would be more susceptible to false memories for ostensibly paranormal events simply because such events would be more plausible for believers than for non-believers. For example, Mazzoni, Loftus, and Kirsch (2001) presented a three-stage model in which, for a false memory to develop: (a) the event in question must be deemed to be plausible, (b) the individual must have good reason to believe that the event is likely to have happened to them personally, and (c) they must interpret their thoughts and fantasies about the event as actual memories (see also Mazzoni & Kirsch, 2002; Scoboria, Mazzoni, Kirsch, & Relyea, 2004). They presented evidence in support of this model by showing that individuals who initially reported that they had not witnessed another individual being possessed subsequently increased their estimate of how likely it was that they had witnessed such an event following interventions designed to increase the subjective plausibility of the event for the participants.

However, given the fact that so many psychological variables that correlate with susceptibility to false memories also correlate with paranormal belief and the tendency to report anomalous experiences, it is possible that believers in the paranormal may show a more general susceptibility to false memories that also encompasses susceptibility to false memories for non-paranormal events. Recent findings from our research unit support such a possibility. Wilson and French (2005) had one hundred participants complete a “News Coverage Questionnaire” concerning personal memories of where they were, what they were doing and who they were with when news footage of dramatic news events was first shown on television, as well as asking them to recall details of the footage itself. These news items included four events that are known to have been captured on film and one item concerning non-existent footage of the explosion of a bomb in a nightclub in Bali. Overall, 36% of respondents reported false memories of the alleged

footage of the Bali bombing. Participants reporting false memories were found to score significantly higher than those who did not report such memories on various measures of paranormal belief and experience, supporting the hypothesis that believers in the paranormal may be more susceptible to even non-paranormal false memories than non-believers. We have subsequently replicated this finding in a new sample of participants.

A Link with Childhood Trauma?

The final topic I wish to cover is that of the various possible interpretations of the empirically established link between paranormal belief and experience, dissociativity and fantasy proneness, and reports of childhood trauma. Ever since the early 1990s, psychologists and parapsychologists have known that fantasy proneness correlates with both paranormal belief and tendency to report paranormal experiences (e.g., Irwin, 1990, 1991). Fantasy proneness was first identified by Wilson and Barber (1983) as being a characteristic of highly hypnotically susceptible individuals. Fantasy-prone individuals spend much of their time engaged in fantasy, have particularly vivid imaginations, sometimes confuse imagination with reality, and report a very high incidence of paranormal experiences. They also are much more likely to report a history of childhood trauma. It has been postulated that fantasy proneness sometimes develops as a defence mechanism to help a child to cope with an on-going aversive situation (e.g., Lynn & Rhue, 1988; Rhue & Lynn, 1987). Fantasy provides an escape from an intolerable situation over which the child has no control into a world of imagination where the child at least has the illusion of control.

Similar arguments have been put forward to explain the association between reports of childhood abuse and tendency to dissociate. Dissociation is defined by the DSM-IV as “A disruption in the usually integrated functions of consciousness, memory, identity, or perception of the environment. The disturbance may be sudden or gradual, transient or chronic” (American Psychiatric Association, 1994, p. 766). There are problems with this definition and in practice the term

dissociation is applied to a wide range of altered states of consciousness. Many therapists believe, however, that dissociative tendencies develop in childhood, again as a defence mechanism to help the child cope with trauma. It is believed that the dissociated state somehow attenuates awareness of the child's stressful circumstances. Indeed, many therapists would view dissociation as being the mechanism responsible for repression of memories of such experiences, although the concept of repression itself has been questioned by experimental psychologists (see, e.g., McNally, 2003). The issue of whether or not repression ever occurs is beyond the scope of the current presentation. We do know, however, that dissociativity has been found to correlate with retrospective reports of childhood abuse (e.g., Mulder, Beautrais, Joyce, & Fergusson, 1998) and also with reports of ostensibly paranormal experiences and with paranormal belief (e.g., Irwin, 1994; Pekala et al., 1995; see French, 2003, for review). A number of investigators have provided evidence for a direct link between reports of childhood trauma and paranormal belief/experience (e.g., Irwin, 1992, 1993; Lawrence, Edwards, Barraclough, Church, & Hetherington, 1995).

What is the best explanation for the positive relationship between fantasy-proneness, tendency to report paranormal experiences and belief in the paranormal? Sceptics would argue that many claims of paranormal experiences reflect the overactive imaginations of the claimants. People with fantasy-prone personalities have very good imaginations and their claims to paranormal experiences may well reflect such imagination rather than any events that actually occurred. It is, furthermore, widely accepted that one of the most important factors in determining belief in the paranormal is personal experience of ostensibly paranormal events. According to this chain of reasoning, fantasy proneness leads to the experience of ostensibly (but not actually) paranormal events, which in turn leads to belief in the paranormal.

Proponents of the paranormal, on the other hand, have often offered a different explanation of the relationship. They have argued that "fantasy proneness may engender paranormal belief, which in turn may be conducive to parapsychological experience" (Irwin, 1991, p. 321). It

is widely believed by proponents of the paranormal that believers are much more likely to experience genuine paranormal events than disbelievers. According to this chain of reasoning then, fantasy proneness leads to belief in the paranormal, which in turn makes that person more likely to experience genuine paranormal phenomena.

A third possibility is that both the reports of ostensibly paranormal experiences and the reports of childhood abuse are based upon false memories, as the measures of childhood abuse used in these studies were retrospective in nature. French and Kerman (1996) presented data comparing fantasy proneness scores and levels of paranormal belief in 23 institutionalised adolescents with reported histories of abuse with those of 23 well-matched control participants with no known history of abuse. The former group did indeed score higher than the controls, suggesting that the results of studies using retrospective questionnaire measures can probably be accepted at face value. However, the limitations of this study must also be recognised. Although the adolescents had been institutionalised on the basis of alleged abuse, the investigators did not have direct access to documentary proof of such abuse. While it seems reasonable to assume that the institutionalised group would indeed have endured a higher level of actual abuse than the control sample, much more research is needed in this area before definitive conclusions can be drawn.

Of course, the above possibilities are not mutually exclusive. It is conceivable that childhood abuse does indeed lead to increased levels of fantasy proneness and dissociativity and consequently that retrospective reports of abuse have a basis in fact. It may also be the case, however, that this leads to heightened susceptibility to false memories and that *some* of the reported memories of abuse from any particular individual are false, even though others are true. This leaves open the question of whether or not the reported memories of paranormal and related anomalous experiences are based in fact. It is possible that someone may have an increased susceptibility to false memories as a consequence of actual, always-remembered abuse. It would be ironic and tragic, however, that the testimony of such a victim might be severely undermined in the eyes of most psychologists and psychiatrists if it were to be contaminated with false memories for more

extreme forms of abuse and/or paranormal experiences. Such considerations underline the extreme caution needed to approach the truth in such cases.

The link between childhood trauma and reports of paranormal experiences merits much more research. Within anomalistic psychology, correlations between variables are often exactly those that one would expect. For example, it is not too surprising that reports of having personally experienced the paranormal are correlated with paranormal belief. But the link with reports of childhood trauma is not one of those intuitively obvious relationships ñ and yet it seems to be a reliable and robust finding in need of explanation.

Conclusion

The evidence reviewed above and by French (2003) confirms the notion that much is to be gained by considering the implications of research into eyewitness testimony and into false memories when assessing the accuracy of reports of anomalous events. Research into the accuracy of eyewitness testimony, especially for staged pseudo-psychic demonstrations, strongly suggests that such reports should be treated with caution. In addition to the basic unreliability of human memory, factors such as verbal suggestion and post-event misinformation also have a distorting effect upon memory and possibly even the actual perception of such events. In many situations, believers in the paranormal appear to be more susceptible to such memory distortion but more research should be directed at establishing whether belief-congruent biases might also be found in non-believers in appropriate contexts.

With respect to false memory research, recent findings suggest that believers in the paranormal may show greater general susceptibility to false memories, including false memories for non-paranormal events. Future research should be directed at confirming such findings with a wider range of experimental techniques, as well as attempting to distinguish between the various possible explanations for the link between paranormal belief and experience, psychological variables such as fantasy proneness, dissociativity and hypnotic suggestibility, and reports of traumatic childhoods.

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MEMORY AND SUGGESTIBILITY IN MALTREATED CHILDREN: AN EXAMINATION OF CURRENT RESEARCH AND THEORY RELEVANT TO CHILDREN'S DISCLOSURES OF ABUSE

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Abstract

In this presentation I plan to discuss the most recent scientific advances in our understanding of suggestibility effects and false memory formation in children, with special attention to issues of memory and suggestibility in forensic interviews of maltreated children. I will address recent studies examining individual differences in children's memory and suggestibility as well as situational factors that have been found to affect suggestibility in children. This presentation will also cover recent theoretical advances in our understanding of suggestibility effects and false memory formation in children and adolescents. Further, vignettes from clinical case studies will be used to illustrate various types of suggestibility effects in cases involving suggestive interviews and parental coaching. I will also address the effects of chronic stress and trauma on children's memory and suggestibility

In addition to presenting an up to date review of the literature on suggestibility effects in children, this presentation will include a discussion of recent studies examining the quality and consistency of children disclosures of abuse. There is an emerging controversy surrounding what can be expected of children's disclosures of abuse when interviewed on multiple occasions. Recent studies examining patterns of disclosure and recantation will be discussed in context of the extent literature on memory for, and reporting of, experiences of physical and/or sexual abuse. Findings will be discussed in terms of

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psychological theory and practical implications for clinicians and legal professionals who work with maltreated children.

Over the last two decades, controversy surrounding the validity of children's allegations of abuse in legal cases has focused considerable attention on children's memory abilities. Over the years, heated debates have been focused on a few specific areas that have been thought to affect the validity of children's abuse allegations presented in court. Early on, in the 1980s and early 1990s much attention was given to the effects of trauma on children's memory, and debate raged over whether special explanatory mechanisms were needed to describe children's memories for sexual abuse and other potentially traumatic life events. Throughout the early 1990s the focus of applied memory researchers turned to a close examination of what situational factors and dispositional traits would lead to an individual falsely accepting suggestions that they have been abused during forensic interviews; and what processes lead to the creation of false memories over time. Although research continues in these areas, the controversy has died down considerably in recent years, as the scientific community has come to some consensus on the reality of suggestibility effects and clear advances have been made in the development of our understanding of the core mechanisms involved in the acceptance of misinformation.

More recently, a new controversy has emerged among researchers related to what can be expected from children's disclosures of abuse when interviewed on multiple occasions. Some very well known researchers who had lead the way in the debate on suggestibility effects in the early 1990s have recently published two controversial and provocative reviews of the literature claiming that children almost never recant their abuse allegations, and that inconsistent or recanted allegations are likely to be false. This new debate is only starting to brew in the scientific community.

Today I will briefly review the history and current state of the science in each of these latter two areas: (1) Situational and dispositional factors involved in suggestibility effects in children; and (2) Recent emerging controversies related to what can be expected from children's disclosures of abuse when interviewed on multiple occasions. In discussing each of these areas, I will present some of my own data

from studies that are currently in press or under review examining each of these issues. Although I will not comprehensively review the literature on how trauma affects memory, I will describe some of our findings related to how stress and trauma affect memory and suggestibility.

SUGGESTIBILITY

Suggestibility is a fairly broad term that is frequently used in a vague and over generalized manner to explain a wide array of diverse phenomenon. Failure to adequately distinguish between different types of suggestibility effects can do a great disservice to the courts who increasingly rely on experts to explain these phenomenon to the triers of fact during legal proceedings. Since psychologists are frequently called upon to explain these phenomenon to lay people and legal professionals in the courtroom, it is more important than ever to develop a comprehensive understanding of the underlying processes involved in different types of suggestibility effects that can be well articulated and clearly distinguished.

Schooler and Loftus (1993) proposed a useful distinction between two general types of suggestibility effects: Immediate misinformation acceptance and delayed misinformation retrieval. Schooler and Loftus noted that immediate misinformation acceptance involves the immediate acceptance of inaccurate presuppositions in misleading questions. In contrast, delayed misinformation retrieval corresponds to whether an individual incorporates the misinformation into subsequent memory reports.

Delayed Misinformation Retrieval Errors

Studies designed to create delayed misinformation retrieval errors have generally used the 'classic' three-stage misinformation paradigm (Loftus, 1975; Loftus & Zanni, 1975; Loftus, 1979; see Loftus, 1993; & Hyman & Loftus, 2001 for recent reviews). In the first stage of this procedure, participants read, listen to, or observe a target event. The second stage involves the introduction of misinformation. Minutes, days, or weeks after presenting the target event, participants are presented

suggestively misleading information. Later, in the third stage, participants are questioned about their memory for the target event.

Subsequent variations of the classic misinformation paradigm designed to induce delayed retrieval errors vary the manner in which the misinformation is imparted. Common variations of this paradigm involve introducing the misinformation through repeatedly asking suggestive questions to participants (see Poole & White, 1995 for a review), having participants imagine a false childhood event on a single occasion (Garry, Manning, Loftus, & Sherman, 1996), or repeatedly visualize and imagine experiencing a false childhood experience (Garry & Loftus, 1994), suggesting misinformation during hypnosis (Laurence & Perry, 1983), or having a confederate co-witness share an inaccurate account of the crime with the participant-witness (Luus & Wells, 1994; Gabbert, Memon & Allen, 2003). Classic studies with children looking at delayed retrieval errors would include Ceci's Mousetrap study and Sam Stone experiment, or Poole's Mr. Science study.

Immediate acceptance of misinformation

Studies examining the immediate acceptance of misinformation are designed to assess how an individual's 'memory report' might be affected by excessively biased and suggestive questioning techniques that are often employed during forensic interviews. In these studies, the participant typically takes part in a staged event, and then sometime later (minutes, hours, days, or weeks later) they are interviewed on a single occasion in a highly suggestive manner. Reporting errors in this paradigm do not necessarily involve memory alterations, and can often be the result of the social demands of the interview (Belli, 1989; McCloskey & Zaragoza, 1985).

This type of suggestibility effect is frequently examined in children because it is so central to understanding childrens' response to suggestive forensic interviewing techniques. This effect has also been examined in adults (e.g., Eisen & Carlson, 1998; Eisen, Morgan, & Mickes, 2002; Gudjonsson, 1986, 1987, 1988, 1990, 1992a; Roebers & Schnieder, 2000), and is relevant in understanding coerced confessions obtained during police interrogations (Gudjonsson, 1989, 1992b; Gudjonsson & Clark, 1986; Kassin, 1997). Gudjonsson and his colleagues have identified several individual difference factors that are

related to this general type of suggestibility effect, including but not limited to acquiescence (Gudjonsson, 1990; Gudjonsson & Clark, 1986), short-term memory (Gudjonsson, 1987), assertiveness (Gudjonsson, 1988), locus of control (Gudjonsson & Lister, 1984), and intellectual ability (Gudjonsson, 1990). Other investigators who have used variations of this approach, have also found relations between errors on misleading questions and dissociation (Eisen & Carlson, 1998; Wolfradt & Meyer, 1998; Merckelbach, Muris, Rassin, & Herselenberg, 2000), fantasy proneness (Merckelbach, Muris, Schmidt, Rassin, & Herslenberg, 1998), age effects (Roebers & Schneider, 2000), and other individual difference factors (See Pipe & Salmon, 2001; Eisen et al., 2001; for recent reviews).

Individual differences in Memory and Suggestibility among Maltreated children

When considering memory reports of maltreated children, clinically significant factors are of special interest. Child maltreatment places individuals at increased risk of such trauma-related psychopathology as dissociative tendencies and posttraumatic stress disorder (PTSD) symptoms, which are associated in adults with a multitude of memory deficits and with increased suggestibility (e.g., Bremner, Shobe, & Khilstrom, 2000; Hyman & Billings, 1998). These clinically relevant factors have been hypothesized to affect children's event memory and vulnerability to false suggestion as well (see Eisen & Goodman, 1998, and Eisen & Lynn, 2001, for reviews). Further, on standardized tests, maltreated children, on average, often display language delays and deficits in intellectual abilities (Culp, Watkins, Lawrence, Letts, Kelly, & Rice, 1991; Egeland, 1991: Gaudin, 1999), which have been identified as potential influences on children's event memory and suggestibility (e.g., Alexander et al., 2003; Dent, 1992; Quas, Goodman, Bidrose, Pipe, Craw & Ablin, 1999).

Our Studies

Memory, suggestibility, stress arousal, and trauma-related psychopathology were examined in 328 suspected victims of child

maltreatment. This study was designed to examine memory and suggestibility in children with substantiated maltreatment histories, as well as in a control group of children without known histories of abuse or neglect. It was also designed to investigate individual-difference factors hypothesized to be related to children's event memory. Specifically, we examined memory for three distinct events and sought to determine if individual differences in intelligence-related measures (intellectual ability, language comprehension, short-term memory) and psychopathology (dissociation, depression, PTSD, general psychological functioning) were related to various indices of event memory (performance on free recall, open-ended, specific, and misleading questions) and face identification (identifications in target-present and target-absent line ups) for children aged 3 to 16 years. Testing occurred in the context of official forensic investigations of abuse and neglect. Children's memory and suggestibility were assessed for three events: Anogenital examination/ venipuncture, clinical interview, and play activity. This was a replication of Eisen et al.'s (2002) research, with several important additions and methodological advances. Most notably, we significantly improved the methodology employed in the previous study to examine possible relations between stress and memory. For example, we examined Hypothalamic-Pituitary-Adrenal (HPA) responsivity (i.e., cortisol), together with heart-rate measures and observer and self stress judgments

Age and Event Memory

Many studies in this area have found that younger children tend to have poorer memory abilities compared to their older peers, and more importantly, are more suggestibility. Specifically, most studies in this area have found that preschool children are particularly suggestible compared to school age children or adolescents (See, Ceci & Bruck 1993 for a review).

Findings. As predicted, older children showed better event memory and greater resistance to misleading questions than did their younger counterparts when questioned about an anogenital exam and blood draw, a play event, and a clinical interview. In general, the older

children were more likely to provide correct answers and less likely to make errors (omission and commission errors). The increase in accuracy and resistance to misleading information from preschool to elementary school years has been well documented in previous research. What is less well documented is the decrease in inaccuracy with age when 6- to 10-year-olds are compared to adolescents. In our studies we found that school aged children were more suggestible than the adolescents.

Stress and Children's Memory

For adults, studies largely converge on the notion that information relevant to the main stressor, as opposed to more peripheral information, is retained particularly well (Christianson, 1992; Reisberg & Hertel, 2004). However, for children, findings are more mixed. Some studies lead to the conclusion that stressful events are clearly impressed upon children's minds, resulting in particularly accurate memory reports (e.g., Goodman, Hirschman, Hepps, & Rudy, 1991; Peterson & Bell, 1996; see Pezdek & Taylor, 2001, for review), although the memories are still subject to normal memory processes such as forgetting. Other studies suggest that stress and trauma result in poorer memory for the details of the event and increased susceptibility to suggestive questions (e.g., Fivush, McDermott Sales, Goldberg, Bahrick, & Parker, 2004; Merritt, Ornstein, & Spicker., 1994; see Eisen & Lynn 2001, for review).

However, like adults, children do not all respond in the same way to highly stressful or traumatic experiences (e.g., Stewart et al., 1996). These differential responses to stress and trauma are likely moderated by a host of individual-difference factors, for instance in psychopathology, that can influence children's memory for highly stressful events, perhaps more so than they do for non-stressful events (Alexander et al., 2002; see Eisen & Goodman, 1998, for review).

Findings. It was expected that children would have better memory for central details of an experience than for peripheral details. As expected, the two older age groups showed better eyewitness memory performance for central information than for peripheral information in response to specific questions about the anogenital exam and in

response to misleading questions about the neutral event, whereas this was not the case for younger children. It may be more difficult to predict what a young child focuses on as "central" to their experiences.

In addition, Bivariate correlations indicated that children who were more distressed during the anogenital exam/venipuncture were more suggestible (i.e., made more commission errors to misleading questions).

Abuse related questions

Errors to the abuse-related questions are of particular forensic concern. For purposes of our study we designed questions to be similar to the types of questions asked in actual sexual abuse investigations, and they generally involved memory for genital touch and undressing. Asking suggestive abuse-related questions about the anogenital exam becomes a particularly good analogue to certain actual forensic interviews, especially given the study's context within an ongoing abuse investigation. By the time we interviewed the children about their memory for the exam, the children had been repeatedly questioned over an extended period of time, being asked whether friends, family members, or strangers touched them inappropriately or attempted to hurt them in anyway. Therefore, this mode of inquiry had been essentially normalized for these children. Asking them about possible transgressions by the doctor and/or nurse was arguably not outside reasonable expectations.

Findings. Consistent with Eisen et al.'s (2002) previous report, errors on the abuse-related questions were quite low across all groups. Although the youngest children made significantly more errors on these questions than did children in the two older groups, these preschoolers made commission errors at a rate of only 7%, on average (and thus a 93% rate of resistance to the leading and misleading abuse questions). Across all ages, commission errors to the abuse-related questions occurred at quite low rates (4%). Considering the fact that many of these children in this special population showed developmental delays and poor receptive language skills, this small number of errors on complex, tricky, albeit at times blatantly, misleading questions was impressive.

Individual Differences in Cognitive Functioning

Language ability. In the forensic context, language ability plays a key role in determining a young child's ability to understand questions and to respond in an appropriate and coherent manner. Given the fact that child maltreatment is often associated with language delays (Culp et al., 1991; Egeland, 1993), it is important to investigate the relation between language abilities and memory/suggestibility in a child maltreatment sample.

Intelligence and short-term memory. Maltreatment is associated with lower scores on intelligence tests (e.g., Gowan, 1993). Again, children who suffer neglect may be especially at risk of lower scores on intelligence tests and with developmental delays in cognitive ability generally (Gaudin, 1999). Because intelligence and cognitive abilities may influence eyewitness memory skill, it is important to examine relations between intelligence and memory in maltreated samples. When studies on nonmaltreated samples have examined relations between intelligence and eyewitness memory, significant relations emerge when individuals are included who have intelligence test (IQ) scores indicative of retardation (e.g., Dent, 1992; Henry & Gudjonsson, 2003). Moreover, short-term memory capacities are typically tapped on IQ tests, and such capacities could in theory be related to memory performance more generally (e.g., as an indication of general memory capacity). Bennet and Pipe (1999) recently found that children's digit span scores correlated with recall of a staged event.

Findings. Children were administered measures of short-term memory, intelligence, and receptive language ability. As a group, the sample scored rather low on each of these measures relative to published standardized norms. It is likely that this poor performance is due to a lack of appropriate stimulation at home, as the majority of the sample came from neglectful and/or traumatizing environments. As expected, performance on measures of short-term memory, IQ, and receptive language ability were related and thus formed our cognitive functioning factor. When event memory across the three events was

considered, cognitive functioning (with age statistically controlled) predicted correct responses to free recall and open-ended questions, and commission errors to specific and misleading questions.

Psychopathology, Stress, and Memory

A major intent of the study was to examine possible associations between trauma-related psychopathology and memory performance in maltreated children. Because the self-report measures of trauma symptoms (A-DES, CPAS, TSC-C, CDIS) were intercorrelated, we combined them into a self-report trauma symptoms factor. In addition, we considered individually the other-report measures of psychopathology, specifically, CDC scores, PTSD diagnosis, and GAF ratings.

Psychopathology and memory. Eisen et al. (1998) noted that general psychopathology could affect children's event memory in a variety of ways. For example, concerning encoding, to the extent that children with general emotional problems evince attention deficits, they could encode the details of events more poorly and therefore show problems in event memory. Similarly, to the extent that depression is a characteristic of some children's general psychopathology, such children may also be less attentive to outside events as they unfold which could have an adverse effect on their ability to encode the details of experienced events. Further, general psychopathology that produces states of high anxiety could lead to children becoming easily overwhelmed by external stressors, and this elevated stress could limit the information encoded, perhaps especially during highly stressful and/or traumatic events. In addition to these problems with encoding, Eisen and colleagues proposed that psychopathology can also affect children's ability to recall information during memory interviews. For instance, disturbed children might have more problems handling lengthy, challenging memory interviews, perhaps especially ones in which they are asked to recount the details of difficult, embarrassing, and/or traumatic events. In the present study, we attempted to replicate Eisen's findings by examining the relation between GAF scores and

memory for stressful and non-stressful events. It was predicted that children with higher levels of psychopathology would perform more poorly than more psychologically healthy children on the event memory tasks.

Findings. Consistent with our previous work, we uncovered a significant relation between general psychopathology, as measured by clinicians' ratings of global adaptive functioning (GAF), and maltreated children's errors on misleading questions following a stressful medical procedure.

PTSD and memory

It has been proposed that dissociation is related to the development of PTSD, which is one of the most common diagnoses given to traumatized children (Browne & Finkelhor, 1986). The core processes involved in PTSD are hypothesized to be the failure to integrate trauma-related information into existing memory structures (Foa & Kozack, 1991; Horowitz, 1986; see Dalgleish, 2004; McNally, 2003; Pynoos et al., 1995, for reviews). PTSD can result in re-experiencing symptoms (intrusive and distressing memories, thoughts, mental images, dreams, and flashbacks related to the traumatic event), avoidance and numbing symptoms (social withdrawal, avoidance of trauma-related cues, thought stopping, amnesia for the trauma), and hyperarousal symptoms (irritability, hypervigilance, problems with concentration, sleep problems) (American Psychiatric Association , 1997).

Although PTSD is associated with adults' memory performance (e.g., Alexander et al., in press; Bremner et al., 2000; Qin et al., 2003; Zoellner et al., 2000), and has been defined as a disorder of memory (Sullivan & Gorman, 2002), there is a paucity of research addressing the effects of PTSD on children's memory. Consistent with findings in the adult literature, Moradi et al. (1999) reported that children and adolescents with PTSD showed poorer overall memory performance when compared to children with no evidence of psychopathology. However, Beers and De Bellis (2002) failed to uncover differences in memory ability between a sample of maltreated children with PTSD and a

matched sample of children without psychopathology, although differences in attention and abstract reasoning were revealed.

Findings. In our study, PTSD diagnosis was unrelated to memory performance or suggestibility. The lack of relations might reflect the need to have a continuous measure of PTSD symptoms as opposed to a dichotomous measure, such as PTSD diagnosis, to detect PTSD-memory associations (Alexander et al., in press). Alternatively, clinicians' diagnoses of PTSD in children may have been unreliable. Finally, it is possible that relations between PTSD and memory emerge in adulthood but are not evident in childhood.

Dissociation and memory

Dissociation is frequently cited as a common response to trauma in maltreated children (see Putnam, 1996, for a review). Theoretically, the child dissociates as a defensive response to psychologically remove her or himself from the traumatic situation. This results in a form of cognitive avoidance (Carlson et al., 1997) designed to protect the child from overwhelming stressors. It is believed that such responses of, in effect, "zoning out," may become enduring personality or coping tendencies in times of stress (Koopman, Classen, & Spiegel, 1994; Lynn & Rue, 1994). If so, maltreated children who remain high in dissociative tendencies may become more dissociative during a (nonabusive) stressful event and thus evince worse memory and greater suggestibility than would maltreated children who show less elevated dissociative tendencies. Such tendencies should be less likely to affect memory for nonstressful events, although a more general encoding/memory deficit is possible.

In adults, dissociation is associated with various forms of memory impairment. For instance, a number of investigators have reported a positive relation between dissociation and adults' suggestibility (Eisen & Carlson, 1998; Hymen & Billings, 2001; Paddock et al., 1999; Winograd, Peluso, & Glover, 1999; but see Eisen, Morgan, & Mickes, 2002). Dissociation in adults is also related to lost memory/lack of disclosure of childhood abuse experiences (Goodman et al., 2003). The only study

we could find that examined dissociation and suggestibility in children failed to find significant relations (Eisen et al., 2002).

Findings. As predicted, dissociation, as measured by the CPAS, was significantly related to children's memory for the anogenital exam/venipuncture. Specifically, for children who were highly dissociative, commission errors regarding the anogenital exam/venipuncture were predicted by cortisol response and self-reported trauma symptoms. Thus, being a child who was highly dissociative and who mounted a stronger physiological (cortisol) response, presumably indicating greater distress, was associated with greater memory errors and suggestibility later. At the same time, more trauma symptoms predicted fewer omission errors for the highly dissociative children.

CONSISTENCY OF CHILDREN DISCLOSURES OF ABUSE

When considering suggestibility effects, our concern is of course with commission errors: That is, saying something happened (the abuse) that in fact did not occur in reality. However, in any given case, omission errors could also be quite important, as not all children will consistently report their abuse experiences when asked in an appropriate and non-suggestive manner. This is an area where considerable debate is emerging in the scientific community. Before getting into the details of this debate, lets us first deal with the issue of what is known about the consistency of children's disclosures over time.

Multiple interviews

When reviewing the literature on suggestibility, we discussed studies that showed the impact of repeated suggestive interviews. However, what can we expect from a child when they are interviewed on more than one occasion in an appropriate and non-suggestive manner?

It is important to note that in virtually all legal investigations of this type, the child is interviewed on more than one occasion (e.g., Goodman et al., 1992). Frequently, the allegation starts with disclosure

to a relative, friend, or teacher, and this initial report is followed-up with subsequent interviews by patrol officers and specialized child abuse investigators. If the case is adjudicated in the dependency/juvenile and/or criminal courts, the child may be re-interviewed by investigating officers and attorneys. Questions have been raised about how common it is for a child to disclose the abuse during one interview and then either to refuse to talk about it when the child is re-interviewed, or to recant the allegation all together. Thus, it is important to understand typical rates of consistency in children's disclosures of abuse across repeated interviews.

Mock juror studies and studies of actual jurors indicate that inconsistent compared to consistent child witnesses are judged to be less believable (Berman, Narby, & Cutler, 1995; Leippe, Manion, & Romanczyk, 1992; Myers, Goodman, Redlich, Prizmich, & Imwinkelried, 1999). However, despite the fact that consistency is used as an index of believability by jurors, there is a wide variety of reasons why children may not be consistent in their disclosure of abuse across interviews, including; fear of retaliation by the abuser, fear of not being believed, denial, protecting the abuser, limited understanding of the point of the interview, or simply not wanting to talk about the incident with a particular interviewer at a given moment in time.

Two Types of Inconsistency

Detail inconsistency. It is important to note that there are different types of inconsistency to be considered when evaluating a child's disclosure. One type of inconsistency that is frequently studied is related to inconsistency of details within children's narrative reports provided across various interviews. This type of inconsistency involves the child reporting different, but not necessarily contradictory elements of an event across interviews. As Fivush (1993) noted, children often report different features of an event across interviews in part because the children are simply responding to the demands of a particular interviewer and answering different questions. To demonstrate this phenomenon, Fivush and Shukat (1995) questioned children about a trip to Disney World on two occasions. They found that although 90% of the information recalled across the two interviews about an event was correct, only 10% of the information was consistently reported in

both interviews. In this case, the children were not often actually contradicting themselves, but were more frequently just reporting new information about the experience. In general, when children (or adults) are interviewed on multiple occasions about events they have experienced, in the best of circumstances, one would expect that the gist of their memory reports would be consistent, and that various minor additions and omissions would occur, depending, for example, on the focus of the discussion and the cues offered by the interviewer (Poole & White, 1995). Because children compared to adults are more reliant on the interviewer for cues and direction as to exactly what is being asked of them, we can expect variation in children's reports to be more closely tied to the questions asked by interviewers across various interview sessions.

Gist inconsistency. What happens when a child is directly asked about his/her experiences of abuse on more than one occasion, and the fundamental gist of the child's report changes from one interview to the next? Reporting different yet non-contradictory elements of an experience across two or more interviews is clearly less of a concern to investigative interviewers and legal authorities than inconsistencies that involve gist contradictions, recantations, or intermittent denials. Because most children are interviewed on several occasions over the course of an actual investigation, children may report the abuse at one time and then deny it during a subsequent interview. This type of gist inconsistency presents a great concern to forensic investigators. If a child initially refuses to disclose abuse, this is taken as a denial. If a child initially discloses and then denies the abuse in a subsequent interview, that is taken as recantation. Either way, gist inconsistency has the potential to hamper the prosecution of a criminal case. Arguably the best-known and most influential paper published on this type of inconsistency in children's abuse reports is Roland Summit's (1983) article on Child Sexual Abuse Accommodation Syndrome (CSAAS).

Child Sexual Abuse Accommodation Syndrome

Summit's (1983) paper on CSAAS was essentially a description of clinical observations made by Roland Summit and others describing why many children may be delayed and/or inconsistent in their abuse

disclosures. Summit noted that in his many years of consulting and clinical practice, he frequently encountered cases where children would make delayed and/or inconsistent disclosures of their experiences of abuse. Summit stated that these children were often hesitant to disclose the secrets of their sexual abuse because they felt helpless in the face of adult authority. As a result of this helplessness, they would frequently go along with the abuse (accommodate) for extended periods of time without telling anyone. Summit observed that if and when such children decided to disclose, many of them provided inconsistent reports of their experiences, as they carefully checked to see how the information was being received by the adults in their lives. Summit added that it was not uncommon for children to retract their allegations all together when the disclosures were not well received by family members or other key adults.

In the US, Summit's original paper garnered a great deal of attention in the community of mental health practitioners who specialized in dealing with allegations of sexual abuse. Bolstered by Summit's observations, in the years following the publication of his paper, mental health practitioners in the US frequently offered expert testimony on CSAAS evidence in court proceedings (Heath, 2000; Summit, 1992), informing jurors that this type of inconsistency in children's reports was not only common, but consistent with what is typically seen in abused children. As Summit (1992) noted, CSAAS has been misused in court by experts asserting that delayed and/or inconsistent reports somehow confirm the child was in fact abused, as if not telling makes the child a more typical case of actual abuse. CSAAS testimony is still offered in court cases where the child delays his or her report or provides inconsistent disclosures (i.e., recants or denies the abuse on one or more occasions). Although there is little empirical data to support this model, in most states in the US, CSAAS testimony is allowed as rebuttal to counter the proposition that not telling right away, or being inconsistent in one's report, invalidates the child's disclosure altogether (Lyons, 2002; Myers, 1992).

Research Examining Delayed Disclosure

Although there is widespread agreement that many children do not disclose sexual abuse right away, and often keep these experiences a

secret into adulthood (Hanson et al., 1999; Lamb, Edgar & Smith, 1994; Smith et al., 2000; see London et al., 2005, for a recent review), there is considerable debate over how consistently children, during a forensic investigation, will disclose sexual abuse when asked directly about their experiences. This issue has been addressed in several published studies that involved reviews of case files at clinics that specialize in the assessment of allegations of abuse (Bradely & Wood, 1996; Cantlon et al., 1996; Elliot & Breire, 1994; Greis et al., 1996; Gordon & Jaudes, 1996; Keary & Fitzpatrick, 1994; Lawson & Chaffin, 1992). Rates of disclosure as assessed through these case file reviews vary greatly, with some studies indicating that less than half of their participants disclosed known abuse, while others report that most all children disclosed when asked directly. Overall, young compared to older children seem less likely to disclose abuse when interviewed in formal settings (e.g., Keary & Fitzpatrick, 1994).

The emerging controversy

London et al. (2005) argue that when only methodologically superior studies are considered (those studies that only include children *with substantiated cases of sexual abuse* and who were apparently not subjected to highly suggestive interviewing techniques), most children disclosed abuse during the first or second interview. Also, in a recently published paper, Bruck and Ceci (2004) proposed that "...it is a common misconception that sexually abused children do not disclose when asked directly." Specifically these authors assert that children will tell about their abuse if asked directly, and that models such as CSAAS overstate the issue of children's inconsistency or likelihood of recanting. In fairness, Ceci and Bruck make a valid point, that if we assume children will not always tell, interviewers may push to hard with directed or leading questions, which may result in increased risk for suggestibility effects. Still, claiming that children will generally tell when asked directly may not be entirely accurate, and may in fact overstate the data. If this argument were accepted on it's face, this could lead to a blanket dismissal of all allegations that are not made in a consistent manner by the children involved.

Problems with London's analyses

Although London et al.'s review provides a step forward beyond the existing published data, London's analysis is also quite limited. Although these case file reviews provide the best data available to examine rates and patterns of disclosure, reliance of clinical records in general can be problematic. Lyon (2002) points out that for legal reasons, investigators and clinical interviewers may not make note of denials and recantations (see also Bradley & Wood, 1996). In fact, bias is quite common in clinical evaluations, as these records are frequently written to support and protect the patient in an effort to get children needed services and to insulate them from perceived threats (e.g., returning to the home of a suspected abuser).

Our recent data on consistency

In a recent study, we had the unique opportunity to record the forensic interviews as they occurred, thus avoiding biases that are often inherent in relying on the notes and reports of clinical investigators and caseworkers. Specifically, we were interested in how frequently children are consistent in reporting abuse when asked directly across repeated interviews (i.e., gist consistency). Further, we examined frequency and consistency of disclosures in relation to individual differences in age, gender, cognitive functioning, and trauma-related psychopathology.

Two hundred ninety-two 3- to 16-year-olds were interviewed twice over a 5-day period as part of an ongoing forensic evaluation. Three specific hypotheses were set forth. First, we predicted that younger children would be less likely than older children to disclose and also less consistent in their disclosures of all three types of maltreatment we studied (sexual abuse, physical abuse, and neglect). Second, we predicted that males would be less likely to disclose substantiated reports of sexual abuse and more inconsistent in their disclosure of sexual abuse across interviews. Third, we predicted that children with higher levels of trauma-related psychopathology (i.e., symptoms of PTSD, dissociation, and depression) would be more likely to disclose maltreatment and be more consistent in their disclosures across the two

interviews. In addition, we examined relations between the frequency and consistency of children's disclosures and measures of children's cognitive ability (i.e., short-term memory, intellectual ability, and receptive language skills).

Disclosure of Sexual Abuse

As predicted, our data indicate that when children with substantiated sexual abuse were asked directly about their experience of abuse on two separate occasions, females were more likely to disclose sexual abuse than males. Overall, 81.3% of our sample of sexually abused females disclosed sexual abuse in one or both of the interviews compared to 61.9% of the males. A closer examination of the data revealed that the gender differences were not evident in the pre-school group (3- to 5-year-olds) and were restricted to the older children. For school-aged children (6- to 10-years of age) and adolescents, only 44.4% to 50% of the boys who had been sexually abused disclosed this when asked about these experiences in two different interviews. In contrast, 82.1% to 90% of the girls in these same age ranges disclosed sexual abuse in one or both interviews. It may be easier for females to discuss sexual abuse in general, since sexual abuse of females usually involves heterosexual interactions, whereas sexual abuse in males usually involves homosexual activity that may be more embarrassing and difficult to discuss with others (De Voe & Faller, 1999; Goodman-Brown, Edelstein, Goodman, Jones, & Gordon, 2003; Spatz-Widom & Morris, 1997). This would account for the lack of gender differences in disclosure among the younger children, as the 3- to 5-year-old boys may not have yet learned about issues of homosexuality or learned that sexual abuse is particularly embarrassing.

Despite their greater willingness to discuss experiences of sexual abuse, still 20% of the girls did not disclose the abuse in either of the interviews. Although 80% disclosure across two interviews seems promising, one in five girls with substantiated sexual abuse did not disclose even when interviewed on two separate occasions.

Consistency of Sexual Abuse Disclosure

As predicted, older children were more likely to consistently disclose sexual abuse in both interviews than were their younger

counterparts. The 11- to17-year-olds were perfectly consistent in their disclosures across the two interviews, compared to the 6-to 10-year-olds who had a 76.9% consistency rate, and the 3- to 5-year-olds who had a 62.5% consistency rate. It is important to note that these interviews were conducted only a few days apart within the same clinical context. Also, the children were in an inpatient setting and did not have any unsupervised contact with their parents and relatives that might have influenced their reporting from one interview to the next. Still, despite these controls, nearly a quarter of the school-aged children and more than a third of the preschool children were not consistent in their disclosures of sexual abuse across the repeated interviews.

Our data indicate that among the 68.8% of 3- to 5-year-old children who disclosed sexual abuse in either interview, only 45.5% of them consistently disclosed the abuse in both interviews. These data have great practical significance, as almost half of the very young children with substantiated experiences of sexual abuse who disclosed in at least one of two interviews, either recanted their disclosure, or failed to acknowledge their sexual abuse experiences consistently across two interviews. It may therefore be necessary to interview younger children on more than one occasion to maximize opportunities for disclosure.

A close examination of the data shows that for the 3- to 5-year-olds, 37.5% of those children who did not disclose substantiated sexual abuse initially, disclosed when they were re-interviewed. The fact that about one in three children in the youngest age group needed two interviews to disclose substantiated experiences of sexual abuse should be considered when weighing the costs and benefits of repeated interviews. When interviews are conducted carefully in a manner that minimizes suggestive questioning techniques (Sternberg et al., 2001), the risks associated with the interview per se of repeating the procedure should be relatively minimal, while the benefits could be substantial. However, it should also be noted that interviewing a second time could increase to a certain extent the risk of false negative denials that might ultimately reduce the likelihood of the case being successfully prosecuted.

Child Sexual Abuse Accommodation Syndrome. Although these findings show that is it not uncommon for children to be inconsistent in

their disclosures of sexual abuse, these data should not be taken as direct scientific support for the existence of CSAAS as described by Summit (1983). Specifically, our data do not address the underlying processes described in Summit's description of CSAAS, such as shame, fear, accommodation and concealment. Further, our data do not directly address the incremental pattern of disclosure in Summit's model or the issue of recantation. In essence, we do not know why these children did not disclose their experiences of sexual abuse consistently when asked directly. It may be that some children did not feel like talking about these issues with a particular interviewer at a given point in time. Or alternatively, a child may deny abuse, or be inconsistent in disclosure due to personal issues of shame and fear as described by Summit. This type of speculation goes beyond our data. What our findings do show is that high expectations of gist consistency in disclosures of sexual abuse among young children need to be scaled back considerably. For one reason or another, many young children did not reliably report substantiated experiences of sexual abuse from one interview to the next, and males were more likely to deny substantiated reports of sexual abuse than acknowledge them.

Individual Differences in Trauma-Related Psychopathology

Disclosure. We were also interested in relations between trauma-related psychopathology and willingness to disclose abuse. Children who disclosed CSA showed lower levels of adaptive functioning, as indexed by their GAF scores. Although none of the other trauma-related psychopathology measures stood alone as significant predictors of abuse disclosure, without exception, children who disclosed sexual abuse and/or neglect on average showed higher levels of disturbance on each and every one of our measures of trauma-related psychopathology. This consistent pattern of findings across the different types of maltreatment is congruent with data reported by Elliot and Briere (1994) who also found that more disturbed children were more likely to disclose abuse. It is possible that children who scored higher on measures of psychopathology had more maltreatment experiences to report. There may also be a pathology bias in abuse reporting, where the more disturbed children were more likely to report abuse during clinical or forensic interviews. However, it is important to note that

although this finding was consistent across all indices of psychopathology, the relations were not statistically significant. Thus, extensive speculation on the meaning of these trends may be unwarranted at this time.

Consistency of disclosure. Although consistently disclosing confirmed experiences of sexual abuse or neglect across the two interviews was not significantly related to any of our measures of trauma-related psychopathology, consistently disclosing verified instances of physical abuse across the two interviews was significantly associated with more symptoms and/or higher levels of disturbance on all of our indices of trauma-related pathology (although not all of those differences were statistically significant). Again, it may be that the most distressed children are more willing to discuss abuse and are generally more cooperative over all phases of the assessment process. Or, these children may be in more need of assistance, and their acknowledgement of abuse serves as a cry for help. Alternatively it is possible that the more disturbed children lacked the ego defenses to hold out and deny abuse in the face of repeated inquiries by the authorities. This would run counter to the idea that more disturbed children may be more erratic in their reporting and less consistent in their disclosures.

Individual Differences in Cognitive Ability

Children who disclosed sexual abuse, physical abuse, or neglect did not differ from children who did not disclose in regard to short-term memory, receptive language skills, or intellectual ability. Similarly, no significant relations between cognitive ability and consistency in reporting any of the forms of maltreatment emerged. These data suggest that disclosing maltreatment is not tied to language skills, intellectual ability, or short-term memory, at least for children who possess the types of language skills and cognitive abilities evident in our sample.

MEMORY AND ESP: A REVIEW OF THE EXPERIMENTAL LITERATURE

John Palmer*

Introduction

As first noted explicitly by G. N. M. Tyrell (1946-1949), the ESP process can be divided into 2 stages. Stage 1 concerns how the information gets from the source to the receiver, and Stage 2 refers to how it is processed by the mind once it gets to the receiver. Stage 1 is primarily the domain of physics, whereas Stage 2 falls under psychology. Most process-oriented parapsychological research has concerned itself with Stage 2, partly because most parapsychologists have been trained as psychologists, but also because the research questions in this domain are at present more amenable to scientific investigation. It is considered axiomatic that when an ESP signal reaches an organism, it undergoes cognitive processing that restructures the stimulus and often distorts it. If we are ever to gain a full theoretical understanding of psi, as well as improve its accuracy and reliability, it is essential that we understand these cognitive mechanisms in some detail.

It is reasonable to suppose that the cognitive processing of ESP stimuli involves memory to at least some degree. Parapsychologists' interest in a possible memory/ESP relationship was inspired by a theory of William Roll (1966) that ESP consists of the activation of memory traces. If this view is correct, memory plays not only a role in the cognitive processing of ESP, but a very central role. As is the case with most research in the psychology of psi, the methodology applied to the ESP/memory relationship has been primarily correlational. More specifically, scores on tests of memory have been correlated with scores on forced-choice ESP tests that are related to, and sometimes embedded

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in, the memory test. Various characteristics of these tests have often been manipulated to gain further insight into the process and sometimes to explicitly test inferences from Roll's theory.

The First Experiment

The first published experiment to compare the results on tests of memory and ESP was conducted at the Foundation for Research on the Nature of Man (FRNM), the heir to J. B. Rhine's famous Duke Parapsychology Laboratory, by Rhine's daughter, Sally Feather (1967). The exploration began quite by accident. In an effort to induce frustration in order to obtain below-chance scoring on a standard ESP card-guessing test, Feather gave her participants only 15 seconds to memorize a randomized list of 25 of the standard ESP symbols immediately before the guessing task. To her surprise, she found that the memory and ESP scores were positively correlated ($p < .04$), but these results were not reported in detail. She followed this up with 3 additonal series, in each of which 10 participants, mostly high school students, were tested individually. Between 2 sets of 2 or 3 25-trial card-guessing runs, participants were again given 15 seconds to memorize a sequence of 25 of the same ESP symbols used in the ESP test, but in a different order. Finally, they were asked to recall the ESP symbols they had attempted to memorize. The correlations between the ESP scores and the memory scores were positive in all 3 series, and the combined result was significant ($p = .012$). As noted by Rao, Morrison, and Davis (1977), the short time duration for the memory test could mean that it functioned more like an ESP test, in which case the positive correlation would be attributable to the fact that the 2 tests were essentially equivalent.

ESP, Recall, and Association Strength With Paired Associates

The Kanthamani Experiments

Several years later the thread initiated by Feather was picked up by another investigator at FRNM, the Indian psychologist H. Kanthamani. Her experiments differed from Feather's in 2 fundamental respects. First, paired-associate learning was substituted for the simple serial learning

task used by Feather. Second, the memory-ESP association was based on the trial rather than the run, to better reflect the "possible interaction between the two processes" (Kanthamani & Rao, 1974, p. 287). It was predicted that ESP scoring would be better on those trials in which the associate was remembered correctly than on those trials in which it was remembered incorrectly or not at all. A preliminary experiment, used to refine the details of the methodology, was followed by a pilot series and 2 confirmatory series. In the preliminary series, 6 participants were given from 3.5 to 6 minutes to memorize a list of 20 word pairs that were the alternative English meanings of 20 Telegu words also listed on the sheet. Following a 2-3 minute interference period, participants were given a sheet containing only the Telegu words and asked to write down both English equivalents. Participants made their ESP guesses by circling which of their 2 memory responses they thought was the randomly chosen ESP target for the trial. The target was really the location of the word in the pair (first or second), not the word itself. During each trial, the experimenter attempted to transmit the correct target telepathically from another room. ESP results were significantly below chance on recall-correct (RC) trials and lower than the scores on recall-wrong (RW) trials, opposite to the prediction.

A major change was introduced for the pilot series, in that the Telegu words were eliminated entirely and the 16 participants, all high school students, were given one of the two English words as the cue stimulus during recall. As this was intended to make memory easier, the learning time was reduced from 3.5 minutes to 3 minutes, and there was no sender. The RC/RW effect reversed, with significantly above-chance scoring on the RC trials and only slightly above chance scoring on the WR trials, thereby confirming the prediction. This result will hereafter be referred to as the RC/RW effect.

The 2 confirmatory series, each with $N = 20$ high school students, differed from the pilot series and from each other in the exact way participants made their ESP responses. In the first confirmatory series, the ESP target was defined as whether participants wrote their guess of the correct associate in capital or small letters. When no guess could be made, the letters C (or c) or S (or s) were substituted. In the second confirmatory series, the ESP targets were defined as on which of 2 adjacent lines participants wrote their guess of each associate. In both

series, ESP scoring was significantly positive on the RC trials. In the first series, the WR trials were very close to chance and in the second series they were significantly negative and significantly lower than the RC trials, supporting the hypothesis.

In 2 additional series, each with 20 high school students, the authors varied the association strength of the associates (Kanthamani & Rao, 1975). Association strength refers to how closely related the words are in meaning, but the relationship need not be similarity. For instance, antonyms (e.g., hot-cold) would also have high association strength. In the previous series, the associates, being the two meanings of Telegu homonyms, were generally if not exclusively of low association strength. The experimental task again consisted of 20 paired associates, which participants were given only 1 minute to memorize, compared to 3 minutes in the confirmatory series of Kanthamani and Rao (1974). The first member of each word pair was a trigram that would be meaningful to the participants (e.g., USA), whereas the second was a word. For half the trials, the association strength between the trigram and the word was high, whereas for the other half it was low, as rated by a sample of 10 different high school students. The order of trial types was randomised within each list of 20 pairs. In Series 1, as expected, scores on the RC trials were significantly above chance, and significantly higher than on the nonsignificant RW trials. In Series 2 the overall mean shifted in the negative direction, but the ratio of scoring on the RC and RW trials remained about the same: scores on the RW trials were significantly negative and significantly lower than scores on the nonsignificant RC trials. In both series, the RC/RW effect was driven by the low-association pairs, for which the RC trials were significantly positive in Series 1 and the RW trials significantly negative in Series 2. The RC-RW difference was significant in both series. Thus, in this sense the hypothesis was confirmed, although it is awkward that the only significant evidence for psi in Series 2 (i.e., results significantly different than chance) occurred among the wrongly remembered associates.

The two articles cited above (Kanthamani & Rao, 1974, 1975) reported the results of 6 series, 5 of which obtained higher scoring on RC than on RW pairs, 3 significantly so. The one reversal was the small preliminary series of Kanthamani and Rao (1974), in which the memory task was markedly different than in the other series. Collectively, these

experiments provide strong support for the RC/RW hypothesis, provided the word pairs have low association values. To assess the overall statistical significance of the Kanthamani experiments, I combined the z statistics from all 6 series using the Stouffer method (see Table 1). The component zs were not weighted by sample size. The difference between RC and RW trials produced a Stouffer Z of 2.82 ($p < .005$). The RC/RW effect would have even been stronger had the analysed results been limited to low-association word pair and the components of the Stouffer Zs weighted by sample size. On the minus side, it should be noted that the authors computed their statistics using the trial rather than the participant as the unit of analysis, and I had to do likewise in computing the Stouffer Z. Using the trial as the unit of analysis provides a more powerful statistical test, but the results cannot properly be generalized to a population of persons because the effect in the sample may not have been distributed uniformly across the sample participants (cf., Stanford & Palmer, 1972). However, the obtained effect is so strong that it is unlikely an analysis with the participant as the unit would lead to a different conclusion. I was also able to calculate Stouffer Zs for the RC and RW trials separately. For RC trials, the results were significantly positive ($Z = 2.59$, $p < .01$), whereas for RW trials they were significantly negative ($Z = 2.52$, $p < .01$).

	Recall-Correct (RC)			Recall-Wrong (RW)			
SERIES	*N*	*T*	*M*	*Z*	*T*	*M*	*Z*	Z_{DIFF}
KR74_Pre	6	58.328	-2.62		62.468	-0.51	-1.53	
KR74_Pil	16	150.607	2.61		170.482	-0.46	1.42	
KR74_1	20	279.574	2.45		121.496	-0.09	0.70	
KR74_2	20	242.558	1.80		158.411	-2.23	2.30	
KR75_1	20	307.573	2.57		93.473	-0.51	2.01	
KR75_2	20	229.485	-0.46		171.410	-2.37	2.00	
TOTAL	102	1265.521 [a]	2.59 [b]		775.457 [a]	-2.52 [b]	2.82 [b]	

[a] Unweighted mean
[b] Stouffer Z

Table 1. Total Number (T) and mean proportion of ESP hits on correctly and wrongly recalled trials in the Kanthamani and Rao paired-associate recall experiments

Attempted Replications

The RC/RW hypothesis was significantly confirmed in one of 2 paired-associate memory series reported by O'Brien (1976), in which words were paired with nonsense syllables, but only if the words were found to occur with low frequency in a normative population. Several other independent replication attempts failed to provide significant support for the hypothesis (Gambale, 1976, Gambale, Margolis, and Cruci, 1976; Harary, 1976; Lieberman, 1975). Gambale et al. reported significant below-chance scoring for RC word pairs, which suggests that the difference between scoring on RC and RW word pairs was most likely in the direction opposite the prediction. Nothing was mentioned about the association strength of the word pairs. Lieberman found a significant interaction indicating superior scoring on low-association pairs when participants were tested individually and superior scoring on high-association pairs when they were tested in groups. However, the effect was not reported separately for RC and RW trials. Nonetheless, this result suggests that perhaps the effect found by Kanthamani only occurs with participants tested individually, which is the procedure she used. Among the other studies listed above, only Harary used group testing. Finally, Parker (1976) obtained significantly higher scoring in RC compared to RW trials in 1 of 2 series using a digit- span test instead of the paired-associate test. In the digit-span test, participants were required to memorize and recall sequences of digits varying in length from 3 to 9.

Unfortunately, only abstracts are available as reports of these replication attempts, and they did not include specific means and significance test results. If the nonsignificant trends were generally in the predicted direction, a combination of all the results might continue to show a significant overall effect, but this, of course, remains speculative. In any event, it appears, as is often the case in parapsychology, that experimenters differ widely in their capacity to obtain a given relationship (Palmer, 1997; White, 1977).

Relation to the Memory-trace Theory

Kanthamani and Rao (1974) stated that the confirmation of the RC/RW hypothesis is in line with Roll's (1966) memory-trace theory because the theory "contends that memory traces of recently learned

events serve as good vehicles of ESP" (p. 297). However, it is clear from the context in which this point was embedded that Roll meant that the memory trace itself becomes the ESP response. In the Kanthamani experiments, the memory traces that served as the "good vehicles" were the correctly remembered paired associates, whereas the ESP responses consisted of such decisions as the line on which the word was written. The corresponding ESP targets were selected to be completely independent of the remembered words, and thus it is difficult to imagine the word memories mediating the location memories.

An alternative explanation of these findings is that successful recall created a momentary feeling of accomplishment, or positive mood, that somehow facilitated ESP success on the RC trials. One would expect this satisfaction to be greater if the recall was difficult, which could account for the superior scoring on RC trials being greater when the word pairs had low association strength.

Proximate Hits in a Paired-Associate Test: The Rao Experiments

At FRNM the Kanthamani research was extended by the Indian psychologist K. Ramakrishna Rao and colleagues (Rao, 1978; Rao, Kanthamani, & Palmer, 1990; Rao, Morrison, & Davis, 1977; Rao, Morrison, Davis, & Freeman, 1977a). The participants were high school students tested in groups in a classroom setting. They were given 5 minutes to memorize a set of 50 word pairs. Each pair consisted of a "high-association" trigram followed by 1 of 10 meaningful words that differed in semantic similarity (meaning) to one another. "High-association" means that the trigram would tend to evoke a meaningful word (e.g., "FAM" might lead one to think of "fame" or "family", whereas "JEX", a low-association trigram, would have such a tendency much less). The words were then randomly matched to the trigrams, which suggests that the associations *between* trigrams and words were primarily if not exclusively low. After the learning period, participants were given a recall sheet with 25 of the original trigrams plus 25 new high-association trigrams, in random order. They were informed that the new trigrams constituted an ESP test, in which one of the 10 words had been randomly assigned as a target to each trigram. Thus, the recall

sheet included a 25-trial memory test and a 25-trial ESP test. The recall sheet was affixed to an opaque envelope, inside of which was a sheet of paper containing the correct answers for all 50 trials. Participants were asked to write on a line next to each trigram on the recall sheet the word they thought had been connected to it on the learning sheet.

Up to this point, the procedure was similar to that used by Kanthamani in the earlier experiments. The primary difference is that instead of there being both a memory score and an ESP score for each trial, half the trials provided only memory scores and the other half only ESP scores. Thus the RC/RW hypothesis could not be tested in these experiments. Following the memory/ESP test, a new element was added to the procedure. Participants were given a sheet containing the 10 test words in a column followed by 9 blank lines. They were asked to rank the other 9 words on the basis of their semantic similarity to the first word in the row by writing them in the appropriate order on the 9 blank lines, thereby creating a 10 x 10 matrix. Each of the participants' incorrect responses ("misses") provided an "association rank score" (ARS), which consisted of the rank given to the response relative to the target word for that trial. Thus, if the target for a given trial was "ball" and the participant selected "moon" as the response and wrote "moon" on the second line following "ball", his or her ARS for that trial would be 2. It was predicted, in effect, that participants with overall positive and negative ESP scores ("hitters" and "missers") would achieve significantly different mean ARSs on their missing trials (Rao et al., 1977). Participants whose ESP scores were within ± 3 hits from chance were eliminated from the analyses.[1] The hypothesis did not specify a direction for the relationship, and no rationale was given for it in the Introduction.

The first report (Rao et al., 1977) described 2 series, each of which consisted of 7 classroom test sessions with different groups. The first 2 testing sessions for Series 1 differed from the others in that low-association trigrams were used and participants were not told that an ESP test was involved. Nonetheless, these sessions were pooled with

[1] The authors specified in addition a more rigid criterion for inclusion, but I will base my review on the first criterion for all studies.

the others in reporting Series 1. The hypothesis was confirmed, in that the mean ARS of the hitters was significantly lower than that of the missers. This means that on trials where hitters guessed wrong, they tended to guess with a word more similar in meaning to the target word than did the missers. The results were based on scores from 150 of the 162 participants, and results from the first 2 sessions confirmed the general trend. For Series 2, the effect was in the predicted direction but did not achieve significance. Results were based on 118 of the 132 total participants. The second report (Rao et al., 1977a) describes 1 series based on 3 classes with total N = 76. The comparison of ARS means between hitters and missers was very slightly in the predicted direction and nonsignificant. For the third series (Rao, 1978), the 95 participants were attendees at a lecture on parapsychology and thus, unlike the previous experiments, represented a wide range of ages. For unexplained reasons, the pool of meaningful words was reduced from 10 to 5. The results for the AR hypothesis were again in the predicted direction but nonsignificant. The fourth experiment was reported only as a short abstract and only minimal data were provided (Rao & Weiner, 1982). The only significant statistic reported was overall negative scoring on the ESP test. However, Rao et al. (1990) reported that "the missers responded much like the hitters in that their missing responses had closer association with target words, $t(41) = 1.57$" (p. 249). This sentence implies that both hitters and missers had low mean ARSs that were similar to each other. In the fifth experiment (Rao & Kanthamani, 1989; Rao et al., 1990), 104 high-school students were tested, with the ARS means of hitters and missers both well below the midpoint of the scale and nearly identical. A manipulation was introduced in this experiment, such that for some of the participants the answer sheet inside the opaque envelope was blank. Support for the AR hypothesis was actually stronger (and marginally significant, $p = .07$) in this condition than in the standard condition. However, it appears that the 2 conditions did not differ significantly from each other, and thus it is best to combine them for the purpose of drawing conclusions about the status of the overall hypothesis.

My name was added to the second report of this experiment because I discovered a possible artifact in the ARS measure. In the report, I gave an extreme-case example to illustrate the problem:

Imagine ... that a particular subject ranked the word love as the closest associate (i.e., a rank of 1) to all the other 9 words on the association rank sheet. Imagine also that this subject used the word love as his or her response on all the ESP-missing trials. His or her [mean] association rank score would be 1 (the lowest possible value), even though ESP was clearly not involved. (Rao et al., 1990, p. 255)

Of course, if a participant were found to have responded in such an extreme manner as in the above example, their data would be discarded. However, a subtler version of the same response pattern could skew the results enough to affect their statistical significance. When a correction was applied to the data, the ARS means of both groups moved from an average of .484 to .489. The latter value, however, remained significantly below the midpoint of the scale ($p < .05$).

I was able to combine the results from all 6 of the Rao studies that tested the AR hypothesis (see Table 2). This time, results were reported by the authors as *t*-tests using the participant as the unit of analysis.[2] However, because *ts* were never reported for the subgroups and sometimes neither were the subgroup sizes (*n*), I at times had to estimate these statistics to compute the necessary ts.[3] I believe that my

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[2] Because the ts were virtually identical to the zs derived from their p-values, the ts were used to compute the Stouffer Zs.

[3] Only 2 of the 6 studies provided sufficient data for the necessary calculations. Five of the 6 experiments reported t tests distinguishing hitters form missers, and this allowed computation of the overall standard deviations (SDs) and standard errors. The ratio of hitters' *SD*s to total *SD*s in the 2 completely reported studies were used to estimate the hitter *SD*s for the other 3 experiments in the group of 5. The subsample ns for hitters and missers for the 3 experiments in which these were not reported were estimated from the average ratios of hitters to missers in the 3 studies where they were reported. In all 3 of these there were more hitters than missers. This result is expected, because the low expected hit probability of .1, combined with the small number of trials per participant, should produce a positively skewed distribution of ESP scores. As the skew should not be as extreme in the Rao (1978) experiment, where the hit probability was 2, the estimated ratio of hitters to missers was halved for this experiment. Results of the Rao and Weiner (1982) experiment were particularly difficult to estimate, as only the t-test for missers and the various ns were reported. The averages of the *SD*s reported for the total sample and each subsample from the other experiments, minus Rao (1978) because of the different scale, were used as estimates of the corresponding values for Rao and Weiner. From the estimated *SD* and the t for missers, the mean for missers could be estimated. Based on the quote from Rao et al. (1990 - see text), it was estimated that the means for hitters and missers were identical.

estimates are close to the true values and, more importantly, unbiased. For reasons of comparability, the uncorrected results from the last study were used.

ESP hitters vs. missers:

	HITTERS				MISSERS		
SERIES	*N*	*M*	*SD*	*t*	*N*	*M*	t_{DIFF}
RMD77_1	*66*	4.85	*.546*	2.43	*84*	*5.04*	2.52
RMD77_2	50	4.88	.598	1.42	68	4.96	0.80
RMDF77	*33*	5.01	*.672*	0	*43*	5.02	0.11
Rao78	*32*	[4.94]	*.326*	0.57	*38*	[5.08]	1.07
RW82	29	*4.88*	*.603*	1.10	42	*4.88*	*0*
RKP90	44	4.83	.529	1.38	48	4.84	0.28
TOTAL	*254*	*4.90* [a]		*2.79* [b]	*318*	*4.98* [a]	*1.95* [b]

Total sample:

SERIES	*N*	*M*	*SD*	*t*
RMD77_1	150	*4.96*	.479	*1.02*
RMD77_2	118	4.93	.543	1.40
RMDF77	76	*5.02*	.589	*-0.29*
Rao78	70	*[5.01]*	.286	*-0.29*
RW82	71	*4.88*	*.529*	*1.87*
RKP90	92	4.84	.506	3.05
TOTAL	572	*4.94* [a]		*2.76* [b]

Note. Estimated values are in italics. Bracketed means are double the reported values to make them comparable to the other means in the table. The midpoint of the ARS scale was only half as large in Rao78 compared to the other studies, as the response alternatives were 5 instead of 10 (see text).

[a] Unweighted mean
[b] Stouffer Z

Table 2. Means and standard deviations of association rank scores in the Rao ESP/paired-associate experiments.

The hypothesis of lower mean ARSs on miss trials among missers as compared to hitters was just on the borderline of significance (Stouffer $Z = 1.95$, $p = .051$). The unweighted estimated mean ARS of the hitters was 4.90, which differs significantly from the midpoint of the scale (5), Stouffer $Z = 2.79$, $p < .01$. The mean for missers was 4.98, which is very close to the midpoint and nonsignificant. For both groups combined, the estimated mean ARS was 4.94, yielding a significant Stouffer Z of 2.76, $p < .01$.

Overall, the results suggest that when positive ESP scorers make an incorrect guess, they tend to miss with a word relatively similar in meaning to the target word. However, it is unclear whether this finding reflects ESP or a response bias. This issue can only be resolved by employing the statistical correction applied to the data of Rao et al. (1990) to the data from the 5 preceding experiments.

If the AR effect is real and a manifestation of ESP, it makes sense intuitively. ESP is clearly not an all-or-none process, and one would expect that at times it would only approximate reality. The finding would also indicate that the ESP stimulus is processed semantically before being reported.

Can ESP Stimuli Influence Memory?

The general hypothesis tested by the research discussed up to this point is that ESP is mediated by memory. In a sense, the research asks whether memory causes ESP. However, this question can be reversed. That is, we can ask if ESP causes memory or more precisely, can ESP enhance or inhibit recall, such as in the memory tasks used in the experiments above? The most direct way to test this hypothesis would be to have someone attempt to send a telepathic message to facilitate the memorization of some words in a list, while either attempting to hinder the recall of other words or simply not to try to influence memory of these words at all. No one has attempted such an experiment with proper controls. We could also ask a somewhat different question. Can the mere presence of ESP stimuli help or hinder the memory process? This more passive approach to the issue has been investigated in several experiments, to which I will now turn.

The Stanford Experiment

The first of these experiments was conducted by the American psychologist Rex Stanford (1970). Stanford asked 30 volunteer participants to listen to a taped dream report, ostensibly to test whether they could make correct inferences about the dreamer's personality from their dreams. To support this cover story, participants were subsequently given a psychological test (Q-Sort) in which they were asked to classify a set of personality descriptors as applying or not applying to the dreamer. Citing the need to be sure the participant remembered the dream well enough to properly access it for the personality assessment, Stanford then gave each participant a 20-item quiz asking them questions about the dream. Eight of these 20 items had 4 response alternatives and definite answers based on the dream content. Unbeknownst to the participants, Stanford randomly assigned ESP-correct answers for each of these 8 questions. On average, the ESP-correct and memory-correct answers would be identical close to 25% of the time. The main research question was this: In cases where the memory-correct answers and the ESP-correct answers differed, would the presence of the latter guide participants' recall away from the memory-correct answers and toward the ESP-correct answers? Two analyses can be cited in support of this hypothesis, both of which are based on the 32 times in which the subjects gave memory-incorrect answer to 1 of the 8 key questions. Stanford called these answers "counter-story responses". First, 31 of the 32 counter-story responses (97%) were from cases where the ESP-correct and memory-correct answers differed, compared to the 75% expected by chance, corrected $\chi^2(1) = 7.04$, $p < .01$[4]. Second, 15 of these 32 counter-story responses corresponded to the ESP-correct target ($p < .01$). Thus, in these cases, not only did the ESP-correct answer in effect "pull" participants away from the memory-correct answer 97% of the time, thereby creating inaccurate recall, but in 47% of these cases the alternative "pulled to" was the ESP target.

[4] I calculated this chi-square based on figures cited in the report.

The Kreiman and Schmeidler Experiments

The same general hypothesis was subsequently tested in 2 experiments by the Argentine psychologist Noam Kreiman (1978, 1980). Participants were students in Kreiman's parapsychology classes. The specific hypothesis was that the presence of ESP stimuli would influence the successful recall of words only if the words were difficult to memorize; easy to memorize words, Kreiman theorized, would not require a boost from ESP to be recalled correctly, and thus no ESP effect would be revealed for such words. Participants were given 5 minutes to memorize a list of 50 words, 20 of which were randomly selected as ESP targets. Easy-to-recall words were defined as the first half of those recalled and difficult-to-recall words as the second half. The ESP score was the proportion of recalled words that also were target words. Because 20 of the 50 words were chosen as targets, the mean score expected by chance is .4. Confirmation of the hypothesis required a significantly higher score for the words remembered in the second half of the recall list than in the first half. Such confirmation was obtained in only the first of the 2 experiments (Kreiman, 1978). Several independent attempts to replicate the original finding, some incorporating small changes in methodology, produced no significant confirmations (Schmeidler, 1980, 1981; Weiner & Haight, 1980).

The American psychologist Gertrude Schmeidler studied the results of all these studies, as well as some unpublished pilot data of her own, and developed therefrom a refined hypothesis that she attempted to confirm in 3 new experimental series (Schmeidler, 1983)[5]. Participants were college students and acquaintances of the testers. There were two main changes from Kreiman's procedure. First, the prediction of success was restricted to those participants who expressed moderate belief in the possibility of ESP occurring in the experiment and who were in a positive mood at the time of testing. As mood questions are susceptible to biased answers (so-called "demand characteristics"), Schmeidler measured mood indirectly by asking 2 questions concerning how "lucky" participants felt they were in general. A positive answer to both questions was required to enter the experimental group. The second change was to compare results from the first half of the recall list to

[5] The first series was also reported in Schmeidler (1981).

results from the last quarter of the list, dropping results from the third quarter. Her 3 series, which were conducted by different testers, were identical in methodology, except that only in Series 1 and 3 were participants told explicitly that ESP could help them remember the target words, a list of which was provided to each participant inside a sealed, opaque envelope in all 3 series. In each series, testing continued until 16 participants, the same N as used by Kreiman, were obtained for the experimental group.

The hypothesis was significantly confirmed in Series 1 and 3, and a clear trend in the predicted direction was found in Series 2 (see Table 3). Combining these results yielded a Stouffer Z of 3.65 ($p < .001$) in strong support of the hypothesis of more positive ESP scores in the last quarter of the recall list compared to the first half. The mean ESP score of .503 in the last quarter was significantly positive (Stouffer $Z = 2.66$, $p < .01$) and the mean of .372 in the 1st half was nonsignificantly negative (Stouffer $Z = 1.48$). Schmeidler also reported the results of participants who did not qualify for the experimental group. These results are also summarized in Table 3, sans t-tests. The mean ESP scores were .390 and .369 for the first half and last quarter respectively. These values are both in the negative direction and most likely not significantly different from chance. The difference between these 2 means is clearly nonsignificant. The entire ensemble of means shows that the hypothesized effect is attributable to strong positive scoring by the experimental group in the last quartile of the recall list.

SERIES	*N*	*r*	*p*
Feather67	30	+.455 (Av./3)	<.02
KR74_1	20	+.284	<.01
Parker76_1	40	-.347	<.05
Parker76_2	40	+.107	*n.s.*
RMD77_1	150	+.04 (Av./8)	*n.s.*
RMD77_2	118	-.18	<.05
Rao78	93	+.18	<.10
Kreiman78	16	-.54	<.02
WH80_1	31	?	*n.s.*
WH80_2	31	-.48	<.01

Table 3. Correlations between total memory and ESP scores

Taken collectively, the experiments reviewed in this section support the hypothesis that the presence of ESP target stimuli influences the extrasensory apprehension of these stimuli. In the Kreiman and Schmeidler experiments, the salience of these targets was enhanced by putting them inside a sealed envelope given to the subjects, a procedure also used by Kanthamani and by Rao in their experiments described in previous sections. Whether this added salience actually had an effect is unknown, although there have been experiments in which the presence or absence of such salience enhancers have been manipulated and their presence influenced ESP scoring (e.g., Johnson, 1973). However, Stanford (1970) confirmed his hypothesis without resorting to such measures. Stanford's experiment also differed from the other experiments described in this section, and in fact from all the experiments discussed in this review, in that participants were unaware that they were being tested for ESP. Thus, such knowledge does not appear necessary for the effect to occur.

It is also noteworthy that Schmeidler could only obtain the predicted results with moderate believers in the paranormal who were in a positive mood at the time of testing. No such constraints were required in Stanford (1970) nor, for that matter, in any other of the successful experiments reported in this paper. On the other hand, it might be that results from these experiments would have been even stronger had they restricted their experimental samples as Schmeidler did. Finally, earlier in the paper I suggested positive mood as a possible mediating factor in the success of the Kanthamani experiments. This could be a further indication of the importance of this variable.

The Correlation Between Memory and ESP Scores

Recall that the purpose of the Feather (1967) experiment, described at the beginning of this paper, was to test for a positive correlation between scores on a memory test and a relatmed ESP test. In this experiment, the correlation was found to be significantly positive. Some of the other experimenters reviewed above also examined their data to see if this relationship held. The results of these analyses provide a decidedly mixed picture. Rao et al. (1977) reported that a significant

positive correlation between ESP and memory was found in Confirmatory Series 1 of Kanthamani and Rao (1974). Rao (1978) also reported a significant positive correlation between memory and ESP with paired associates. On the other hand, significant negative correlations were reported by Rao et al. (1977) in their second series, and by Parker (1976) in her first series using the digit-span test. Nonsignificant relationships were reported by Rao et al. (1977) in their first series and Parker (1976) in her second series. Data regarding this relationship are not available from the other experiments reviewed so far in this paper. The results are summarized in Table 4.

Experimental Group (n = 16 per series)

	First Half		Last Quarter		
SERIES	*M*	*t*	*M*	*t*	t_{DIFF}
Series 1	.36	2.12	.51	1.71	2.66
Series 2	.355	0.44	.49	0.97	1.18
Series 3	.40	0	.51	1.93	2.48
TOTAL	.372 [a]	1.48 [b]	.503 [a]	2.66 [b]	3.65 [b]

Control Groups:

		First H	Last Q
SERIES	*n*	*M*	*M*
Series 1	30	.353	.337
Series 2	27	.402	.315
Series 3	28	.415	.456
TOTAL	85	.390 [a]	.369 [a]

[a] Unweighted mean
[b] Stouffer Z

Table 4. Proportions of correctly recalled words that were ESP targets in the first half and last quarter of the recall task for experimental and control participants, Schmeidler (1983)

The Irwin Analyses

The Australian psychologist Harvey Irwin (1979) attempted to reconcile these contradictory findings by employing the well-known distinction between primary and secondary memory. Primary, or short-term, memory occurs immediately after a stimulus is recognized but lasts a maximum of about 20 seconds, provided no attempts are made to memorize it, a process called type 1 rehearsal. If such rehearsal occurs, the duration of the primary memory can be extended. Primary memories can be recalled with little additional cognitive processing. Some primary memories become permanently stored in secondary, or long-term, memory. Only at this stage are they processed semantically. Retrieval of memory traces from secondary memory is also more complicated than from primary memory.

Memory experiments often employ "interference" tasks between the learning and attempted recall of stimulus material. The effect of such interference tasks is to block the prolongation of primary memories. Thus, tasks that employ effective interference procedures are more likely to be tapping secondary memory, whereas those that do not are more likely to be tapping primary memory, because they allow participants to engage in type 1 rehearsal.

Irwin interpreted Roll (1966) as intending his theory to apply to secondary memory. Thus the positive memory/ESP relationship should occur only if secondary memory is tested. On the other hand, the theory has no implications for a possible relationship between ESP and primary memory.

Irwin noted that the experiments reporting significantly positive memory/ESP tasks utilized interference tasks that would effectively block the preservation of primary memories. In Feather's (1967) experiment, this task consisted of 2-3 runs of ESP card guessing. In the Kanthamani and Rao (1974) experiment, the interference task consisted of listening to instructions about the next phase of the experiment, which the authors described as lasting 2-3 minutes. Parker (1976) also employed an interference task in her digit-span experiment - asking participants to name an example of each of 2 collective nouns - but it lasted only a few seconds, not long enough to affect the primary memory traces. Thus, Irwin concluded that her experiment, in which a

negative ESP/memory correlation was found, measured primary memory.

The relevant experiments of Rao and colleagues (Rao et al., 1977; Rao, 1978) gave mixed and contradictory results. These studies employed no interference task at all; recall followed immediately after the attempt to memorize the paired associates. Irwin argued that with such a design, whether the test measured primary or secondary memory would depend on how many participants chose to engage in type 1 rehearsal near the end of the learning phase. It is noteworthy that these 2 experiments sampled different participant populations. The study providing the negative correlation (Rao et al., 1977) tested exclusively high school students, whereas the study providing a positive correlation (Rao, 1978) used a more variable group of participants, attendees to a lecture. It is reasonable to assume that the lecture audience was more positively motivated than the high-school students, who were a "captive audience". If so, the lecture audience may have been more likely to engage in type 1 rehearsal, which would preserve primary memory.

Additional Experiments and Findings

Two of the experiments discussed under the previous subheading also examined the memory/ESP correlation, but they were published too late to be included in Irwin's review (Kreiman, 1978, Weiner & Haight, 1980). In the Kreiman experiment and in 1 of the 2 series reported by Weiner and Haight, there was a significant negative correlation between memory and ESP scores. The procedure was similar to the experiments of Rao et al. (1977) and Rao (1978), in that the recall task followed the learning task immediately, with no intervening interference. Recall that these are the studies for which Irwin declined to make a "prediction". With regard to my additional speculation about the difference in participant samples, those tested by Kreiman and Weiner/Haight were more comparable to the sample tested by Rao (1978), which yielded a positive correlation between memory and ESP, contrary to my speculation. Although he employed a much different memory/ESP task than the other authors, Stanford (1970) reported that his participants who scored above the mean on a test of "incidental memory" he developed "scored significantly better" on counter-story

responses than did below average scorers, but only on those items that had definite correct answers. Stanford concluded from this comparison that "memory per se is not related to ESP performance; its effect is an indirect one in terms of its relation to response bias" (p. 177), a theme explored more fully in his experiment to be described below.

Beginning with the second of their reports testing association ranks (Rao et al., 1977a), Rao and colleagues introduced the hypothesis that the mean ARSs on the memory and ESP trials should be positively correlated. This can be considered a further test of the generic hypothesis that ESP and memory scores should correlate positively. Whereas for the analyses described above the correlation was based on correct guesses, the present analyses test the hypothesis with respect to near misses. Rao et al (1977a) reported a significant positive relation between the association rank scores of ESP and memory misses in support of their hypothesis ($p = .052$) by a sign test. The Rao (1978) experiment provided a non-significant result in the predicted direction, this time based on a Spearman correlation of .15.

Relation to the Memory-trace Theory

Irwin concluded, based on the experiments he reviewed, that those testing secondary memory provided evidence of a positive memory/ESP relationship in support of Roll's theory. He has no explanation for why ESP should correlate negatively with primary memory.

Perhaps the most noteworthy feature of the experiments reviewed in this section is the relatively large proportion that yielded significant correlations in one direction or the other, even allowing for the likelihood that the correlation was nonsignificant in the experiments in which it was not reported. Another possible interpretation of the results is that the ESP/memory relationship applies to both primary and secondary memory, but for some unknown reason in some experimental circumstances the ESP of participants with good memory skills manifests as psi-missing, thereby yielding a negative ESP/memory correlation. Roll (1966) discussed psi-missing in his paper and considered it to fall within the rubric of his theory. He compared psi-missing to "parapraxes" (e.g., slips of the tongue) and considered both to be caused by a motivation to avoid the correct response. Thus, the

memory trace activated is a wrong one, but participants with good memory skills would be more likely to have these incorrect memory traces come into consciousness than would participants with poorer memory skills.

ESP and Primary Responses in a Word Association Test: The Stanford Experiments

Stanford (1973) tested Roll's theory using a word association experiment that ironically did not include a memory test. He noted that "[Roll's] theory implies that frequently reinforced memory traces more often serve as vehicles for ESP than less frequently reinforced ones" and, more specifically, that "responses in a situation likely to elicit ESP should more often be psi-mediated when those responses depend upon well-established associative connections...." (Stanford, 1973, p. 150). This, incidentally, is the direct opposite of the inference from Roll's theory made by Kanthamani and Rao (1975), who proposed that ESP scores should be more positive with low-association paired associates, because Roll's theory implies that "formation of associative habits inhibits the function of ESP" (Kanthamani & Rao, 1975, p. 2).

Stanford gave 60 volunteer participants a 66-item word association test, 36 items of which were used to test the memory trace hypothesis. Participants were asked to spontaneously respond to each word in the list with the first word that came to mind within 4-8 seconds. For example, if the stimulus word was "cat", the participant might respond with "dog". Each word in the 36-word sublist had a distinctive primary (most common) and secondary (second most common) response according to population norms. For each of these 36 words, either the primary or the secondary response was randomly selected as the ESP target. A co-experimenter in another room attempted to "send" the targets telepathically to the participant. The prediction based on Roll's theory was that participants should choose the ESP target a greater proportion of the time (more ESP "hits") when they made primary responses than when they made secondary responses, under the assumption that the memory traces underlying primary response have been more frequently reinforced in the past. Stanford added that this

conclusion would apply regardless of how frequently the primary responses were chosen in this particular experiment. The prediction was significantly confirmed ($p < .025$).

A complication arose from the fact that Stanford (1973) also proposed a seemingly contradictory hypothesis, based on the empirically supported proposition that participants who have a strong tendency to avoid a particular target alternative obtain a relatively high percentage of ESP hits when they do make such a response (Carpenter, 1977). The hypothesis seems to contradict Roll's theory in that these counter-bias responses are the ones that presumably have not been reinforced in the past. Stanford found a significant positive correlation between the number of primary responses on the 36-word list and the relative proportion of ESP hits on primary as opposed to secondary responses ($p < .03$, one-tailed), thus confirming the response-bias hypothesis.

Stanford attempted to reconcile these 2 disparate findings by proposing that greater ESP hitting on primary vs secondary responses only applies for those participants who lack a strong bias in favor of giving the primary response. He confirmed this proposition by noting a significant negative correlation between the number of primary responses and the relative ESP hit rate on these responses ($p < .025$).

An attempted replication of Stanford (1973) by Stanford & Schroeter (1978) failed to yield enough secondary responses to test Roll's hypothesis by the principal method used in the first experiment. However, a secondary measure called "sensitivity to primary targets", which Stanford and Schroeter reported as approaching significance in the predicted direction in the Stanford (1973) experiment ($p = .11$), again approached significance in the replication ($p = .10$, one-tailed). The response bias hypothesis failed confirmation in the replication attempt.

Relation to the Memory-trace Theory

Roll (1966) in fact devoted considerable attention in his paper to the response bias effect, which he labelled as "associative habits", and he considered it to fall within the framework of his theory. In line with Stanford, he suggested that "other memory traces associated with the

former [memory trace] will also be evoked, not because of their relationship to the ESP stimulus but because of their relationship to the first memory trace" (p. 508). Thus, if a participant has a strong bias to make a certain response, that bias will drown out the ESP stimulus as a response determinant and no ESP effect should be observed. On the other hand, participants who lack such biases would be assumed to be sufficiently spontaneous in their responding to allow an ESP-correct primary response to be activated by the ESP stimulus. This appears to be the same set of interpretations that Stanford (1970) had in mind to explain his data, although he did not present them as fully as I do here.

Nonetheless, Stanford's (1973) data in principle challenges the implication from Roll's theory that only strongly reinforced memory traces can be vehicles for ESP. This challenge can be answered for this experiment by noting that even secondary word-association responses are strongly reinforced in the culture, although not as strongly as primary responses. However, on the surface this conclusion does not appear to apply to sequential guessing tasks commonly employed in forced-choice ESP testing and from which most of the support for the response bias hypothesis comes. Research has consistently shown that in such situations participants have a strong bias to avoid calling the same symbol or number twice in a row (Wiegersma, 1982). It would seem therefore that such a response sequence has not been strongly reinforced. However, this is not necessarily the case. Roll (1966) defined what he meant by strongly reinforced when he stated that "we expect ESP responses to be expressed in terms of memory traces that are recent, frequent, and vivid" (p. 507). The key word here is "recent", because the most recent relevant memory trace a participant has available in a sequential guessing task is his perception of his response on the immediately preceding trial. So insofar as recency plays a role in defining reinforcement, repetition of one's last response has some claim to being positively reinforced. But, if this is the case, why are participants in fact biased against such repetitions? Perhaps the first impression they get of the next target actually is the previous target, but this image is very quickly suppressed because of the common (but mistaken) belief that targets don't commonly repeat in a random sequence. I am not aware of any research testing this speculation.

Summary and Conclusions

This paper has reviewed several sets of experiments directed at specific questions about the relationship between ESP and memory. The following statistically significant relationships have emerged from pooling the results of the members of these sets of experiments:

1. Participants score above chance on ESP test items linked to correctly remembered paired associates and below chance on ESP items linked to incorrectly remembered paired associates.
2. When participants score above chance on an ESP test in which they are asked to guess the second word of a pair of words to which they had not been exposed, incorrect guesses tend to be similar in meaning to the correct choice.
3. Memory can be influenced by the presence of relevant ESP target material, at least among participants who believe moderately in ESP and are in a positive mood at the time of testing.
4. There is a linear relationship between total scores on an ESP test and a related test of memory, but the direction of the relationship is inconsistent. A positive relation tends to emerge when secondary memory is tested and a negative relationship when primary memory is tested.
5. In a word association test, persons with strong biases to respond with primary associates score better on a related ESP test when they give secondary responses, whereas persons with less or no such biases score better on the ESP test when they give primary responses.

These conclusions must be treated with some caution however, primarily because for the most part they are based on consistent results obtained by just one investigator. In some cases there have been no independent replication attempts of the work of the investigator whose repeated series form the backbone of the evidence for the effect ([2], [3], and [5] above) and when such replications have been attempted [2]) the result have not been encouraging. The case for [4] (and to a limited extent [3]) is built on results from a wider range of investigators using different methodologies but [4] is the case where the results are most inconsistent and difficult to interpret. At this point all the results should

be considered as suggestive, but they are promising enough to encourage further research, especially by independent investigators.

All the groups of experiments except [4] were designed to test Roll's memory trace theory, although the [1] experiments did not in fact do so. In all cases, the results can be taken as supporting the theory. However, this support can only be considered as strong as the evidentiality of these various results, which, as I have just noted, must be viewed with some circumspection. Moreover, the tests of the theory have been quite indirect. It would be more convincing if it could be shown more incisively that ESP responses actually are memories as opposed to direct representations of the ESP stimuli. Furthermore, sensory stimuli do not arrive in our consciousness as such direct representations; they undergo various kinds of cognitive processing that include interactions with memories. Thus, memory almost certainly plays a role in determining the conscious ESP imagery or thought, even if that cognition originated with some sort of quasi-sensory percept.

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WHY DO GHOSTS WEAR CLOTHES? EXAMINING THE ROLE OF MEMORY AND EMOTION IN ANOMALOUS EXPERIENCES

Richard Broughton*

If I were to say that I had seen a ghost, I think most people could imagine what my experience was like. I would have encountered an entity that was not a living human being, but might have been at one time. But what did that entity look like? Does the pale, shapeless, shroud-draped figure of illustrations and cartoons spring to mind? If it does, that would be far from what most people who claim to have seen a ghost typically report.

Consider this example from a Canon Phillips in 1963. Shortly after the death of C. S. Lewis, he had this experience.

"But the late C. S. Lewis, whom I did not know very well, and had only seen in the flesh once, but with whom I had corresponded a fair amount, gave me an unusual experience. A few days after his death, while I was watching television, he 'appeared' sitting in a chair within a few feet of me, and spoke a few words which were particularly relevant to the difficult circumstances through which I was passing. He was ruddier in complexion than ever, grinning all over his face and, as the old-fashioned saying has it, positively glowing with health."

Canon Phillips had a second similar encounter with the ghost of C. S. Lewis, and later he was asked by two investigators from the Society for Psychical Research to describe the scene in more detail. He reported

"On both occasions C. S. Lewis was dressed in rather rough, well-worn tweeds, brown in colour. This did not strike me as remarkable at the time until I realized some weeks later that I had never seen him in ordinary clothes." (MacKenzie & Goldney, 1970)

The fact is that in most cases the "ghost" is clearly recognizable as a person, and in many cases it is a person who is known to the one who sees the ghost. Often the ghost is a loved one, but sometimes they represent historical figures, or complete strangers.

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The popular interpretation of ghosts has traditionally been that the ghost is the spirit of a dead person who has returned for a visit, and many still believe that today. Almost as soon as the study of these experiences became systematized with the formation of the Society for Psychical Research in the late 19th century investigators began to see problems with this interpretation. The possibility of an immaterial soul or spirit continuing beyond bodily death was not a problem and, in fact, was in keeping with prevailing religious belief. The problem was that the ghosts were wearing clothes, and sometimes bearing objects. The idea that one's very material clothing also passed on to the afterlife did not give way to any convincing explanation within the traditional religious concept of survival. Often the clothing that the ghost appeared in was what the deceased customarily wore, not necessarily those in which the person died. Are there "valet angels' in heaven who will fetch your favourite clothes?

A more serious problem arose as the early investigators began to amass a large collection of reports of ghostly apparitions that were, for all practical purposes, like other ghost experiences, except that the person in the apparition was not dead. This led early SPR researchers such as Edmund Gurney to conclude that these experiences were hallucinations mediated by telepathy (Gurney, Myers, & Podmore, 1886) F.W.H. Myers largely agreed with the idea that the experience was hallucinatory, but he proposed that the departed spirit interacted with the mind of the person experiencing the apparition in order to bring it about (Myers, 1903)

Thus from the earliest days of psychical research there was an awareness, if not a consensus, that classic ghost experiences were essentially a product of the mind of the percipient – an hallucination composed of images taken or constructed from the experiencer's memory. How the hallucination was triggered remained a mystery, and, of course, a subject of much discussion to this day.

It is not primarily my intention to discuss ghosts and apparitions beyond using them to illustrate that very quickly the psychic experience was being deconstructed into its constituent parts, and one of those parts was our memory store of images. What makes psychic experiences interesting for science is that they involve an apparently

anomalous transfer of information. Hallucinations are of psychological interest of themselves, but when an hallucination carries information that alerts a person of a tragedy or provides other information that we are confident was neither sensed or deduced, then it presents a different challenge to science.

A simple way of looking at psychic experiences involves three main components:

1. A trigger event. Usually this is of personal significance to the experiencer, but not always.

2. A method of "transmission". I put the word transmission in quotes because I do not want to suggest a radio model. It is precisely this point that makes the paranormal just that. What could possibly carry information from great distances with no apparent medium, or, worse yet, bring information from the future? My opinion is that this is a problem to be solved by physics, but at this point we can say little more than that this is an unknown linkage between the event and the experience.[1]

3. The recipient. This, of course, is the person who receives the information about the event. I use the term recipient rather than percipient to avoid pre-judging the issue on whether something is being perceived in a manner analogous to sense perception. In the recipient, the process probably involves two stages: (1) How the anomalous information enters the system, and (2) How the human system turns the input into useful information that can change behaviour or the content of conscious awareness. [2]

It is with this third component – the recipient – that some glimmer of understanding is beginning to emerge. Irwin (Irwin, 1999) refers to this as the experiential phase of psi, and he draws a distinction between two different approaches to understanding this phase. One approach is a sensory-like approach (or "pseudo-sensory" in Irwin's terminology) which is embodied in the popular notion of a "sixth sense". In contrast

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[1] This is not to suggest that there are no theories of what might be happening. Indeed there are several (see Stokes, 1987) but all remain speculative.

[2] Parapsychological researchers commonly speak of a generally accepted "two-stage" model for receptive psychic experiences (extrasensory perception or ESP) which is simply the second and third items in this list.

to this are models of psychic experience based on memory. Although some investigators have proposed models based on sensory processing (e.g. Schmeidler, 1991) Irwin's thorough analysis of spontaneous psychic experiences and laboratory research (Irwin, 1979) lead him to the conclusion that "...the available evidence does not encourage a view that extrasensory information receives the same sorts of processing as sensory input" (Irwin, 1999, p. 166). The typical characteristics of sensory stimuli, e.g., its discriminability, have no systematic effect on performance in ESP tests. In the cases of spontaneous ESP, apart from waking hallucinations, which are the least common and have only an apparent sensory input, the anomalous information enters consciousness without any clues to how it got there. In all cases of normal sensory experience one can reflect upon the process by which the information arrived through the senses, but one cannot for psychic information. While sensory models are not ruled out, Irwin believes that the weight of the evidence points toward memory-based models that involve what he calls "ideational" processing. In these models, the bulk of the information processed in a psychic experience comes *not* from a source external to the receiver, but from within the receiver.

I am in general agreement with Irwin's assessment, but I take a slightly different perspective on the input issue. While I agree that the pseudo-sensory model has not been helpful, we still have to recognize that there is an input of some sort. I think a better way of characterizing the issue is to consider whether the anomalous input is a high bandwidth channel that can convey a great quantity of new information, or a low bandwidth channel that carries only a very little information, but sufficient information to activate the memory systems needed to "get the job done". Although we have no way of measuring the channel capacity, the evidence suggests that component images and ideas of a psychic experience arise from within the system.

Memory Models of Extra Sensory Perception

The role of memory in the psychic process was recognized to a greater or lesser degree by the earliest investigators, first in the case of hallucinations, and then more broadly across the entire range of psychic experiences. In the 1930s René Warcollier observed, "We must

admit...that the images which appear to the mind of the percipient under the form of hallucinations, dreams, or more or less well-formed images, spring exclusively from his own mind, from his own conscious or subconscious memory. *There is not carrying of visual impressions from the agent to the percipient...*" (Warcollier, 1939, p. 133, italics in original).

William Roll (1966) was the first to develop a comprehensive memory theory of extrasensory perception. Drawing on observations of spontaneous case researchers Roll argued that psychic experiences of extrasensory perception (ESP) consist of revived memories. In Roll's model there is an anomalous input or trigger, but after that the psychic experience is based on the contents of existing memory. Roll draws a contrast between ordinary sensory input, in which new input is processed by comparing it against existing memory data, and the extrasensory experience, which arises solely from memory. Once evoked, the ESP-triggered memories are subject to the same mental processing that ordinary memories are (Roll, 1966).

The most fully elaborated memory model of ESP has been developed by Harvey Irwin (1979). Building upon Roll's work, Irwin acknowledges that while neither the spontaneous cases nor the experimental research establishes it conclusively, the case for ESP being wholly based on memory is very strong.

One of the obvious characteristics of ESP information is that it is new to the recipient. Irwin notes, however, that this does not mean that memory not is involved. A psychic experience that informs you that a good friend has just died obviously could not activate a memory of your friend dead, but would likely activate networks of memories involved with your friend, and death in general, and perhaps the manner in which he died. ESP activates and links the discrete components that give rise to new information by their conjunction. This sort of experience is often accompanied by the distinct awareness that the ESP-triggered memory images are suddenly intruding in mental activity that was directed elsewhere.

If this conjecture is correct, then it can account for the fact that ESP information is often frustratingly incomplete. The recipient simply may not have the memories needed to complete the picture, or perhaps for

whatever reason, the ESP trigger is unable to cause some links to be made.

Investigators who have used the experimental technique known as the ganzfeld have often seen this memory process in action. In a ganzfeld experiment the subject is placed in a state of mild sensory isolation and asked to describe the images that parade across consciousness while someone in another location attempts to "send" an image. Afterwards the subject judges a set of pictures to identify the one that was most like his or her imagery. Often the technique produces very striking hits, but the actual images that the participant reports are drawn from his or her past experience. Those memory images, however, approximate the target picture reasonably well and the subject can identify it as the target. It is probably no coincidence that two of the most successful targets in one well-known series of ganzfeld experiments conducted by Charles Honorton and his colleagues were a short segment of a Bugs Bunny cartoon and a static picture of Santa Claus in a Coca Cola advertisement. These would be well-established memory images for his American participants.

Another issue for Irwin was what type of memory is involved in ESP. It seems clear that the type of memory involved in the ESP process is long-term declarative memory. The spontaneous cases suggest that the memories are primarily visual and generally concrete rather than abstract. Irwin observes that in a number of experimental and quasi-experimental studies it is the *structure* of the target that seems to come through. Semantic memories do not seem to be activated. If any semantic information emerges it is usually at a later stage when the subject attempts to interpret the images he or she has experienced. Warcollier's extensive naturalistic telepathy experiments led him to conclude, "It is not the *meaning* which is transmitted, but the *image*" (Warcollier, 1939, p. 131). Upton Sinclair's experiments with his wife as subject (Sinclair, 1962), repeatedly produced examples of strikingly close reproductions of the target material, but which Mrs. Sinclair could not identify at all. More contemporary examples come from the research on remote viewing that supported America's psychic spying programme. Early in that project the scientists realized that the sketches and drawings the remote viewers made were likely to be more useful

and accurate than the viewer's effort to interpret his or her impressions verbally. This led to the standard operating procedure to remind the remote viewers to just draw their images, not to try to interpret them.

While I agree with Irwin that semantic memory seems to play little or no part in ESP, I think more than mere structural elements are involved. I think the evidence, particularly from the dream research and the ganzfeld experiments, indicates that complete images of objects or individuals are often activated. I am inclined to think that the basic memories that underlie the ESP process when it arises into consciousness are primarily *visual* images, but I don't think this is far from Irwin's notion. .

Having focused on the structural memories as the vehicle for ESP, Irwin goes on to develop a model of how the known features of human information processing will determine how the ESP information eventually emerges into consciousness. Although I think we are dealing with more than just structural memories, I think the idea that once activated, ESP-triggered memories will be subject to the same sort of cognitive processing that ordinary memories are makes sense. One simple example is that issue of cognitive processing capacity. Spontaneous case investigations reveal that a large proportion of the waking psychic impressions come when the recipient is relaxing or engaged in a more-or-less "mindless" activity such as washing dishes. Spontaneous experiences are rarely reported during periods of intense cognitive activity. In the most successful laboratory ESP experiments an effort is made to make the subjects relaxed and undistracted by sensory or cognitive activities.

Revisiting Spontaneous Psychic Experiences

Irwin's model, and Roll's which preceded it, offer an important framework within which to understand how psi works in the brain. Surprisingly, there has been very little research to explore Irwin's model, and no research at all studying possible connections between memory function and ESP using experimental techniques that produce better psi combined with appropriate sample sizes based on power analyses. In the years since Irwin's work, neuroscience has moved on,

and I shall argue that today there are even more compelling reasons for suspecting the involvement of memory systems in psychic experiences, and that other systems of the brain and body are likely to be involved as well.

First a brief review of the salient characteristics of extrasensory psychic experiences as they occur in life is needed. However much we may learn in the laboratory, the natural occurrence of psi in daily life remains the touchstone against which out understanding must be tested.

Spontaneous cases naturally fall within three main categories. Because the various case collections have used different criteria and have had different emphases, it is not wise to rely on them for exact proportions, but we draw some general conclusions. By far, the most common vehicle for psychic experiences is the dream. Dreams form between one third and nearly two-thirds (in the massive L.E. Rhine collection) of all spontaneous cases reported (Stokes, 1997).

It has long been recognized that the images that play across our dreams are from memory sources. The dreaming state seems particularly designed to facilitate the re-arranging and combining of memory images in new patterns and episodes. Of course, this is fundamental to psychoanalytic theory and is a well known feature of creativity and problem-solving. It is not a big step to expect that dreams might form an ideal workspace in which the yet unknown psychic influence can assemble the various memory images to present new information to consciousness.

Louisa Rhine, who specialized in the analysis of spontaneous cases, divided the dream experiences into two categories: realistic and symbolic. Two examples from the thousands of dream cases will serve to illustrate how memory underlies the ESP process.

The first case comes from a streetcar (tram) driver in Los Angeles. He reported an extremely realistic and detailed dream in which he was driving a streetcar on the W line loaded with passengers. “All the things in the dream were as they actually were; I mean the streets, stores, traffic conditions. Everything was in the dream just as they were in real life.” Crossing an intersection he saw a northbound number 5 tram and waved to the motorman as he passed. “Suddenly, without warning a big truck, painted a solid bright red, cut in front of me ... and the truck

making the illegal turn could not see my car because of the other streetcar. There was a terrific crash. People were thrown from their seats and the truck overturned." Two men from the truck lay dead on the pavement and the driver walked up to a woman from the truck who was screaming in pain. She looked at the driver with what he described as the "largest, bluest eyes I had ever seen" and repeatedly shouted, "You could have avoided this."

The driver awoke from his dream soaked in sweat and very shaken. Later when he arrived at work he was assigned to the W line and had forgotten about the dream. On his second trip of the day he was at the intersection from his dream with a full load of passengers.

"I was waiting for the signal to change, still not thinking of the dream, when suddenly I became sick to my stomach. I was actually nauseated. I felt provoked at myself and hoped it would go away. As I left the intersection on the signal change, I saw, just as in my dream, a Number 5 car, northbound. Now I was definitely sick. Everything seemed to have happened before, and my mind seemed to be shouting at me about something. When I waved to the motorman on the '5' car, the dream came to me. I immediately shut off the power and applied the brakes, stopping the car. A truck, not a big truck completely red as in my dream, but a panel delivery truck, with the space for the advertising on the side painted over with bright red, shot directly in my path. Had I been moving at all, I would have hit it as surely as I did in the dream.

"There were three people on the truck, two men and a woman. As the truck passed in front of me, the woman leaned out of the window and looked up at me with the same startled, large blue eyes I had seen in my dream, and...waved her arm and hand, thumb and forefinger circled in the familiar 'okay' gesture."[3]

This is an example of a realistic case and it is easy to see that the dream consisted of the driver's well-established memories of the streetcar route and the situations he normally encountered, plus apparently new information assembled from his memories of other vehicles, people and possible scenarios. Although he may not have had

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[3] From the LE Rhine collection, quoted in Broughton (1991, pp. 20-21).

the memory of a streetcar collision as such, it would be a simple matter to construct a plausible scenario from existing memory images just as anyone could imagine an auto accident happening. It is interesting to note that the dream images of what had not yet happened were not entirely accurate (e.g., the truck), but they were enough to get the message across.

The second illustration is a symbolic dream from a woman whose son was in the Navy in the South Pacific Theatre during the Second World War. "I dreamed that my young son, an only child...came to me while I was busy in the kitchen and handed me his uniform which was sodden, soaking, and dripping wet. He had a most distressed expression on his young face..." The woman's dream continued with her wringing the water out of her son's uniform but her son took it from her and dropped into the laundry tub and took her into his arms and said, "Isn't this terrible! Oh, Mom – it's all so terrible!" In the dream the woman reminded her son that nothing was so terrible that they could sit down and talk it through, and the two of them went into the living room and the woman sat down and her son sat in her lap, and, as these things happen in dreams, the son turned into an infant as she rocked him and the dream soon ended. In due course the woman learned that her son's ship had been torpedoed on the very night that she had the dream and that her son and 250 others had been killed in a massive explosion of ordnance that resulted (Rhine, 1961, p. 49).

Again, the dream images are all drawn from memory images or the plausible manipulation of image components. It is classified as a symbolic dream because it is far from a representation of the related event. One can only speculate as to why some psychic dreams are symbolic, but it could be that for this woman the images needed to construct a scenario of the carnage of a torpedoed munitions ship were simply not available and the "message" was carried through other images. This was before television, of course, and the images that the public saw in theatres and elsewhere were carefully controlled during the war.

The second largest type of spontaneous ESP in most of the collections is the intuitive impression, representing a little more than a quarter of the cases. The intuitive impressions often amount to a sudden

hunch or an unexpected awareness that something of consequence has happened (usually to someone connected to the person who has the experience). The cases are often described as "just knowing" and are unaccompanied by any imagery or reasoning process. A typical example of an impression case would be when a mother suddenly "knows" that something has happened to her child and drives to the school where she learns that her child had just had an accident in the playground. Many of these impressions are accompanied by strong emotional feelings, often anxiety or dread. A significant number of cases involve *only* the feelings, with no cognitive content to explain them. Consider this case from a young man in California.

"One night in July of 1951 we had just finished supper, and my brother-in-law was getting ready to go to a meeting in San Jose, which is twenty-five miles from our house. For no reason I started crying, me, crying, twenty-five years old! I *begged* him not to go. Well there was quite a fuss and I got everyone upset. Mom kept saying, "He will be all right." You know, the usual soft soap you give an upset person. This went on for about fifteen minutes. Then the feeling left me, and I said, "It's all right for Bob to go now."

"By this time the fellow he was to ride with had waited at their meeting place, but left before Bob got there, so Bob had to drive his own car down. He got as far as Bayshore and Charter Streets, when the traffic began to back up. A wreck, which is nothing unusual around here, but when Bob got to the corner, he said he almost passed out. There spread out on the highway was the man he was to have ridden with; his head was half gone. The car was a total loss. They found later that his brakes had locked on one side, and he flipped up in the air and came down on the other side of the road to be hit head-on by another car." (Rhine, 1961, p. 127)

This is a striking example of a case where the unexpected onset of strong feelings – with no images or other information – managed to prevent a family tragedy.

The final type of spontaneous case is that of the waking hallucination or quasi-sensory image. This is where we find our clothed ghosts, but most of the hallucinations involve living individuals. Sometimes the hallucinations can be relatively trivial, but frequently

they convey important information. In another Second World War example, an American soldier had been driving a car with several officers on an inspection tour of the front lines. Just before starting the return journey, another soldier told him of a short cut back to the base. He found the shortcut and began driving down the road when suddenly he saw a friend waving him to stop, telling him to go back the way he came. Without thinking much of it, he reversed the car, taking care to avoid a truck full of marines waiting to go down the same road. Only when he was nearly back at the base did he realize that the friend who had just directed him back to the main road had, in fact, been killed a couple of weeks earlier. The next day when the casualty reports came in, the driver learned that the truckload of marines that he had taken care to avoid had hit a mine on the road he almost went down, and all were killed.

Those are the main types of psychic experiences when we recognize them to be sufficiently different from our ordinary experience that we take not of them. But if psi can affect behaviour rather dramatically, as we seen in spontaneous cases, might it not operate at more subtle and less noticeable ways. This brings us to a fundamental question that we often loose sight of, “What is psi for?” Surely it is more than a curious freak show on the margins of consciousness. At an operational level, it seems that the chief function of psi is to provide information that we use, consciously or unconsciously, to make better decisions about future courses of action or to otherwise guide our behaviour. At a more general level, at least for me, the answer is simple: psi is meant to be useful, and it is meant to be useful in the same way as all our other abilities-to contribute to our survival and our ability to pass on our genetic heritage (Broughton, 1988).

From this perspective we would expect ESP (or receptive psi) to have certain characteristics. As something that is likely to have co-evolved with consciousness we should expect ESP to be tightly integrated with all the other components of human behaviour. The “normal” operation of ESP (in contrast with the more exceptional instances that attract our attention) might be to bias our decision-making process and influence our behaviour in such a way that serves our needs. The most thoroughly developed model of receptive psi as a

need serving component of human behaviour has been developed by Stanford (Stanford, 1990). In Stanford's model, ESP information can unconsciously initiate a wide range need-relevant "responses," including behaviours, feelings, images and associations or even desires.

One of evolution's distinguishing characteristics is that it is a remarkably economical process. Evolution tends not to devise entirely new systems but rather adapts and extends existing systems to serve new needs and confer new advantages. Our own brains are build on the substrate of a reptilian brain which remains a crucial part of what it means to be human, and to survive. New functions are typically "piggybacked" onto existing systems that they might enhance.

What system or systems of the human brain might we speculate that psi is piggybacked upon? If I were to place a bet, it would be on the emotional system, working in conjunction with memory. The emotional system is, of course, the underpinning of that most fundamental survival adaptation, the fight or flight response, and, through the operations of the amygdala is designed the detect threats and danger and to initiate responses automatically. This is a system that we share with virtually all vertebrates. LeDoux (1996) argues that it is precisely the merging of the emotional and cognitive systems that conveys our immense evolutionary advantage because it allows us to shift from simple automatic reactions to planned actions. Damasio (1994) has further shown that the emotional system plays a major role in the action phase by constraining the available options by biasing the selection of memory images that present to us our options for action.

Elsewhere I have argued that Damasio's somatic marker hypothesis offers a very promising framework in which to see the possible operation of anomalous information in consciousness (Broughton, 2002). In that paper I drew attention to the emotional components of psychic experience, but Damasio's somatic marker hypothesis involves both the emotional system *and* memory working in close harmony.

In this context, let us turn again to the spontaneous cases. The special relationship between sleep and dreaming, and memory is an area of much research and considerable debate today (Hobson, Pace-Schott, & Stickgold, 2000; Stickgold, Hobson, Fosse, & Fosse, 2001). Sleeping and dreaming seems to enjoy a two-way relationship with

memory. Memory is obviously the source of the content of dreams, but dreams, and more generally, sleep, plays a role in the consolidation and strengthening of long-term memory.

Precisely what the sources of the memories for dreaming are and how they are selected remains uncertain. Some researchers now see dreams not simply the result of more-or-less random brain activity, but a form of meaningful memory processing (Paller & Voss, 2004; Revonsuo, 2000).There is growing evidence that the emotional system plays an important role. Recent research shows that during REM sleep the central nucleus of the amygdala and the limbic forebrain structures are activated, contributing substantial input from the emotional system into the dreaming process. Braun and colleagues have shown that the limbic system and visual association areas are active in REM sleep, but not the primary visual area of the brain, suggesting that dreaming may represent a closed system that operates without the brain systems that mediate sensory information (Braun et al., 1998). Interestingly, Stickgold and colleagues note, "... although emotions appear to play an important role in the selection of memories for incorporation into dreams, the dreams themselves often show little or no emotional content." (Stickgold, Hobson, Fosse, & Fosse, 2001, p. 1056). The particular combination of brain activity that occurs during dreams may be suited to the creative and problem solving activity associated with sleep and dreams (Wagner, Gais, Haider, Verleger, & Born, 2004), and it may provide a suitable canvas upon which the memory images needed to convey anomalous information can be painted.

Intuitions, which form the second most common type of psychic experience, might, on first inspection, present problems since they seem to involve neither memory nor images, but that would be misleading. The momentary contents of consciousness are images, some sensory, others drawn from memory. During periods of low sensory input, memory images will dominate. These images are held in working memory for periods from a fraction of a second to several seconds. The mechanism of basic attention holds a particular image in working memory more or less to the exclusion of other images. As part of his somatic marker hypothesis Damasio (1994) argues that the emotional system, working in concert with the prefrontal structures of the brain,

plays a major role in the generation of the particular images that play across consciousness and in determining which images receive the focus of our attention. When this unconscious process leads to sudden conclusions or decisions, it will feel like *intuition* because the solution or the decision will seem to have just "popped" into mind. In fact, a very subtle interplay of learned emotional experience and memory will have been behind the process, but that will all be hidden from conscious awareness. My speculation is that this would be a convenient process on to which a system for using anomalous information might be grafted.

A particularly interesting feature of many of the intuitive cases is that they involve strong emotional feelings. LeDoux (1996) and Damasio (1994) and others have shown that in addition to declarative memory a person has emotional memory. These are memories that can set our bodies in the physical states – the feelings – associated with past experiences. These memories of feelings are the somatic markers that underpin decision making and planning in Damasio's somatic marker theory. Cases such as the young man who's teary outburst delayed his brother-in-law's departure provides a most important clue to the process in that they seem to represent a direct activation of feelings with no cognitive content at all.

The third and least common type of psychic experience – hallucinations – also involve memory images, but now masquerading as sensory input. The images are most commonly visual, but also can be auditory or involve other senses. The hallucinations of spontaneous cases are also quite unlike the recurrent hallucinations associated with clinical and neurological conditions. They usually are isolated events in normal individuals.

The aetiology of hallucinations in clinical and non-clinical populations is also the subject of much research and debate (see Collerton, Perry, & McKeith, 2005, and accompanying commentaries) The prevailing models for hallucination generally involve deficiencies or malfunctions in attributing the source of images in short term memory (Bentall, 2000; Collerton, Perry, & McKeith, 2005). In these models, images from internal sources are incorrectly attributed to external sensory input. More recently, speculation has grown that waking

hallucinations may have their origins in the same mechanisms that underlie dreaming (Gottesmann, 2005; Ingle, 2005; Mahowald, Woods, & Schenck, 1998; Pace-Schott, 2005).

However hallucinations are triggered, there is little doubt that the images are drawn from, or constructed from schema held in, long-term memory. In psychic hallucinations, what is it that selects the particular memory images that convey the message? At this stage the answer to that question can only be speculative, but, again, Damasio's somatic marker hypothesis provides some intriguing clues. A fundamental component of his model is the role of the emotional system in selecting the images to which we attend, and the evolutionary roots of this system are in that part of the brain concerned with threat detection and survival reactions. That system has evolved mechanisms to bias and influence the attentional resources we deploy to the images that represent our options for action (Damasio, 1994, 1996). Again, if we have a system designed for fast automatic processing of survival-related information, would it not make sense for it to incorporate psychic information if and when it is available? Evolution may simply have found that the way to present psi information during ordinary waking consciousness may to "superimpose" suitable memory information on ongoing sensory processing.

A final intriguing suggestion linking our use of psychic information and the emotional system lies in what we often think of as one of the great problems with research in this area – our inability to control consciously the use of psychic information. Spontaneous cases are just that – spontaneous. They come of their own accord – one does not make them happen. In the laboratory psi is described as "elusive" and that its operation is unconscious. In my evolutionary view of psi I argued that one of the characteristics we could expect of evolved psychic abilities might be that they are not subject to conscious control, because if they were they would be too easy to misuse in a way that was not conducive to survival in evolutionary terms (Broughton, 1988). At the time I could not think of a mechanism, but recently Dolan has noted that emotions "...are less susceptible to our intentions than other psychological states insofar as they are often triggered, in the words of James, 'in advance of, and often in direct opposition of our deliberate

reason concerning them'" (Dolan, 2002, p. 1191). Indeed, it seems the very nature of the emotional system's operation as a survival response system is that it is unconscious and independent of our intentions.

In conclusion, it seems clear, perhaps even obvious, that memory mediates the appearance of psychic information into consciousness. How those memories are triggered remains a mystery, but I am convinced that the evidence from our growing understanding of the emotional systems suggests that we should look there for the origins of the psychic influence on behaviour. The substantial number of spontaneous cases that seem to consist solely of emotional feelings further suggests that we may be dealing with a system that affords multiple paths for psi to achieve its goals. One might involve the interaction of the emotional system and memory, as we have been examining, and another may be a direct triggering of the feeling component of emotions.

Ultimately there is a need for new and innovative experimental approaches if there is any hope of translating my speculations into hypotheses. Recent years have seen the emergence of exciting new experiments exploring more directly the link between psi and the emotional system in the work of Dean Radin (Radin, 1997, 2004) and Dick Bierman (Bierman, 2004; Bierman & Radin, 1997), which has been reported at these symposia. But this is just the beginning, and more than ever there is a need for parapsychologists to join with neuroscientists in the quest for understanding psychic experiences.

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MAKING SENSE OF THE "EXTRASENSORY": MODELING RECEPTIVE PSI USING MEMORY-RELATED CONCEPTS

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Abstract

Although the physics of psi-mediated information acquisition has not been scientifically elucidated, both spontaneous-case reports and laboratory psi research suggest that some unknown ("psi") process preconsciously activates extant knowledge structures (i.e., constructs, schemata, and their associative linkages, including those to affective and behavioral response). Such activation may thereby elicit overt response. The nature and extent of prior learning relative to specific informational inputs is proposed to determine whether, in what way(s), and in what degree this preconscious activation may initiate behavioral response, including adaptive (or even maladaptive) response. It is proposed that preconsciously-mediated response to psi information is typical and that, contrary to popular assumption, conscious awareness of the triggering information may be the exceptional case. The specifically psychological assumptions of this model of preconscious, potentially adaptive, receptive-psi influence would seem to derive support from cognitive psychologists' findings on preconscious sensory influences, including subliminal ones. The findings on preconscious sensory priming provide suggestions of how preconscious psi functioning might be more effectively conceptualized and studied. Additionally, laboratory findings from psi research are reviewed that suggest how receptive psi interacts with memory-related structures and processes.

"Receptive psi" is said to have occurred when the organism acquires information other than through known sensory processes. The term *receptive psi* may be deemed preferable to the more traditional one,

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extrasensory perception (or ESP), because it does not bias the consideration of these events by suggesting something analogous to the conscious experience associated with sensory-perceptual function, which seems inappropriate. My own theoretical position is that receptive psi is not an intrinsically perceptual function but is, instead, a mechanism that can support adaptive response to preconscious psi-mediated information. This implies that organisms can respond to psi information in an adaptive fashion without the necessity of developing conscious cognitions or perceptions relative to the information to which they are responding. Studies of subliminal and of otherwise-preconscious (e.g., unattended) *sensory* stimulation provide evidence of psychological, and even adaptive behavioral, response to preconscious information, including the creation of goals (see, e.g., Chartrand & Bargh, 1996). Such effects often may occur through the activation of automatic, pre-established associative connections, often concerned with evaluation or affect, which can elicit behavioral dispositions in current situations (e.g., Ferguson, Bargh, & Nayak, 2005). The evidence of adaptive response to preconscious sensory information indicates the existence of cognitive mechanisms that also might support adaptive preconscious response to extrasensory information. If so, contemporary research on adaptive response to preconscious sensory information may usefully inform the investigation of receptive psi. Subsequently, this paper will address how psi information about a given situation (or object) may activate that situation's (or that object's) stimulus-response associations in memory, thereby releasing the associated response, potentially for adaptive purposes, but how psi-activated response tendencies sometimes may have to interfere with or block prior response inclinations in order to produce an adaptive outcome.

A Real-Life Event Illustrating a Conceptual Schema for Adaptive Response to Preconscious Knowledge, including Knowledge Derived from Receptive Psi

The following incident involved my wife and me (for a fuller account, see Stanford, 1982). It illustrates how preconscious information may have an adaptive influence on behavior in everyday life. I do not

know whether the information that apparently elicited the following experience was subtle, unattended, preconscious sensory information or whether this incident involved, alternatively or additionally, information conveyed by psi. Late one evening we were driving eastward along a major expressway in the Borough of Queens, New York City, heading home from an operatic performance. We had soon to turn off that route onto a southbound expressway in order to get home quickly. Although the time was close to midnight, shortly before I was to turn toward the southbound route, I felt a very powerful urge. The urge was to ignore my former driving plan and to continue eastward to look for ducks and geese on a bay along this more easterly route. (We are avid bird-watchers, hence the possibility of this sudden desire.) Just as I was about ready to yield to that urge, I realized how bizarre and inappropriate it really was. How does one study and enjoy study distant waterfowl in darkness? It might even be impossible to spot them. Following the irrational impulse would have involved getting home tired and later than we had wished, after, quite possibly, seeing no waterfowl at all. Having contemplated for a few moments the irrationality of this impulse, it seemed so peculiar that I believed it might have been prompted by implicit (i.e., preconscious) knowledge of some kind of danger ahead on our planned and otherwise optimal route. I had published (1974) a paper detailing a theory of unconscious, adaptive, psi-mediated behavior. What was happening seemed very much like the sudden, impulse-driven responding that I had discussed in that paper as capable of mediating adaptive response to preconscious need-relevant information, including psi-created information. I now felt so certain that my odd impulse was a response to implicit knowledge, quite possibly of an extrasensory character, that I told my wife about the impulse and my conclusion that it might mean there was danger ahead on the originally intended route. I proposed that, despite my urge, I would, in order to test my hypothesis, very cautiously follow the originally planned route to determine if there truly was a dangerous circumstance present there. If so, it might help to explain my strange impulse to deviate from the most appropriate route at what seemed a most inappropriate time. We therefore took the originally planned southbound expressway and exited, slowly and cautiously, at our usual

exit only to find, a very few feet ahead of the exit, lying across much of the access road, a lamp pole that had just been knocked down by another vehicle. There also was considerable glass in the road. Had I gone this route and exited at my usual pace at that late hour with little traffic, I might easily have run my vehicle into or across the fallen pole, damaging it or its wheels and tires and possibly injuring us. My special caution had obviated a potentially bad mishap.

I do not tell this story to prove a point but to illustrate a proposed theoretical principle of which it may be an example: Preconscious knowledge, whether sensory [1] or psi in character, may be presumed to activate previously learned response dispositions related to the implicit knowledge. I knew, ahead of time, both the eastbound and southbound routes home and knew about the ducks and geese that frequent the bay along the eastbound route. We had even, on previous trips, deviated, during daylight hours, from the shorter southbound route because of the possibility of seeing ducks and geese along the eastbound route. Implicit knowledge of danger on the planned route might, therefore, automatically have activated existing knowledge (i.e., a schema) of the alternate route, including its possible attractions. Interestingly, had this impulse struck us during daylight hours, it would not have seemed irrational, I never would have inferred that psi (or other preconscious knowledge) might have been the cause of the impulse, and I probably would have taken the eastbound route home, happily scanning the bay for waterfowl and not objecting to the longer route. This suggests how subtle and fully unconscious may be the adaptive response to preconscious knowledge, at least under suitable circumstances.

Elsewhere I have proposed a detailed model for adaptive response to preconscious psi-mediated (and, also, preconscious sensory) information (Stanford 1990), but full details of that model are beyond

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[1] The preconscious sensory possibility involved in this impulse-to-see-birds incident derives from the possibility that the lamp pole's being knocked over, at nighttime, about 2.5 miles from my car's location might have caused either a flash (or some other signal) to which I paid no attention but that elicited in me preconscious apprehension. At the time I certainly had no experience of having noticed a flash in that direction, but if there had been an unattended one visible at my considerable distance from the accident, it might have had a preconscious effect upon me.

the scope of this discussion. The model does, though, assume that information entering the system in preconscious form by an unknown means labeled "psi" (or the psi process), activates related, preexisting knowledge structures, thereby priming response dispositions (e.g., goals or affect) that are habitually related to such knowledge. These dispositions, then, potentially can mediate adaptive response to that information without there being conscious awareness of it.

Any testable idea must indicate boundary conditions for effects, and this conceptual model of psi function makes a number of very specific assumptions about the psychological circumstances that should inhibit or block the influence of psi upon behavior. These important assumptions about boundary conditions are beyond the scope of this paper, but they and related research have been discussed in detail elsewhere (Stanford, 1990).

This model of psi function has been termed the psi-mediated instrumental response model (PMIR model). That model proposes in some detail how the subtle, but potentially powerful, influence of receptive-psi information (and, also, preconscious sensory information) may manifest in everyday life, based on preconscious, automatic activation of preexisting cognitive structures.

Theories and Research on Preconscious Sensory Priming: Psychological Effects of Unattended Information

The idea of adaptive response mediated by subtle information (psi or sensory) is very plausible in light of contemporary developments in cognitive psychology. Recent theoretical and related empirical developments in cognitive psychology have demonstrated that there exists preconscious sensory priming (i.e., activation) of *goals*, of *evaluations*, and of *perception-related ideas* (such as social stereotypes or trait inference) (see, e.g., the review by Bargh & Chartrand, 1999; see, also, Bargh, 1997) without the individual's becoming aware of the process of this happening. Hence, no attention is devoted to it. Preconscious priming appears to be of short duration, but it can influence mental activity and even overt behavioral response for a brief period of time. The ideas and research just cited involve, of course,

subtle sensory, not extrasensory effects. John Bargh has been one of the leading figures in conceptualizing the role of priming-related automaticity in everyday life, and many of the findings that emerge from his (and his students') laboratory work provide evidence for easily triggered, automatic, mechanisms that can subserve even some fairly high-level mental functions. This recent evidence for preconscious sensory influences documents the existence of the very kinds of mechanisms that I long ago (Stanford, 1974, 1990) proposed could accomplish the proposed adaptive influences of preconscious psi information. This is the case, at least, if we are willing to assume that the preconscious priming of the mental structures that occurs sensorially also can occur due to psi influence. The non-psi theorization and research of a number of investigators has provided substantial experimental evidence that internal, automatic, mechanisms exist that can subserve adaptive needs, mechanisms that have, in a number of instances, shown to be triggered even by extremely subtle, including presumably subliminal, stimulation. This makes it the more plausible that these memory-related mechanisms also may be utilized by psi influence.

As Bargh and Chartrand (1999) noted, some of the automatic mechanisms they have posited do not require prior experience to function, whereas others do require it. Mechanisms that presumably do not require learning include the tendency to mimic behaviors that we perceive in others. On the other hand, many other automatic influences depend upon the individual's learned associations that develop and are regularly used or accessed during life. Some of these learned associations are evoked by social stimuli and some by other kinds of situations. Those involving social stimuli have been more extensively studied by psychologists, especially social psychologists. Information that triggers learned associations can support the automatic activation of goals and motives, if a goal or a motive has earlier become associated with the situation or with the object whose internal representation is being preconsciously (or, of course, consciously) activated. These generalizations have, of course, been developed from studies of sensory priming. The psychologists who have demonstrated them have not studied psi processes.

Evidence of Preconscious Sensory Activation of Learned Associations

Preconscious activation of existing mental structures can activate response dispositions or even activate specific goals. Bargh, Raymond, Pryor, and Strack (1995) examined the hypothesis that men likely to sexually harass women have a strong, pre-existing association between power and sex, unlike men who are unlikely to engage in such behavior. In such a case even subliminal or other subtle sensory cues related to power may activate an inclination toward sexual relations with a woman who is close at hand. The hypothesis of Bargh et al. was that in such men power automatically primes the idea of having sex. In one study they demonstrated that men, known from other evidence to be disposed to sexually harass or to sexually aggress, exhibited automatic associations between power and sex, as assessed by a *subliminal priming methodology*. This automatic association of power and sex was not in evidence with men who were not dispositionally inclined toward sexual harassment. In another study these investigators very subtly primed the idea of power (and contrasted this with a neutral priming condition). Men likely to sexually aggress, as determined by independent evidence, and who had been subtly primed with the idea of power were more attracted to a female confederate who was present at the time than when the prime was neutral. Men who had been exposed to the same power prime but who were not likely to sexually aggress as pre-determined by other evidence failed to show this facilitation of attraction on account of the power prime. *The fundamental message here is that in individuals with pre-existing strong associations between two constructs, even preconsciously activating one construct can activate the other.* Therefore, a situation, including one subtly or unconsciously primed, can activate an associatively related disposition or goal, and that activation may eventuate in actual behavior.

As a more socially desirable manifestation of preconscious priming, consider the fact that priming a sense of socially-based security by subliminally presenting names of personal attachment figures (e.g., the name of a parent), as contrasted with subliminally presenting the names

of close relatives or acquaintances who are not attachment-related, fosters compassion and the readiness to help someone who needs help (Mikulincer, Shaver, Gillath, & Nitzberg, 2005). These findings were observed in both Israeli and USA samples. This research, interestingly, shows the value of preconscious stimuli being custom-selected as personally relevant. Personal attachment figures have been associated in one's life with safety and security. They are presumed by attachment theorists to become incorporated into a *secure base schema*. Subliminal presentation of the name of the significant other may, Mikulincer and colleagues reasoned, produce a sense of personal safety and security that supports altruistic caring that is threatened neither by empathically experiencing others' suffering nor by the level of interdependence that can be engendered by caregiving. I personally wonder if this security-figure-priming-yields-compassion effect might be mediated, partially or alternatively, by entirely unconscious activation of an inclination to abide by the social norms that the named attachment figure had taught the individual in earlier years. Nonetheless, the general principle of eliciting previously learned inclinations by preconsciously activating past associations would still explain these findings, and that is the main point here, not to advocate for or against a specific application of attachment theory.

Another of the methodologically sophisticated subliminal-priming studies of Mikulincer and colleagues illustrates a different facet of personal-association-based priming. Mikulincer, Gillath, and Shaver (2002) demonstrated that a subliminal prime with possible consequences for self-esteem (e.g., *failure* in Hebrew, *nichshal*) can selectively enhance the accessibility of the mental representation of personal attachment-relevant individuals. Presumably it is the learned tendency of think of attachment figures when one feels threatened that underlies this finding. Once again we see the use of personalistic information-often primes or, sometimes, as here, as targets of priming-as having great value in the study of preconscious processes. I strongly suggest the potential value of these personalistic strategies in the study of receptive psi. It comes back to tapping into the associations that the individual has personally learned that relate to feeling, reacting, and striving.

The Possibility of Preconscious Activation of Affect- and Goal-Relevant Dispositions

The research and writings of Bargh and colleagues have strongly emphasized preconsciously apprehended circumstances and unconscious processing in the development of feelings, judgments, and behavior, including goal development (Bargh & Chartrand, 1999). From an evolutionary perspective and in light of contemporary research (see later citations) it makes sense to think that preconscious processing of unattended or weakly-presented sensory information might be particularly attuned to getting the individual ready to respond dispositionally, in terms of approach or avoidance (i.e., positive or negative reaction), to related circumstances that subsequently might be encountered in a less equivocal sensory way. This possibility seems compatible with the outcomes of some recent studies involving subliminal affective priming, including those now to be considered.

Ferguson et al. (2005) showed that subliminal stimulation with affectively positive or negative primes (i.e., nouns with pleasant or unpleasant connotations) can, immediately afterward, influence: (a) the interpretation of ambiguous words (i.e., homographs with positive and negative meanings; Experiment 1); (b) the categorization [2] both of affectively ambivalent social objects (persons or employment categories) and of affectively ambiguous nonsocial objects (Experiment 2); and (c) the tendency to emphasize positive or negative trait judgments [3] of people shown in photographs (Experiment 3). In all three experiments the expected priming effect was observed even though the sole shared element between the primes and the subsequent targets was

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[2] This was not spontaneously-generated categorization. Participants first saw an object displayed on a computer screen and then saw two affectively-distinct category names applicable to that object. They were asked to select the categorization that seemed more applicable to the object and to do so as quickly as possible. They were informed that their responses would be timed. This discouraged rumination.

[3] These were not spontaneously-generated traits. Participants used an 8-point scale to rate the applicability of a named trait. They were asked to give their first response and to decide quickly because their responses would be timed.

affect. The absence of shared semantic attributes between the primes and the targets of priming did not prevent affective priming.

Ferguson et al. advanced the intriguing suggestion that preconsciously-driven affective interpretations of target objects might become encoded into memory and thus affect future reactions and decisions related to those objects. These investigators' bold suggestion of potentially long-term consequences of such priming is novel and worthy of investigation. Many cognitive psychologists believe that the consequences of priming tend to be very short-lived. Of course, preconscious affective priming could have very adaptive significance in the "real world," even if its effects should turn out to be exclusively short lived. Given that the organism seems built to support this kind of priming, evolutionary considerations suggest that it likely has adaptive roles to play outside the laboratory. This adaptive significance could derive, in major degree, from getting the organism inclined, on the basis of "early information" (i.e., weak or very transient), for a response such as flight/fight or approach. As was noted earlier, in real-world settings preconscious affect-related priming may often occur due to brief or inadequate sensory exposure to an object or circumstance of known affective relevance due to previous encounter(s). The cognitive meanings of these objects or circumstances may never enter conscious awareness, but their potential to elicit appropriate affect or an adaptation-relevant response disposition may nonetheless be very real.

Preconsciously-evoked affect and dispositions, as contrasted with priming consequences of a more cognitive character, may, because of their action-relevance, have special importance in relation to adaptation. This should not, however, be taken to imply that the consequences of affect-related preconscious primes – and perhaps, especially, their initial consequences – need always be conscious. Indeed, contemporary research is providing evidence that those early consequences of affect-relevant preconscious stimulation may at least sometimes be nonconscious (Winkielman & Berridge, 2004; Winkielman, Berridge, & Wilbarger, 2005; Winkielman, Zajonc, & Schwarz, 1997). The unconscious, automatic character of early response to preconscious, affect-relevant inputs may be important in adaptive response, for their speed and automaticity presumably help to get the organism ready, in

timely fashion, for adaptive response. This may happen even before there is an opportunity for consciousness to intervene in this adaptation-relevant response. This is not, however, to suggest that such early, preconsciously primed influences always will be unconscious. Research has not as yet illuminated the boundary conditions for their being unconscious.

Let us briefly consider some of evidence suggestive of the possibility that consciously-perceived affect may not always accompany successful subliminal affective priming. In two studies by Winkielman et al. (1997), despite evidence of subliminal affective priming, participants reported themselves to be unaware of feelings being elicited by the primes, and some ancillary evidence would seem to have supported the interpretation of the lack of awareness of feelings associated with the affective primes. Although these authors were unable on the basis of their studies to decide between some competing theoretical interpretations of their data, they were able to conclude that future consideration should be of models of affective priming that do not imply, and hence do not require, mediation of the observed effects by consciously experienced feelings. Two more recent studies by Winkielman et al. (2005) may be deemed compatible with that conclusion. They examined three conceptually interesting predictions about subliminal affective priming. This work involved happy, neutral, and angry faces as subliminal primes. It examined predicted effects of the affective priming upon the pouring, consuming, and affective response to a novel sweetened drink. Mood and arousal also were assessed. These investigators had predicted that (a) the affective priming would be most effective on measures (e.g., amount consumed and desirability of the drink) that reflect the drink's pleasure-related qualities; (b) the influence of the affective priming would be accomplished without changing consciously perceived mood; and (c) the effect of a prime, be that prime happy or angry, upon behavior would be greatest when the participant was thirsty. The two types of primes were expected to have opposite effects, which they did, namely enhanced hedonic value of the drink with positive primes and reduced hedonic value when the prime was negative. Those effects were, as expected, moderated by degree of thirst. (Discussion later will return to

the potential conceptual significance of this moderation phenomenon for a central thesis of the present paper.) Perhaps of immediate special interest here is the fact that no evidence was found that the affective priming influenced any of the numerous measured attributes of subjective experience. These observations are, at least, compatible with the supposition that the affective consequences of the primes were unconscious.

The Winkielman et al. (2005) work may have special relevance for the central theme of this paper, namely that preconscious activation of mental representations can eventuate in actual changes in behavior, not just changes in arousal or in internal experience. In the Winkielman et al. (2005) work, preconscious affective priming resulted in changes in overt, motivated behavior, namely the drinking of favored, sweetened water. My PMIR model, which describes a theory of adaptive response to preconscious psi-mediated activation, requires that the effects of preconscious activation go beyond just internal consequences. It requires that preconscious activation must be able to generate behavior (or changes in behavior) that allow either approach to a favorable event or outcome or avoidance of an unfavorable event or outcome. The fact that the Winkielman et al. (2005) paper, which used subliminal affective priming, showed clear behavioral consequences of primes may be deemed evidence-from non-psi research, of course-that the psychological machinery exists that can mediate unconscious effects on actual need-related behavior. In fact, a number of receptive-psi studies have produced evidence that effects such as this can occur via psi activation (reviewed by Stanford, 1990). My PMIR model, which has been the major impetus for this kind of work, made the assumption that the kinds of psychological (non-psi) processing occur in the organism that could support adaptive, preconscious psi-mediated response. Contemporary findings from cognitive psychology are supplying substantial evidence that the mechanisms needed for automatic, nonintentional, preconsciously-driven-but potentially adaptive-response exist, questions of the reality of psi all aside.

There is substantial evidence that affect- or evaluation-related consequences of stimuli may have their own, privileged channel of processing that does not depend upon cognitive evaluation of the

affective relevance of the stimulus and that, indeed, may be elicited while a stimulus is still preconscious (for a brief review and related references, see Zajonc, 2001; also, Zajonc, 1980, for a fuller view of this theory). Again, this makes sense, adaptively speaking, given the need for early and rapid determination of the potential of objects relative to personal threat or benefit.

Adaptive Response to Preconsciously Apprehended Circumstances Must Somehow Interact With the Ongoing Needs and Goals of the Organism

The PMIR model of adaptive psi-mediated response to preconscious information posits that the ongoing needs of the organism must somehow interact with and influence response to incoming information, including preconsciously apprehended information. To go back to my personal anecdote early in this paper, my wife and I were driving home late at night and planned on taking a short route to get home early. It would appear that when I (apparently) became influenced by preconscious knowledge of the accident up the planned route, the influence of that knowledge emerged, not as an entirely new goal and certainly not as any direct knowledge of the danger, but as a goal that included getting home while enjoying birds along an alternate route as we traveled home (albeit by a somewhat longer route). This was possible because I had stored in memory a schema (mental map) of the area that indicated alternate routes and their known consequences.

For psychologists, like parapsychologists, making sense of how the organism might adaptively respond to preconscious, implicit knowledge will have to consider how this kind of subtle priming can interact with the ongoing goals and needs of the organism. It therefore was with considerable pleasure that I read the previously cited studies by Winkielman et al. (2005). These investigators broke important ground by examining, guided by theory, how the individual's motivation (level of reported thirst) interacted with the affective-priming manipulation (angry, neutral, and happy faces). This kind of non-psi research provides psychological evidence for the interaction of preconscious activation with ongoing needs to influence behavioral outcomes.

Evidence for this kind of psychological functioning provides a degree of support for an underlying assumption of the PMIR model, which requires that preconscious affect-related activation somehow interact with the ongoing needs of the organism in order for adaptive behavioral response to occur. Of course, much more research is needed on this important matter, both by psychologists and by psi researchers interested in the potentially adaptive, unconscious functions of psi. It is worth noting that this problem area represents an instance in which psi researchers might have the opportunity both to contribute to cutting-edge psychological knowledge and to psi-related knowledge. Indeed, additional psychological knowledge of this area is a prerequisite for advancing psi research (of this particular kind) and for refining existing models (e.g., the PMIR model). That kind of multi-disciplinary contribution is not to be despised!

Priming Can Do More Than Elicit a Response: Elicited Responses Can Attenuate or Block Other Responses

Of substantial pragmatic and theoretical interest is the finding by Mikulincer and Shaver (2001) that subliminal priming of the secure base schema (i.e., using primes that all relate to socially-derived security or closeness) can reduce the normal tendency to react negatively to out-group individuals, even when one's self-sense has been affronted by an experience of failure, something that can enhance out-group discrimination (e.g., Fein & Spencer, 1997). The security-and-closeness primes, which are intended to elicit the feelings that one experienced with an attachment figure, can be presumed to have helped in the self-affirmation process. (Having been the recipient of caring by an attachment figure presumably makes one feel that one must have been worthy of such care. That feeling presumably was activated by the security-and-closeness primes.) Feeling positive about oneself, which is not the same as being in a good mood, can reduce the tendency toward discrimination against outgroups (Fein & Spencer, 1997). Very important, Mikulincer and Shaver showed that the effect of the secure base primes was not due to mood elicited by those primes. More generally, this study illustrates the very important point that priming can

elicit previously learned responses that can modify or even obviate one's usual responses to present situations.

The latter is a conceptually important conclusion that bears upon a major assertion of my PMIR model. According to this model, psi can have adaptive effects either by directly fostering responses that themselves are adaptive or by eliciting competing responses that tend to counter or to block inclinations toward a response that would be non-adaptive, given the situation at hand. (That is presumed to have been the case in the go-see-the-birds impulse anecdote mentioned earlier.) Thus, if the results of the work of Mikulincer and colleagues (and other similar work in the non-psi literature) can be generalized to psi-related priming, they lend a degree of credibility to the proposal that psychological mechanisms exist whereby psi-mediated adaptive response might sometimes occur through preconscious psi-mediated facilitation of ready responses that counter potentially maladaptive responses that otherwise would have occurred. (I also have collected some additional spontaneous cases of possible PMIR that might be deemed to be of this sort.) The Mikulincer and Shaver (2001) work makes it clear that preconscious sensory priming can facilitate responses than can attenuate or block the appearance of otherwise-expected responses. This, of course, is an important development in the study of preconscious sensory priming, the relevance to the PMIR model aside.

This idea of adaptive psi-mediated response competition was directly investigated in a study that led up to my development of the PMIR model (Stanford, 1970). This study provided evidence, in a context that carefully controlled for sensory cuing, that psi information could block the recall of information that was clearly stated in a story heard shortly before by the research participants. I call this "psi incursion." The study had been set up such that responding in accord with psi information, rather than on the basis of actual memory would, on some trials, give the subject a presumably desired advantage.

Summary and Integration of Conclusions to This Point

The already well-documented conclusion that preconscious sensory priming can influence thought, affect, motivation, and goal-setting

indicates that mechanisms exist that might respond similarly to psi-initiated preconscious information. If so, the possibility of adaptive preconscious psi-mediated response becomes more understandable and, psychologically more plausible. Granted, then, the assumption that psi-mediated priming of memory structures is possible and should operate by similar laws as preconscious sensory priming, it may not be surprising that a number of the assumptions and implications of the psi-mediated instrumental response (or PMIR) model already have received some degree of support from studies of preconscious psi function (see Stanford, 1990), as well as from studies that involve a deliberate effort to consciously use receptive psi. Discussion shortly will turn to some of those efforts, for they, too, relate to memory functions, the topic of this conference.

The effort to generalize the findings of preconscious sensory priming to preconscious receptive psi might be very useful. It is evident that doing so will require psi investigators to more carefully study the memory structures (e.g., schemata and associative linkages) of their individual research participants, using techniques used for such purposes in non-psi research. The more information that can be gleaned about the individual participant's existing cognitive structures, including associative linkages, the better will be the chance, in my view, of using those cognitive structures to produce evidence of preconscious (or even conscious) receptive psi. Maximizing the results of studies with preconscious priming, either sensory or extrasensory, may be aided by (a) using stimuli that are individually meaningful to the participant and (b) learning something in advance about the nature of this meaningfulness and about the motivation and affect-related memories with which they are cognitively embedded.

The possibility of adaptive (or even potentially maladaptive) response to unattended or even subliminal sensory information was, up to very recently, considered by most experimental psychologists as something close to unthinkable, but it has found recent empirical support from studies of the preconscious priming of goals and affect. Many psychologists' views about such matters recently have changed, for two reasons (see, e.g., the review by Kihlstrom, 1987, in the journal Science): First, conceptual developments in cognitive psychology,

related largely to ideas and work on procedural knowledge and on connectionist models (e.g., parallel distributed processing or PDP), have made it plausible that fairly sophisticated processing can occur without the need for attention to guide it. Second, experimental findings in cognitive psychology, including in cognitive social psychology, have shown that both goals and affect may be activated preconsciously. We need to apply these methods and principles to research on receptive psi, and, in fact, this kind of things has already been going on, albeit sporadically, for some years by several investigators, as reflected in lectures presented here by other participants and by the studies that I have reviewed in depth elsewhere (Stanford, 1990). Let us now see what studies of receptive psi seem to be telling us about the various roles of memory in receptive-psi function.

When You Try to Make a Preconscious Function Provide Conscious Knowledge, Memory Plays a Variety of Roles

How Does One Know When Receptive Psi Has Occurred (or Can One Know)? It Helps to Know How Unexpected Is the Response

Unlike the sensory modalities, receptive psi function has no specific or unique qualitative representation in consciousness. What is consciously represented, if anything is, appears to be the consequence of preconscious activation of aspects of cognitive structures that exist in memory (Roll, 1966; Stanford, 1974, 1990; Tyrrell, 1946). This surely is a major reason that it is extremely difficult for subjects in deliberate, conscious receptive-psi tasks to have reliable insight into the accuracy of their responses.

In a substantial proportion of spontaneous cases of potential receptive psi there is, nonetheless, a sense of conviction that an unexplained communication has occurred (Rhine, 1967, Ch. 5, “Belief in the Truth of ESP”). This sense of conviction may derive from an inference that one’s prior knowledge and experiences cannot possibly have elicited the experience. This may be presumed to lead to the interpretation that psi influence has occurred. I know of many spontaneous experiences where this kind of attribution appeared to

have played a role, and similar inferences on the part of subjects can occur in laboratory work in which they try to extrasensorially perceive a work of art or a photograph. Really unexpected experiences often seem to elicit the inference that psi might have produced the experience. Inferences aside, new information, including psi information, entering the mind may logically be expected to elicit unanticipated experiences or otherwise unexpected responses, precisely because new information is forthcoming. Hence, the relative novelty of response sometimes may be a marker of informational, including psi-informational, influence.

In line with this consideration, researchers often have found that in forced-choice receptive-psi tests, response choices that occur relatively infrequently are more likely to be correct responses. (Forced-choice ESP tasks involve the subject in indicating which of two or more alternative target categories is thought to be the target on a given trial.) There is substantial evidence that low-probability responses on receptive-psi tasks often show more statistical evidence of having been psi-mediated than do higher-probability response alternatives, and this seems to be the case whether or not the task requires the subject to intentionally try to use receptive psi (reviewed by Stanford, 1975). There have been many reports in the literature that have supported this response-bias hypothesis, but there have been some inconsistencies (i.e., failures to find the effect). Also, these findings have been mostly correlational in character. Palmer has reviewed some of these response-bias findings earlier in the symposium. In what probably is the most systematic examination of this hypothesis, I (1970), in a study of preconscious, non-deliberate receptive psi, experimentally manipulated the likelihood of a subject making a counter-bias response. This was a study of how memory might be modified by receptive psi, and the clarity of some information in the remembered story had been experimentally manipulated. When subjects' responses were contrary to information clearly stated in the story, those responses were significantly accurate. Those same responses also were significantly more accurate than counter-story responses related to something that had only been suggested in the story. The validity of the response bias hypothesis also was strongly supported by the fact that subjects shown by an

independent test of incidental memory to have better incidental memory scored better on their counter-bias responses than did subjects who did less well on the same memory test. So far as I know, this experiment is the most detailed and conceptually incisive effort to examine the response-bias hypothesis. It provided strong, consistent, convergent evidence in support of its operation in a study of nonintentional or preconscious receptive psi, and support for the response-bias prediction has been reported by numerous other investigators, albeit generally in correlational contexts. I know of a single unsuccessful effort (Sheargold, 1972) to conceptually replicate some of my (1970) findings, but the results of his study should be considered in light of the study's methodology, which diverged from my own in some very fundamental ways (discussed in detail in Stanford, 1990).

Nonintentional, preconscious tests of receptive psi may, in theory, be among the best opportunities to observe this response-bias effect. In such tests, participants do not tend to become ruminative about their responses, whereas in deliberate (or explicit) tests of receptive psi, this rumination tends more strongly to occur and perhaps the more so in forced-choice tests. Participants in forced-choice receptive-psi tests often become very analytical and reflective (see next section on constraints upon spontaneity). As a consequence, what may appear to have been a low-probability response, based on earlier trials, could suddenly become a high-probability response if the subject decides "I have not called enough of that target." This kind of thing could well dilute the response-bias effects that may be observed in tests that do not cause participants to become so strongly ruminative about the responses they are making.

As a consequence of these considerations, a relatively high success rate with a given target category in a receptive-psi task should not, by itself, be taken to imply that this target category is particularly communicable by psi (i.e., that someone has high psi-sensitivity to it). It may be, instead, that the psi influence on producing this response category is particularly detectable simply because responses of that kind are not diluted by false alarms, thanks to this response being relatively rarely produced.

It also is very important to note that because a response category has a relatively low probability in a given receptive-psi task situation does not mean that it is not a "ready response" in the sense of its being readily activated (e.g., easy to prime). Being readily activated and even having been primed does not mean that a given construct necessarily will control responding in a given situation at a given moment. Many other factors will influence response selection in a conscious, explicit receptive-psi task, such as presuppositions and pseudo-logical constraints (discussed in the next section). Additionally, competition from an even slightly more ready response category could reduce the probability of actual production of a given response, however easily activated that response (or response category) might be.

Spontaneity is Important to the Expression of Receptive Psi: The Importance of Obviating Rumination

Sometimes subjects in forced-choice receptive-psi tasks think about their earlier responses and constrain subsequent responses on the basis of earlier ones. For example, if they are guessing a series of twenty-five geometrical symbols randomly selected from five alternatives, they may tend to monitor how many of each they have called, not allowing themselves to call too many or too few of each. They may, thereby, tend effectively to balance their responses across the alternatives. This is, of course, imposing a seemingly logical constraint on their spontaneity on a given trial. There is substantial evidence in the receptive psi literature that imposing these kinds of pseudo-logical constraints on one's guessing behavior can prevent the subject's test scores from deviating far from chance average, whether in the positive or negative direction. Constraining the present response on the basis of one's memory of earlier responses can therefore have adverse consequences for performance on receptive-psi tasks. I (1975) reviewed this evidence and found five studies that experimentally contrasted instructions to make calls independently and instructions that encouraged call balancing. Four of the five studies exhibited this effect. Additionally, there has been some correlational work that supports the conclusion that call balancing can interfere with the expression of receptive psi.

Freedom from call balancing may also allow the use of receptive psi in the case of outstanding receptive-psi subjects. For example, Morris (1972) reported that in an outstanding subject's very high-scoring series he generally made no attempt to balance his calls. The cognitive constraints produced by a tendency to balance calls should be most evident later in the test run where the subject tends to run out of options to select a given response. It is therefore interesting that in additional work with this same outstanding subject, Stump, W. G. Roll, and M. Roll (1970) noted that he in some series showed strong within-run declines in his receptive-psi performance and that in these same series he also showed a strong tendency to balance his calls. Conversely, in a series where the within-run decline was absent, the subject showed a decreased inclination to balance calls.

Another kind of rationalistic constraint that may, based on purely correlational (and, hence, causally inconclusive) evidence, interfere with receptive psi is that subjects often call a given target category far fewer times in a row than would be expected by chance. For example, they tend to call fewer "doubles," "triples," etc. (e.g., two or three stars in a row) than would occur by chance in the actual target sequence. This tendency appears to result from subjects' misconceptions about random sequences, for they would seem to act as though the recurrence of a given target category is unlikely on adjacent trials. They are engaging in a form of what is known as "the gambler's fallacy." Given that targets are randomly selected, this kind of pseudo-rationality could impair performance, potentially interfering with the expression of receptive psi. Morris (1972) noted that on high-scoring, as contrasted with chance-performance runs, an outstanding subject tended to call the same symbol twice in a row much more frequently. He performed more successfully when he was able to get away from this form of sequential constraint. Of course, this observation is both anecdotal and correlational, so conclusions about causation cannot be warranted from it, but it does suggest the potential value of experimentally manipulating this particular form of rationalistic sequential constraint and observing its consequences, if any, for receptive-psi performance.

Numerous individuals known to have exhibited some evidence of receptive-psi ability have expressed to me the opinion that inclinations

to constrain responding on the basis of things one knows (or believes one knows) tend to inhibit the free expression of receptive psi. It now seems clear on the basis of laboratory evidence that placing rationalistic constraints on how one responds in a forced-choice test, based on memory of earlier responses, can reduce the likelihood of producing statistical evidence of receptive psi. Although researchers have tended to focus on the adverse consequences of imposing rational constraints in forced-choice receptive-psi tasks, it seems reasonable that rational and other memory-related response constraints could limit the opportunities for receptive psi to act in a variety of other situations, including in free-response studies (e.g., trying to perceive pictures by psi), in experimental studies of non-intentional psi-mediated adaptive response, and in the operation of preconscious psi influences in everyday life (in the latter regard, see Stanford, 1990).

Some Methodological Considerations in Modeling Receptive Psi in Everyday Life

Effective Modeling of Receptive Psi Function May Benefit from Knowing the Mind of the Respondent

As was suggested earlier, effective modeling of preconscious receptive psi influences might profitably involve using psi target materials (or circumstances) that have personal relevance, ideally, of a known kind, to the individual respondent (subject). Also, it may be very useful to employ dependent measure(s) that plausibly can be assumed to reflect response dispositions cognitively linked, in the mind of the individual respondent, to the target material (i.e., to "the preconscious psi prime"). Responses of that kind should therefore be easily activated by that target material. Selection of idiosyncratic materials as stimuli and expected idiosyncratic responses to them as dependent measures is presently being used in some non-psi research by psychologists who wish to study the role of preconscious influences in social life. One very important example of this is in the work of Susan M. Andersen. She has intensively studied unconsciously mediated cognitive and affective consequences, in social perceivers, of verbally-described charactero-

logical resemblances between significant others in their lives and new individuals who share characteristics with those significant others. She terms this *transference* and regards it as a very important element in social cognition. This research is complex and of large scale, so no serious effort will be made to describe it here. Andersen assumes that transference requires neither conscious recollection of the significant other (in response to information about the new individual) nor even an attentional focus specifically on the cues shared by the new individual and the significant other. The veracity of her hypotheses about transference has been supported through her unique, but now standardized, methodologies, as well as through the use of subliminally exposed verbal descriptors (Glassman & Anderson, 1999). Anderson describes her general methodological approach as the "use of idiographic methods . . . in a nomothetic, experimental design" (p. 629). Her methodology is, as a consequence, a clever meld of two very different research traditions. It is one that might profit both psychologists and parapsychologists who study it. I find it particularly interesting in its strategies for controlling potential extraneous variables that may arise. A good overview of her program and of the social-cognitive theory that it has guided and been shaped by it may be had in a paper by Andersen and Chen (2002).

Applying idiosyncratic information in experimental paradigms obviously requires the acquisition of advance information about the psychology of the individual participant, which can inform the kinds of materials and measures one would use in one's study. It also requires thoughtful gathering and use of such information in ways that do not tip off respondents to one's expectations in the research.

One way to gain information that might be useful for modeling preconscious psi would be to get to know how the individual respondent responds to preconscious sensory information of the kinds to be used in the extrasensory phase of the word. Perhaps one could start by selecting even the predictive preconscious sensory stimuli on the basis of interviews, perhaps structured ones, of the individual in regard to things, people, and issues of central importance in his or her life. Then one could, using the pre-selected stimuli in a preconscious sensory paradigm, attempt to predict extrasensory response to them.

This is just one off-the-cuff idea, and it is mentioned here perhaps more as an incentive to invention than as a strategy to guide immediate research.

In regard to this general approach, learning how an individual responds to preconscious (e.g., subliminal) information of the selected kind(s) might be especially useful in predicting receptive-psi-task outcomes, given that preconsciously and consciously presented materials may elicit different outcomes (e.g., Cheesman & Merikle, 1986). This is not the place to elaborate at length on the intriguing possibilities in this approach, but some of them are discussed elsewhere (Stanford, 1990).

Effective Modeling of Adaptive Preconscious Receptive Psi Will Require Remodeling the Traditional Research Methodologies

Research studies intended to illuminate relationships of memory functions and receptive psi have, in my view, been rather uneven in terms of the construct validity of their methods (Stanford, 1990). Indeed, in some cases the conceptual articulation of the problem to be addressed left considerable to be desired. If research methods are to be effective in illuminating how receptive psi acts upon information stored in memory, activates related associational links, and fosters adaptive (or, in some cases, maladaptive) response through such activation, the adopted methodologies may need, far more directly than in most of the existing studies, to model the situations in which adaptive psi influence might emerge in the real world. In that world, persons are not, with very few exceptions, trying to gain conscious extrasensory access to specific kinds of information. Historically, though, the vast majority of laboratory receptive-psi studies have involved efforts at explicit psi testing in a perceptual/cognitive paradigm. These are “use your psi” paradigms.

The result may be an overlaying of the ordinary milieu of receptive-psi function with the manifold cognitive and attentional consequences of (a) trying to make psi do something at our deliberate bidding and (b) directing it to access specific information, regardless of its relevance to the individual’s needs. Perhaps investigation could profit by considering the possibility that cognitive/perceptual manifestations of psi may be

atypical manifestations and should not be taken to indicate the intrinsic direction that receptive-psi events tend to take. In recent years a number of psi researchers appear to have shifted toward less purely cognitive/perceptual assumptions, and this has tended, in some degree, to change the methods employed. The adaptive significance of real-world receptive psi may be more a matter of acquiring valued outcomes or of avoiding undesirable ones than of fostering perception-like cognitions, unless the latter be necessary for such ends. I am reminded, in this regard, of Robert Morris' (1967) study of goldfish in a tank, one of which, on a given trial, would shortly be picked up and held momentarily in a net. The fish were scored for swimming activity for a certain period and then a random selection was made of the one to be picked up. It tended to swim more than the others. Psi may function in support of action relative to real or potential adaptation- or survival-relevant circumstances.

One paradigm that would be useful for the investigation of possible preconscious adaptive psi is a situation in which an individual is not instructed to try to use psi but may be able, entirely unconsciously, to profit from it either to encounter an affectively positive situation or to avoid an affectively negative situation. This models real-world opportunities for adaptive-psi influences on behavior. This is the kind of paradigm that generally has been used in a number of studies of so-called psi-mediated instrumental response (PMIR; reviewed by Stanford, 1990).

Short of such an effort to study wholly unconscious psi-mediated adaptive response, one might use, at least, a paradigm in which someone is not deliberately trying to perceive information. An example would be work that I have done using word-association paradigms as vehicles for the expression of the possible role of receptive-psi in activating memory structures (e.g., constructs and schemata). In such a paradigm the participant may know that an effort may be made to transmit psi information to him or to her, but he or she is assured that it can happen as an automatic influence, one that will be expressed in the responses in the word-association task (Stanford, 1973). Other potentially useful paradigms include efforts to monitor physiological responses to information not sensorially exposed but that has relevance

to the subject's needs, goals, or interests. A number of such studies by a variety of investigators have taken this approach with at least some degree of success.

Of course, there never is a single answer to what is a good methodology. The method adopted must be one suitable to address the question being asked. From this perspective, all the methods that have historically been used to study receptive psi have potential value. Even the often-despised forced-choice methods of old-fashioned receptive-psi research have some utility, but the potential usefulness of any method depends on the characteristics of the method itself. The forced-choice paradigm has serious limitations for addressing many questions of interest to psi researchers, but that is not to say it is without its uses. I have, for example, used it to demonstrate the consequences of rationalistic constraints on receptive-psi performance (as cited earlier). The problem comes in getting stuck in a paradigm and never asking whether there may be better methods to address a specific research question.

The effort to study preconscious receptive psi might profit by using paradigms much closer to those used by cognitive psychologists to study effects of preconsciously presented information, and this trend already has begun. For such research to be maximally effective, investigators must acknowledge that methodologies, however technologically or statistically sophisticated they may be, are genuinely useful only when the work to be done with them is guided by serious conceptualization and careful planning. Conceptual development must accompany methodological sophistication and must inform the adoption and development of new methods.

Historically, the tendency to think of receptive psi as unquestionably-obviously and intrinsically-a perceptual function that struggles for conscious manifestation may have held back discovery of important facts about the events being studied. There are other ways to regard receptive-psi events, and it can be very useful deliberately to create alternative explanations or models for what we study, at least if those explanations are empirically testable by virtue of their making clear, distinctive predictions about what one should observe and under what circumstances. We have seen a number of such efforts in psi

research. The failure to examine assumptions can hold investigators captive to wrong ideas and can seriously undermine scientific progress. Fundamentally important is to recognize that we do work from assumptions, however deeply embedded and implicit they may be, not from knowledge of reality. Sometimes implicit assumptions are not easy to recognize, but once they are recognized it becomes possible to create new visions of reality and to envision ways to test them.

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THE MISUSE OF MEMORY IN PSI RESEARCH

Stephen Braude*

Introduction

One of the most persistent conceptual errors in philosophy, psychology, and neurophysiology is the attempt to explain memory by means of memory traces (sometimes called "engrams"). The underlying problems are very deep and difficult to dispel, and as a result, trace theories are quite seductive. In fact, in the cognitive sciences this approach to memory is ubiquitous and is almost never seriously questioned (for representative samples of the view, see, e.g., Damasio, 1996; Gazzaniga et al., 1998; Moscovitch, 2000; and Tulving and Craik, 2000). If doubts are raised at all, they typically concern how trace mechanisms are implemented or what the physical substrate of traces might be, not whether something is profoundly wrongheaded about the very idea of a memory trace. Moreover, positing memory traces is one aspect of a larger explanatory agenda that prevails in the behavioural sciences – namely, the tempting but ultimately fruitless strategy of explaining human behavior as if it is emitted by, and wholly analyzable in terms of, processes occurring within an agent. And one reason that agenda is so difficult to overturn is that in order to present a viable alternative, one must outline a very different approach to the analysis and understanding of human behavior.

But that last task goes well beyond the scope of this paper. My more modest goals here are (1) to summarize the main reasons for thinking that the concept of a memory trace is, not simply useless, but actually incoherent, and (2) to show, only briefly, how analogous concepts have crept insidiously into various areas of parapsychological theorizing, especially in connection with the evidence for postmortem survival-for example, speculations about cellular memory in transplant cases and

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genetic memory in reincarnation cases. Similar problems also undermine theorizing in areas often related to parapsychology – for example, Sheldrake's account of morphic resonance.

Why Traces?

Suppose I meet my old friend Jones, whom I haven't seen in twenty-five years. How is it, we wonder, that I'm able to remember him? Many believe that I couldn't possibly remember Jones without there being something in me, a trace (presumably a modification in my brain), produced in me by my former association with Jones. Without that trace, that persisting structural modification in my brain, we'd apparently have causation over a temporal gap. We'd have to suppose that I remember Jones now simply because I used to know him. And to many, that looks like magic. How could something twenty-five years ago produce a memory now, unless that twenty-five-year gap is somehow bridged? So when I remember Jones after twenty-five years, we're tempted to think it's because something in me now closes that gap, linking my present memory to my past acquaintance with Jones.

Now parenthetically, I have to say that it's at least controversial (and in many instances rather naive) to suppose there's something wrong with the idea of causation over a temporal gap. Gappy causation is a problem only on the assumption that the only real causes are proximate causes (i.e., that cause and effect must be spatiotemporally contiguous). But that's a thread I can't pursue here. Positing memory traces is problematic enough quite apart from its underlying questionable picture of causation.

So, let's return to the motivation for asserting the existence of memory traces. Notice that traces aren't posited simply to explain how I happen to be in the particular states we identify as instances of remembering – for example, my experiencing a certain mental image of Jones. They're supposed to explain how memory is *possible* in the first place. The idea is that without a persisting structural modification in me, caused by something in my past – in this case, presumably, a physiological representation of Jones, no state in me *could* be a memory of Jones. So if after twenty-five years I have a mental image of Jones, the only way that image could count as a memory of Jones

would be if it had the right sort of causal history. And the right sort of causal history, allegedly, is one that spatially and temporally links my present experience with my past acquaintance with Jones. So my image of Jones counts as a memory of Jones only if (1) there's a trace in me, caused by my previous acquaintance with Jones, and (2) the activation of that trace is involved in producing my present image of Jones. So mental images of Jones might be possible without that sort of causal history, but they wouldn't then be instances of remembering.

History has proven that this general picture of remembering is initially very attractive. But it gets very ugly very quickly, as soon as one asks the right sorts of questions. (In my view, this is where philosophy is most useful, and often the most fun: showing how claims which seem superficially plausible crumble as soon as their implications or presuppositions are exposed.) What eventually becomes clear is that the idea of memory as involving *storage is* deeply mistaken, and that the mechanism of storage, memory traces conceived as representations of some kind, can't possibly do the job for which they're intended. This is actually an enormous topic and one of the most interesting subjects in the philosophy of mind. But since this issue is both vast and only part of what I want to discuss, I can't do more here than outline a few of the problems with the concept of a memory trace and indicate where one might look for additional details. (For extended critiques, see Bennett and Hacker, 2003; Braude, 2002; Bursen, 1978; Heil, 1978; Malcolm, 1977.)

More Preliminaries

The first thing to note is that the problems with the concept of a memory trace are *hardware-independent.* It doesn't matter whether traces are conceived as mental or physical, or more specifically as static, dynamic, neurological, biochemical, atomic, subatomic, holographic (á la Pribram), nonspatial mental images, or (as Plato suggested) impressions in wax. No matter *what* memory traces are allegedly composed of or how they're purportedly configured, they turn out to be impossible objects. Memory trace theory requires them to perform functions that nothing can fulfill. So my objections to trace theory have nothing to do specifically with the fact that those theories are typically

physiological or physical. Rather, it's because they're *mechanistic* and (in particular) because the mechanisms they posit can't possibly do what's required of them.

Before getting into details, I must deflect a certain standard reaction among scientists to the sort of criticisms I'm making here. Many have complained to me that, as scientists, they're merely doing empirical research, and so it's simply beside the point to argue, *a priori*, that their theories are unintelligible or otherwise conceptually flawed. However, I'm afraid that this response betrays a crucial naiveté about scientific inquiry. There is no such thing as a purely empirical investigation. Every branch of science rests on numerous, often unrecognized, abstract (i.e., philosophical) presuppositions, both metaphysical and methodological. These concern, for example, the nature of observation, properties, or causation, the interpretation, viability, and scope of certain rules of inference, and the appropriate investigative procedures for a given domain of phenomena. But that means that the integrity of the discipline as a whole hinges on the acceptability of its root philosophical assumptions. If those assumptions are indefensible or incoherent, that particular scientific field has nothing to stand on, no matter how attractive it might be on the surface. And I would say that several areas of science, as a result, turn out simply to be bad philosophy dressed up in obscurantist technical jargon, so that the elementary nature of their mistakes remains well-hidden. Memory trace theory is just one example of this. And I'd argue that today's trace theories of memory, for all their surface sophistication, are at bottom as wrongheaded and simplistic as Plato's proposal that memories are analogous to impressions in wax.

Two more disclaimers, before outlining my objects to trace theory. First, when I say that the concept of a memory trace is incoherent or that trace theory is conceptually naive in certain respects, I'm not saying that trace theories – or the scientists who hold them – are stupid. To say that a proposal or concept is nonsensical or incoherent is simply to say it makes no sense. Now although the world isn't suffering a shortage of stupidity, not all nonsense is stupid. In fact, the most interesting nonsense is *deep* nonsense, and it's something which can all too easily deceive even very smart people. That's because the problematic

assumptions are buried well below the surface and require major excavation.

Second, I've learned over the years that when I outline my objections to trace theory, many hear me as suggesting that the brain has nothing to do with memory. I'll say a bit more about this later, but for now I'll just note that I'm saying nothing of the kind. In fact, let's overlook for now complications to all physiological cognitive theories posed by the evidence for postmortem survival and restrict our attention to embodied humans. In those cases, clearly, the capacity to remember is causally dependent, not simply on having a functioning brain, but probably also on changes to specific areas of the brain. However, it's one thing to say that the brain *mediates* the capacity to remember, and another to say it stores memories. The former view (more likely the correct one) takes the brain to be an instrument involved in the expression of memory; the latter view turns out to be deeply unintelligible. For a very limited analogy, we can say that while a functionally intact instrument may be causally necessary for performing a musical improvisation, the music is not stored in the instrument (or anywhere else).

The Horns of a Dilemma

So why is the concept of a memory trace incoherent? Let's begin with an analogy (drawn from John Heil's outstanding critique of trace theory-Heil, 1978). Suppose I invite many guests to a party, and suppose I want to remember all the people who attended. Accordingly, I ask each guest to leave behind something (a trace) by which I can remember them. Let's suppose each guest leaves behind a tennis ball. Now clearly I can't use the balls to accomplish the task of remembering my party guests. For my strategy to work, the guests must deposit something reliably and specifically linked to them, and the balls obviously aren't differentiated and unambiguous enough to establish a link only with the person who left it.

So perhaps it would help if each guest signed his/her own tennis ball, or perhaps left a photo of him/herself stuck to the ball. Unfortunately, this threatens an endless regress of strategies for

remembering who attended my party. Nothing reliably (much less uniquely and unambiguously) links the signature or photo with the guest who attended. A guest could mischievously have signed someone else's name, or left behind a photo of another person. Or maybe the signature was illegible (most are), or perhaps the only photo available was of the person 25 years earlier (e.g., when he still had hair, or when he had a beard, wore eyeglasses, and was photographed outdoors, out of focus and in a thick fog), or when he was dressed in a Halloween costume or some other disguise.

But now it looks like I need to remember in order to remember. A tennis ball isn't specific enough to establish the required link to the person who left it. It's not the sort of unambiguous representational calling card the situation requires. So we supposed that something else might make the tennis ball a more specific link-a signature or a photo. That is, we tried to employ a secondary memory mechanism (trace) so that I could *remember* what the original trace (the tennis ball) was a trace of. But the signature and photo are equally inadequate. They too can't be linked unambiguously to a specific individual. Of course, if I could simply remember who wrote the signature or left behind the photo, then it's not clear why I even needed the original tennis balls. If no memory mechanism is needed to make the connection from tennis ball to party guest, or illegible signature to its author, then we've conceded that remembering can occur without corresponding traces, and then no trace was needed in the first place to explain how I remember who attended my party. So in order to avoid that fatal concession, it looks like yet another memory mechanism will be required for me to remember who left behind (say) the illegible or phony signature, or the fuzzy photo. And off we go on a regress of memory processes. It seems that no matter what my party guests leave behind, nothing can be linked only to the guest who left it. We'll always need something else, some other mechanism, for making the connection between the thing left behind and the individual who left it.

In fact, it seems that the only way to stop the regress is for a guest to leave behind something that is *intrinsically* and exclusively linked only to one individual. That is why Wolfgang Köhler, for example, proposed that traces must be *isomorphic* with the things of which

they're traces – that is, the things they represent (e.g., Köhler, 1947, 1969). But what Köhler and others have failed to grasp is that this kind of intrinsic connection is impossible, because nothing can function in one and only one way. As I'll argue shortly, this is especially clear when the function in question is one of representation or meaning. Nothing can represent unambiguously (or represent one and only one thing); representing is not something objects can do all by themselves; and representation can't be an intrinsic or inherent relation between the thing represented and the thing that represents it.

Interestingly, although Köhler failed to see why trace theory is doomed to fail, he was remarkably clear about what trace theory requires. Köhler understood that a major hurdle for trace theory is to explain trace *activation* – that is, how something present triggers my trace of Jones, rather than the trace of someone else. And that's a serious problem, because what triggers a memory (or activates a trace) can be quite different from what established it in the first place. So Köhler wrote,

...recognition...means that a present fact, usually a perceptual one, makes contact with a corresponding one in memory, a trace, a contact which gives the present perception the character of being known or familiar. But memory contains a tremendous number of traces, all of them representations of previous experiences which must have been established by the processes accompanying such earlier experiences. Now, why does the present perceptual experience make contact with the *right* earlier experience? This is an astonishing achievement. Nobody seems to doubt that the selection is brought about by the similarity of the present experience and the experience of the corresponding earlier fact. But since this earlier experience is not present at the *time, we have to assume that the trace of the earlier experience resembles the present experience, and that it is the similarity of our present experience (or the corresponding cortical process) and that trace which makes the selection possible.* (Köhler, 1969, p. 122, emphasis added)

By the way, this passage reveals another serious limitation of trace theory, one I can only mention in passing here. If trace theory has any plausibility at all, it seems appropriate only for those situations where

remembering concerns past experiences, something which apparently could be represented and which also could resemble certain triggering objects or events later on. But we remember many things that aren't experiences at all, and some things that aren't even past – for example, the day and month of my birth, the time of a forthcoming appointment, that the whale is a mammal, the sum of a triangle's interior angles, the meaning of "anomalous monism." Apparently, then, Köhler's point about trace activation and the need for similarity between trace, earlier event, and triggering event, won't apply to these cases at all. So even if trace theory was intelligible, it wouldn't be a theory about memory generally.

In any case, trace theory is not intelligible, and Köhler's observation reveals why. To avoid the circularity (and potential regress) of positing the ability to remember in order to explain my ability to remember (e.g., by requiring further trace mechanisms to enable the previous trace do its job), we must suppose that some trace uniquely and unambiguously represents or connects to the original experience. And because unambiguous representation is an impossible process, trace theory is caught between two fatal options. I'll explain in a moment why unambiguous representation is impossible, but first, we need to observe that the tennis ball/party example hides a further complication noted in the passage from Köhler.

Traces are usually supposed to be brain processes of some sort, some physiological representation produced, in this case, by a party guest. But what *activates* this trace later can be any number of things, none of which need to resemble the experience, object, or event that produced the original trace. Suppose Jones attended my party. Trace theory requires my experience of Jones at the party to produce a representation in me of Jones (or my experience of him) so that I can later remember that he was at the party. But what will subsequently activate that trace? It could be Jones himself, or an image of Jones, or the lingering smell of someone's cologne, or a telltale stain on the carpet, or perhaps someone asking, "Who was at the party?" Of course, some of these potential triggering objects or events might plausibly be said to resemble the thing that originally produced the trace. But how can (say) the smell of cologne, a stain, or the words "Who was at the party?" trigger the trace of Jones created by his presence at the party?

These things aren't obviously similar to Jones himself. If we posit another memory *mechanism* to explain how I draw the connection between the cologne and Jones (e.g., he may have worn it, spilled it, or simply talked about it), or how the question "Who was at the party?" leads me to the right party and not some other party, or even how I remember what the word "party" means, we're starting a regress of memory mechanisms. But if we say it's because I can simply *remember* who wore (or perhaps mentioned) the cologne, stained the carpet, or who my party guests were, then we're still reasoning in a circle. We're still explaining memory by appealing to the ability to remember. Moreover, if I can remember these things without some further trace, then we didn't need a trace in the first place to explain my ability to remember that Jones was at the party. However, if we follow Köhler's lead, then we have to assert some kind of intrinsic similarity or resemblance, some kind of psychophysical *structural isomorphism*, between three things: the original experience or event, the trace produced on that occasion, and the subsequent triggering events.

If nothing else, it should make you suspicious that a representation of Jones at the party will be isomorphic both to Jones (or my experience of him) and to the innumerably many and quite different things that can later activate the trace-for example, a particular scent or a sequence of sounds. What kind of similarity could this be? The answer is that it can't be any kind of similarity and that Köhler's proposal is literally meaningless. As tempting as it is to continue for a while enumerating the problems with trace theories, I'll restrict myself now to two more points, to explain perhaps the deepest confusion underlying these theories.

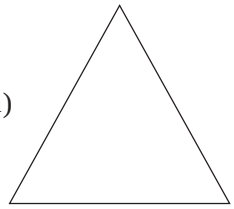
The first problem is with the very idea of structural isomorphism. The term "structural isomorphism" sounds impressive and scholarly, but in trace theories the appeal to structural isomorphism is really just the appeal to an *inherent similarity* between two things, *determined solely by their respective structures.* Traces must be produced in a way that relates them structurally to the things of which they're traces, and they must be activated only by things having the right underlying structure. Moreover, that activation must be determined solely by intrinsic relations between the structures of the trace and the things that activate them. Otherwise, we'd need another mechanism to explain how the

right trace is activated in the presence of a trigger that could just as well have been isomorphic with (or mapped onto) something else. And that raises the circularity or regress problem noted earlier.

But the alternative, inherent similarity, makes no more sense than saying that a square is a circle. Inherent similarity is a static relation obtaining only between the similar things. And it must hold between those things no *matter what*. If, for example, context could alter whether two things count as similar, then those things are not similar merely in virtue of intrinsic relations holding between their respective structures. But that's why intrinsic similarity is nonsense. Similarity exists only with respect to variable and shifting criteria of relevance. It can only be a dynamic relation holding between things at a time and within a context of needs and interests.

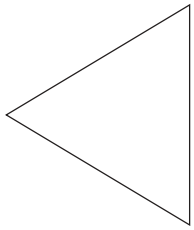
A simple example from geometry should make the point clearly. Consider the triangle:

(a)

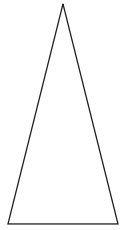


Next, consider these other geometric figures:

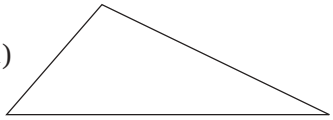
(b)



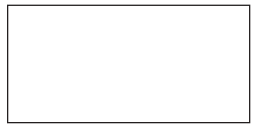
(c)



(d)



(e)



Now consider the question: To which of the last four figures is (a) similar? The proper response to that question should be puzzlement; you shouldn't know how to answer it. Without further background information, without knowing what matters in our comparison of the figures, the question has no answer at all. Mathematicians recognize this, although instead of the term "similarity" they use the expression "congruence." In any case, mathematicians know that in the absence of some specified or agreed-upon rule of projection, or function for mapping geometric figures onto other things, no figure is congruent with (similar to) anything else.

Mathematicians recognize that there are different standards of congruence, appropriate for different situations. But no situation is *intrinsically basic*, and so no standard of congruence is inherently privileged or more fundamental than others. For example, engineers might sometimes want to adopt a fairly strict mapping function according to which (a) is congruent only with other figures having the same interior angles and the same horizontal orientation. But in that case, (a) would be congruent with none of the other four figures. Of course, only in very specialized contexts are we likely to compare figures with respect to their horizontal orientation. In many situations it would be appropriate to adopt a different standard of congruence, according to which sameness of interior angles is all that matters. And in that case we'd say that figures (a) and (b) are congruent but that (a) is not congruent with the other figures. However, there's also nothing privileged about sameness of interior angles. Perhaps what matters is simply that (a) is congruent with any other three-sided enclosed figure, in which case we could say it's congruent with the three triangles (b)-(d), but not with the rectangle (e). But even that criterion of congruence can be modified or supplanted. Mathematicians have rules of projection that map triangles onto any other geometric object, but not to (say) apples or oranges. Of course, the moral here is obvious. If simple geometric figures are not intrinsically similar-that is, if they count as similar only against a background of assumptions about which of their features matter (i.e., are relevant), then we certainly won't find intrinsic similarity with much more complex objects – in particular, memory traces and the various objects or events that allegedly produce and activate them.

But maybe you're still not convinced. Perhaps you think that there is a fundamental principle of congruence for this geometric example. You might think that, first and foremost, (a) is similar to just those figures with sides of exactly the same length, the same horizontal orientation, and with exactly the same interior angles. And perhaps you'd want to call that something like "strict congruence (or identity)." But there are at least three serious problems with that position.

First, even if this sort of congruence counted as more fundamental than other forms of geometric similarity, that could only be in virtue of a kind of historical accident. The primacy of that standard of congruence would reveal more about us, our conventions and values – in short, what merely happens to be important to us, than it does about the figures themselves. In fact, it's a standard appropriate for only a very narrow range of contexts in which we consider whether things are similar. Second (and as an illustration of that first point), it's easy to imagine contexts in which two triangles have exactly the same interior angles, horizontal orientation, and sides, but don't count as similar. If we're interior designers, for example, it might also matter whether the triangles are of the same color, or whether they're placed against the same colored background, or whether they're made of the same material. If we're graphic artists, it might matter whether the triangles were both original artworks or whether one was a print. Or if we're librarians or archivists, it might matter whether the triangles occur on the same page of different copies of the same book. And third, even if we could decide on some very strict sense of congruence (or identity) which would count as privileged over all other forms of similarity, it would be useless in the present context. Memory traces are never strictly identical either with the things that produce them or with the things that activate them. The looser and more complex forms of similarity at issue in trace theories are classic examples of the sorts of similarities that can't possibly be inherent, static relations between things.

And as if that weren't enough, another aspect of this general confusion about similarity is the requirement that traces and other things have intrinsic or inherent *structures* – that is, some context-independent parsing into basic elements. That's a necessary condition

for intrinsic isomorphism to hold between the trace and the things it represents. After all, that isomorphism can't depend on mappings fixed externally to those things, because then we'd need to posit a mechanism to determine which features of those things are relevant, and then a mechanism for disambiguating the application of that mechanism, and so on, as the regress again rears its ugly head. However, the way out of this – the idea that these objects or events have an inherent structure to which the inherent mapping between them is linked – is also nonsense. It's on a par with claiming that a pie has a basic context-independent division into slices or elements, or that there's an absolutely context-independently correct and privileged answer to the questions, "How many events were there in World War II?" and "How many things are in this room?"

Confusions about Representation

The appeal to inherent similarity or structure is merely a specific form of a more pervasive problem in the so-called cognitive sciences-namely, confusions about and equivocations on the term "representation." Traces are supposed to represent their causes, the events or experiences that produced them, and they must be internally and structurally differentiated in ways that correspond to the different things we remember. This is one version of the general view that distinct mental states are caused by (or identical to) certain corresponding distinct internal physical states, and that what those different internal states *are* (i.e., what they represent) is explainable wholly in terms of their distinctive structural features. At this point, cognitive scientists typically do a lot of hand waving and say something like, "We may not currently know all the details, but presumably some super psychology of the future (or perhaps God) could in principle look inside our heads and know, from the way we're configured, what we're thinking."

However, this general picture rests on the utterly false assumption that a thing's representational properties can be determined solely by its structural or topological features. I've examined this error in considerable detail elsewhere (Braude, 1997, 2002). For now, a few brief remarks will have to suffice.

To see what's wrong, we need to appreciate that *anything can represent anything.* In fact, a thing's representational options are limited only by the situations into which it can be inserted. And if that's the case, then what something represents can't simply be a function of how it's configured. Suppose I'm trying to teach a child the alphabet. I show him a picture of a dog and I say "D is for dog." In that case, we might say that the picture represents the class of dogs. But I could have said, "C is for collie," and in that case the picture would have represented a subset of the set of dogs. Similarly, I could have said "L is for Lassie," in which case the picture would have represented an even smaller subset of dogs. I could also have said "Z is for Ziggie," referring to the child's pet collie. And notice, these changes in what the picture represents have nothing whatever to do with corresponding changes in the arrangement of pixels, or atoms, or anything else in the picture. Those structural features of the pictures remained the same in all cases. What the picture represents depended instead on how it was used.

And in fact, the picture's representational properties could be changed even more dramatically. My disgruntled students could make the picture represent me and symbolically express their hostility toward me by using it as target for darts. Or I could jokingly point to the picture and say "This was Joan Rivers before plastic surgery." Or suppose I'm trying to give directions to someone without the aid of a map. I could place the picture on a table and say, "This is the shopping center, this [a ham sandwich] is the hospital, this [my fork] is the access road, and this [a salt shaker] is the water tower."

Of course, contexts in which (say) a sandwich represents a building, or in which a picture of a dog represents a distinguished philosopher (or over-the-hill comedienne), are atypical in some respects. But those situations are unusual only with respect to what the objects represent. They aren't at all unusual with respect to how representational properties are acquired. And it doesn't matter whether we're talking about images, words, or (say) synaptic connections. In every case (familiar and offbeat), what a thing represents depends ultimately on the way we place it in a situation. There are no context-independent forms of representation or meaning. So when it comes to examples like the picture of a dog or the ham sandwich, the mistake many make is to

think that some representational properties-the familiar and apparently default ones-are inherently fundamental and that others are anomalous. That is, they believe that representation in familiar cases is somehow built-into or hardwired into the representing objects, and that this inherent function simply gets overridden in the more unusual cases. But in fact, the familiarity of certain contexts reveals more about us, about our patterns of life and our interests, than it does about the objects themselves. If our form of life were radically different, the default or familiar representational properties of objects could change accordingly.

But then if a brain structure (say) is to represent something past and function as a memory trace, it can't do so solely in virtue of its structural features. Nothing represents or means what it does on topological grounds alone. However, the whole point of Köhler's principle of psychophysical isomorphism (or related hypotheses in the cognitive sciences) is to tie what a thing represents solely to its structure. That was the only way to avoid the equally fatal error of requiring a regress of mechanisms to explain how the original mechanism or state can do its job. So this, too, turns out to be a dead end.

Tokens and Types

But let's return more explicitly to trace theory. A related, and equally unheralded problem with such theories is that traces and their causes or activators are of radically different ontological kinds, and the sort of thing traces have to be is a kind that many think is simply a philosophical fiction. At any rate, it's nothing but a philosophical move, not even remotely a scientific move, to posit the existence of traces. Hopefully, one distinction and one more example will make this clear. (For a considerably more detailed presentation of the following arguments, see Bursen, 1978.)

Trace theorists have always been tempted to regard traces as kinds of *recordings* of the things that produced them. In fact, some previous influential writings on memory compared traces to tape recordings or grooves and bumps in a phonograph record. The justification for that idea, as we've seen, is that traces must somehow capture essential structural features of the things that produce them. However, the poverty of this view is easy to expose.

Consider: One of the things I remember is Beethoven's Fifth Symphony (hereafter abbreviated as B5). Modern versions of trace theory require that my memory is explained in terms of a representation of B5, stored in some form in my brain and produced in me by the experience of hearing B5 in the past. This trace must have certain structural or topological properties that link it to the thing(s) that caused it, properties which also distinguish it from traces of other pieces of music. So presumably this trace of B5 was produced by and captures features of a performance I heard of B5. But which features? Tempo, rhythm, pitch, length of notes, instrumental timbre, dynamic shadings? You'd think so if my trace of B5 was produced by and represents or records a B5 performance, and also if that trace is to differ (say) from my trace of Beethoven's Fourth (B4) or "Yankee Doodle.". But I (like many others) can remember B5 by recognizing a wide variety of musical performances as *instances* (or as philosophers would put it, *tokens*) of B5. For instance, I could recognize B5 when certain notes are held for an unusually long time, or when it's played with elaborate embellishments, or with poor pitch and many mistakes by an amateur orchestra. In fact, I could recognize truly outlandish musical events as instances of B5-for example, when it's played extremely slowly or rapidly, or with tempo changing every bar, or with arbitrary notes raised a major sixth, or when it's played with inverted dynamics, or played only on kazoos, banjos, or tubas. Similarly, I could recognize a series of percussive taps as a pitch-invariant version of the opening bars of B5.

But this means that the trace is not a recording. On the contrary, it must be a very unusual sort of entity. Whereas the remembered and triggering events or experiences are concrete event-tokens, the trace itself must be a relentlessly abstract object-what philosophers call a *type*. And it has to be so abstract that it can't contain any features found in the performances or experiences that produced it (e.g., precise rhythm, pitch, etc.). If it had those features, we'd need to posit another mechanism to explain how my trace can be activated by tokens of B5 lacking them – for example, a tuba – only performance of B5 played at quarter speed with many wrong notes. But if we try to prevent a regress by saying that I can simply recognize that the tuba-only version is an instance of B5, then we don't need to posit the trace of B5 at all. We've

conceded that I can remember B5 without recourse to a B5 trace.

But now look at what has happened. We've seen that the B5 trace is an abstract type. However, that trace has to have *some* features in virtue of which it's a B5 trace and not a trace of (say) Beethoven's Fourth, the "Waldstein" Sonata, or "Yankee Doodle." But it can't have features found in any specific instances of B5, because none of those are necessary for a musical event to be an instance of B5 capable of either producing or activating the trace. So the B5 trace somehow needs to have features necessary and sufficient for being an abstract B5 and not (say) a B4, but without having any specific – dispensable, alterable, or "accidental" – features regarding pitch, tempo, dynamics, etc. (perhaps you can now see why many consider abstract types impossible objects).

In any case, we've arrived at the point where we see the ultimately nonscientific nature of trace theory. It's committed to the view that a memory trace and all its concrete instances have a structure that is essential to all things that are instances of B5, but none of the specific features which versions of B5, including the nightmare versions, can lack. This position is commonly called Platonic essentialism – the view that things are of the same kind in virtue of sharing a common underlying, but abstract, structure. And that's not a scientific view at all. It's a philosophical view, and a bad one at that.

The Abuse of Memory in Parapsychology

It's unfortunate enough that memory trace theory is received dogma in the cognitive sciences. Almost no one seems to doubt that memories are somehow stored and encoded in us. So it's not surprising that this picture of memory has found its way to more overtly speculative or frontier areas of science, including parapsychology. No doubt it's very tempting for parapsychologists to posit trace-like processes in their own theories, because they will at least appear to be reasoning along scientifically orthodox lines, even if the subject matter itself falls outside the scientific mainstream.

For example, Roll has proposed a "psi structure" theory of survival, modeled explicitly after memory trace theory, and according to which

memory traces are left, not simply in individual brains, but in our environment as well (Roll, 1983). Of course, this escapes none of the classic problems of trace theory, because on Roll's view, what certain structures represent (or are similar to) remains unintelligibly tied to inherent features of those structures. This is especially problematical when Roll suggests that an individual mind or personality is a system of such structures. That's no more plausible than saying that we can tell whether a person is thinking about his grandmother just by examining the state of his brain, or that a picture of a dog represents something specific independent of its use in a context. It requires brain or mental structures to mean or represent something simply in virtue of how they're configured, never mind their dynamic position within an equally dynamic life situation. Roll also proposes explaining ESP as the responding to memory traces left on objects by previous guesses. But that seems no more credible than supposing that I could remember my party guests from looking simply at the tennis balls they left behind, or the illegible signatures or photos they left along with the balls.

Trace theory also appears in other guises in connection with the evidence for postmortem survival. One is the suggestion that reincarnation cases can be explained in terms of genetic memory. However, I've found no serious researcher making that suggestion. It seems, instead, to be entertained simply as a real possibility, albeit one that can be rejected on empirical grounds (see, e.g., Almeder, 1992; Stevenson, 1974). That is, it's treated as if it's an intelligible position that happens merely to be inadequate to the data. Another application of trace theory to survival is the attempt to explain transplant cases by appealing to cellular memory (e.g., Pearsall et al., 1999). No doubt the reason it's tempting here to posit genetic or cellular memory traces is that in reincarnation and transplant cases, complex psychological regularities seem to persist in the absence of the usual presumed bodily correlates. So to those for whom it's unthinkable that memories could persist without being stored somewhere, it might seem reasonable to propose that memories and personality traits can be encoded in a kind of hardware that has nothing to do with the brain. However, since the problems noted earlier with trace theories are hardware-independent, it's an insignificant change merely to relocate the traces in different

physical systems. It's still untenable to suppose that representation, meaning, or similarity, are determined solely by a thing's topological features.

To me, it's interesting that when the usual suspect – the brain – isn't available as the locus of memory storage, some find it inevitable that memories must simply be located in a different place or perhaps in a modified form. It demonstrates just how deeply mechanistic assumptions have taken root, and in a way, it shows a profound lack of scientific imagination. The situation here closely parallels what happened in response to Lashley's famous experiments in the 1920s (Beach et al., 1960; Lashley, 1929, 1950). When Lashley found that no matter how much of a rat's brain he surgically removed, trained rats continued to run their maze, some concluded that the rats' memories weren't specifically localized in their brains. Instead, they suggested that the memories were diffusely localized, much as information is diffusely distributed in holograms (Pribram, 1971; Pribram et al., 1974; Pribram, 1977). But to someone not antecedently committed to traditional mechanistic dogma, Lashley's experiments take on a different sort of significance, perhaps similar to that of the evidence for postmortem survival. They suggest that memories are not located anywhere or in any form in the brain. And more generally, they suggest that the container metaphor (that memories and mental states in general are *in* the brain, or in something else) was wrong from the start. Of course, that's what my arguments in the preceding sections were intended to show.

Another variant of this general error emerges in Rupert Sheldrake's suggestion that morphic fields capture the essential structure of developmental forms and even behavioural kinds. Although Sheldrake thought he was escaping the evils of mechanistic theories with his view, in fact he retained the underlying errors of supposing that similarity is an intrinsic structural relation between things, and that things of the same kind are of that kind because they share a common underlying structural essence. The claim that behavioral kinds, such as feeding behavior and courtship, can be captured in strictly structural terms, is especially implausible. (For a detailed critique of Sheldrake's theory, see Braude, 1983.)

Summing Up

I realize that at this conference I'm pretty much a voice in the wilderness, and I find myself in the unenviable position of telling many or most of you that you actually don't know what you're talking about. I wish there were some other, less fundamentally upsetting, way to undercut trace theories of memory. But I believe that the problems really are that deep and that the theories really are that essentially confused.

However, as long as I've probably antagonized most of you anyway, I see no compelling reason to stop where I left off. I might as well finish with brief obnoxious coda. As I see it, memory researchers and parapsychologists are missing an opportunity to be genuine scientific pioneers. Rather than boldly searching for new explanatory strategies (for memory specifically and for human behavior generally), they cling instead to familiar mechanistic presuppositions, which they've typically never examined in any depth, but by means of which they can maintain the illusion that they're doing science according to the allegedly tough-minded methods exemplified in some physical sciences. (Sherry Turkle has appropriately called this "physics envy.") They can't get past the assumption that human abilities and behavior must be analyzed in terms of lower-level processes and mechanisms. And they seem not to recognize the difference between claiming that cognitive functions are analyzable in terms of underlying physical processes and claiming instead that those functions are merely mediated by underlying physical processes. But there are novel explanatory options and strategies they never consider; there are alternative and profoundly different approaches to the understanding of human beings. However, spelling out those options is a huge project, one that must be reserved for another occasion.

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MESA-REDONDA:
SÍNTESE E CONCLUSÕES
COMPREHENSIVE ROUND-TABLE

MESA-REDONDA: SÍNTESE E CONCLUSÕES

Alexandre Castro-Caldas*

Nesta sessão foi feita, inicialmente, uma revisão breve, integradora, das comunicações feitas nos três dias pelos diferentes especialistas, salientando a necessidade de integrar cada vez mais os conceitos oriundos das neurociências com os que são gerados no domínio da investigação em Parapsicologia.

Na discussão que se seguiu constitui ponto de interesse a dificuldade em fazer analogia entre os fenómenos parapsicologicos, que se tornam evidentes num número restrito de sujeitos, e os fenómenos cognitivos que se podem considerar mais universais. Este aspecto limita a abordagem experimental e pode enviesar resultados pelo simples facto de ser necessário escolher a amostra. Por outro lado, o último século foi particularmente rico na colecção de conhecimentos resultantes do estudo de doentes com lesões cerebrais. Estes estudos permitiram o formular de teorias e hipóteses relativas à relação do cérebro com o comportamento e com a actividade cognitiva. Em contrapartida, esta metodologia não teve nunca eco no domínio da parapsicologia onde não há registos, tanto quanto foi possível apurar junto dos participantes na sessão, de doentes que sendo portadores de capacidades paranormais as tenham perdido na sequência de uma lesão focal do cérebro. Foi até sugerido que em alguns casos as manifestações de fenómenos paranormais se tornavam evidentes depois de uma agressão ao cérebro. Foram citados os fenómenos de quase-morte mas considerou-se não serem fenómenos comparáveis e salientou-se o facto de terem já estes aspectos sido discutidos no simpósio anterior.

Os estudos com as novas técnicas de imagiologia começaram a trazer nova informação para este domínio do saber. Levantou-se, contudo, a questão do valor dos achados que sugeriam diminuição de

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actividade biológica regional. Em neurofisiologia há muito que se discute a questão da inibição conhecendo-se bem os mecanismos moleculares em que assenta, porém, quando ela se torna evidente em macrofenómenos como os que informam as imagens de PET scan ou Ressonância Magnética funcional, temos dificuldade em fazer uma interpretação correcta.

Discutiu-se ainda a possibilidade dos fenómenos Psi serem uma forma de sono anormal. Podia assim considerar-se que os fenómenos seriam universais ocorrendo só em situações particulares a que só alguns indivíduos conseguiam aceder. A configuração das activações cerebrais seria então particular dando acesso a novas possibilidades de processamento de informação. Trata-se naturalmente de uma interpretação especulativa que carece de validação experimental.

A sessão terminou com agradecimento à Fundação BIAL que patrocinou, aos palestrantes e ao público que foi convidado a participar no 7º Simpósio que terá lugar em 2008.

LISTA DE POSTERS
POSTERS

Lista de Posters com resultados finais apresentados pelos bolseiros da Fundação Bial

Poster with final results presented by Bial Foundation researchers

1998

30/98 - "Caracterização neurofisiológica e psicofisiológica de disfunções cerebrais utilizando estudos qEEG/ERP. Metodologia e Aplicações" - "Patterns of ERD/ERS in simple motor tasks"

Instituição/*Institution:* Hospital Sto. António - Porto

Duração/*Duration:* 1999/06 - 2004/04

Investigadores/*Researchers:* Prof. António Martins da Silva, Prof. Denisa Maria Vasques Mendonça, Dr. João Manuel Carmona Ferreira Lopes, Dra. Maria Regina Pinto Brito Aguiar Andrade, Dr. João Eduardo Paiva Ramalheira, Prof. João Paulo Trigueiros Silva Cunha, Engº Miguel Oliveira e Silva, Dra. Teresa Temudo, Dr. Óscar Gomes, Dr. Óscar Alves

Abstract:

Objective: To study spatiotemporal changes of the EEG patterns induced by simple motor tasks in healthy voluntaries using event-related desynchronization/synchronization measurements. Results will be used to define normal response patterns and subsequently to study the importance of the variations that may occur in patients.

Methods: Healthy subjects were studied by recording EEG signal at rest and when executing a sequence of simple voluntary movements - brisk right thumb in three different conditions: i) self-paced movement - subject executes movements on every x seconds counted by himself; ii) random time acoustic command stimulus - movements are executed at random time intervals; iii) fixed time acoustic command stimulus - movements are executed at fixed time intervals. Each movement was repeated 60 times with a minimum interval between trials of 8 to 10 seconds. EEG was recorded from 30 electrodes according to the International System 10-20. Data sampling rate was 128 Hz, processed with the specific software (Brain Vision ®) for signal analysis. Epochs

comprised 3 sec. preceding and 2 sec. following motor task execution. Artefacts were manually rejected.

Results: In self-paced and before movement a contra-lateral dominant ERD on EEG alfa band, recorded on C3 was found, followed by a bilateral ERD on Cz and C4 which remained during movement. ERD had an earlier onset and was more pronounced in alpha band than beta. For self-paced movements and movements preceded by a random acoustic stimulus ERD was more prominent than for fixed time related movements. For movements preceded by an acoustic stimulus in fixed time intervals, ERD just appears in the beginning of the movement.

Conclusions: We identify EEG modifications before, during and after movement execution. The contra lateral ERD followed by bilateral ERD in self-paced and in random time acoustic movements was not evident when acoustic stimuli were elicited at fixed time intervals.

39/98 - "Realidade virtual no tratamento da acrofobia" – "Virtual reality in acrophobia treatment"

Instituição/*Institution:* Laboratório de Psicologia da Universidade do Minho - Braga
Duração/*Duration:* 1999/03 - 2005/01
Investigadores/*Researchers:* Prof. Jorge Silvério, Prof. Mário Martins

Abstract: The virtual reality systems (VR) have been more and more used in the field of psychology. Since the hardware is getting cheaper, and the VR systems can create a greater variety of stimuli and situations, practitioners and investigators have been increasingly exploring the potentialities of this technology. With the present work we try to contribute to the study and implementation of the therapeutic potentialities of this system. We have chosen acrophobia as the treatment's target, and a VR system as the mediator of the participant's exposure. The study's results show that, despite the low exposure time, the participants of the VR group were able to climb more steps with less anxiety after the treatment. It is also noteworthy that these results were maintained on a one-year follow-up. There were also positive and statistically significant results for the participants in the real environment

group, which clearly shows the already known therapeutic value of the exposure therapies. The two groups didn't show significant differences when comparing the treatment's efficacy. We also observed that a lot of the symptoms referred in the SSQ questionnaire were not only displays of nausea, but also of fear. We noticed that the VR treatment has real and long-term advantages, not only for its efficacy, but also for the fact that it facilitates the confidentiality of the therapeutic exposure. This kind of intervention also makes it easier for the therapist and the client to start and finish the exposure whenever they want, which gives more control to the client. The VR facilitates the balance between the ecological and the experimental observation, making it easier to evaluate the progress of the participants. The virtual exposure is a promising way for intervention with people with specific phobias, and in this work we noticed that its application to individuals with fear of heights is effective.

2000

02/00 - "Indicadores Psicofisiológicos e Psicossociais do Impacto do relacionamento Conjugal no Desenvolvimento Pessoal e Relacional dos filhos na fase adulta" - "Psychophysiological and Psychosocial indicators of the impact of marital relationship in the personal and relational development of children in adulthood"

Instituição/*Institution:* Laboratório de Neuropsicologia da Universidade do Minho - Braga

Duração/*Duration:* 2001/01 - 2005/05

Investigadores/*Researchers:* Prof. Maria da Graça Pereira Alves, Dra. Vera Araújo-Soares

Abstract: Objectives: This project analyzed the impact of the marital relationship on their adult children using psychosocial and physiological measures.

Methods: 200 young adults composed the sample that was made of undergraduate students at the University of Minho (Portugal) from several fields of study.

Instruments: The variables accessed include the Portuguese Versions of: the Separation-Individuation Process Inventory (Christenson & Wilson, 1985); the Miller Social Intimacy Scale (Miller & Lefcourt, 1982); the Perceived Social Support (Procidano & Heller, 1983); the Conflict Tactics Scale (Strauss & Gelles, 1990); the Ways of Coping Questionnaire (Coyne, Aldwin & Lazarus, 1981); the Beck Depression Inventory (1973); the State- Trait Anxiety Inventory (Spielberger et al.); Rotterdam Symptom Check List (Haes et al., 1990); and the Perception of Parental Relationship by their Adult Children (Pereira & Araújo-Soares, 2002).
Physiological measures included heart rate, blood pressure and skin conductance.
Subjects had to fulfill a physiological protocol that included watching a movie that elicited positive emotions and one that elicited negative ones. They were assessed at several times: baseline, after each movie, relaxation and while talking about the experience.

Results: The results show important differences between the positive and negative assessment moments. In reaction to positive emotions parental relationship has a direct impact on self differentiation meaning that a good parental relationship allows the individual to differentiate and an indirect effect on avoidance coping strategies. This path does not show up in reaction to stressful situations in which less differentiation is related to using less attention strategies as one might expect. Parental relationship has also a direct impact on distress and physical morbidity both in reaction to negative and positive emotions and an indirect impact through social support and social intimacy.
Conclusion: Parental Relationship has an effect upon both psychological and physiological perceived health; this effect is complex and at least partially mediated by other psychosocial variables, namely coping strategies, self-differentiation and social support.
According to results, there is a need to intervene with children from problematic families in order to minor the intergenerational risk of parental relational problems on young adults´ physical and mental health.

2002

01/02 – ”The investigation of Telepathy and the Sense of Being Stared At in Humans and Animals”

Instituição/*Institution:* Centre for the Seven Experiments Project, London - UK
Duração/*Duration:* 2003/03 - 2005/04
Investigadores/*Researchers:* Prof. A. Rupert Sheldrake, Ms. Pam Smart, Dr. Kara Murray

Abstract: ***Objective:*** The objective of this study was to investigate possible telepathic communication in connection with e-mails. Many people claim that they have had the experience of thinking about someone for no apparent reason, and then soon afterwards receive an e-mail from that person. Does this really involve telepathy, or it is just a matter of chance coincidence?

Method: The experimental method involved a series of trials in which each subject had four e-mailers, at least two of whom were nominated in advance by the subject. In each trial, one of the four potential e-mailers was selected at random by the experimenter, and informed by e-mail that he or she had been selected. One minute before a prearranged time at which the e-mail was to be sent, the participant guessed who would send it, and communicated this guess to the experimenter by e-mail. When the randomly chosen e-mailer sent the e-mail to the subject, a copy (using the “cc” procedure) was also sent to the experimenter. The times recorded on these e-mails established that the guesses were indeed sent before the e-mails from the e-mailers.

Results: We tested 50 participants (29 female and 21 male, recruited through an employment web site) in a total of 552 trials. In 235 (43%) their guesses were hits, significantly above the chance expectation of 25% (z=9.49; p = 2x10-19; Cohen’s d .42). The 95% confidence limits of this hit rate were from 38% to 47%. We then carried out further tests with 5 participants (4 female, 1 male, aged between 16 and 29) in which they were videotaped continuously. In the filmed trials, there were 64 hits out of 137 (47%), significantly above the chance level (z=5.77; p=3x10-8; d=.50), with 95% confidence limits from 38% to 55%.

Conclusions: The results were consistent with the hypothesis that subjects were sometimes able to detect telepathically when another person was thinking about them and preparing to send them an e-mail. We are following up this research with an automated online telepathy test.

15/02 - "Bases psicofisiológicas dos fenómenos de consciência visual" - "Neural correlates of visual perceptual decision"
Instituição/*Institution:* Centro de Oftalmologia - Coimbra
Duração/*Duration:* 2003/01 - 2005/12
Investigadores/*Researchers:* Prof. Miguel de Sá Castelo Branco, Dr. Lajos Kozac, Dra. Mafalda Mendes, Dr. Vasco Forjaz, Dra. Manuela Guerreiro

Abstract: ***Introduction and objectives:*** We have aimed to study retinal and cortical patterns of perceptual asymmetry under stimulus conditions that allow to study separately the magnocellular / parvocellular pathways. As a further attempt to link low level and high level visual processes we investigated how the visual system uses contextual information about local similarities to integrate global surfaces into a single percept. We finally aimed to describe how illusory motion interacts with real motion processing in the human motion sensitive area hMT+/V5.

Methods: Low level visual tasks were based on contrast sensitivity measurements under eyetracking conditions. Magnocellular-biased test stimuli were sinusoidal gratings of 0.25 cpd, vertically oriented and undergoing 25 Hz counterphase flicker (parvo stimuli: 3.5 cpd and 0 Hz) To explore contextual modulation of perceptually bi-stable stimuli we asked subjects to report whether they perceived a central stimulus either as two grating surfaces, one being transparent and sliding on top of the other (incoherent motion) or as a single pattern whose direction of motion is intermediate to the component vectors (coherent motion). Functional magnetic resonance imaging (fMRI) was performed at 1.5T using coherent and illusory motion paradigms. Data analysis was performed using BrainVoyager QX.

Results: Using contrast sensitivity tasks, we found distinct nasotemporal patterns of asymmetry both with parvo and magno-biased conditions, which is consistent with known retinal physiological anisotropies. A surprising left/right asymmetry was also observed for this task, suggesting an additional cortical origin for anisotropies in contrast sensitivity.
When exploring mechanisms related to perceptual segmentation of moving surfaces, we found that local collinear configurations may also induce paradoxical dissociations between center and surround percepts instead of the expected congruence in perceptual interpretation. Further, we have found that the human motion sensitive area hMT+/V5, can process concomitantly real and distinct types illusory motion (apparent motion and motion aftereffects) in a non-additive manner.
Conclusions: We conclude that visual sensitivity is not homogeneous, even under conditions of evenly distributed visual attention. Finally, dynamical local-global contextual interactions are the key factors determining motion perceptual segmentation, instead of the generally assumed unidirectional two-stage feedforward mechanism.

20/02 - "Psychophysiological Mechanisms of some aspects of Neurocognitive Deficit in Schizophrenic Patients"
Instituição/*Institution:* Institute of Higher Nervous Activity and Neurophysiology, Russian Academy of Sciences – Russia
Duração/*Duration:* 2003/02 - 2004/12
Investigadores/*Researchers:* Dra. Valeria Strelets, Dra. Janna Golikova, Dr. Vladimir Novototsky-Vlasov, Dr. R.A. Magomedov, Dr. M. V. Magomedova

Abstract: Contemporary neuroscience considers that one of the "key" symptoms in schizophrenia is cognitive deficit. The synchronously cycling of gamma activity (typically 40 Hz, but varying from 30 to 90 Hz) evidently underlies the integration of diverse brain activities and associated neuronal networks (Engel et al., 1997). The disintegration of brain activities is a core disturbance in schizophrenia.
The goal of the of the project was comparative analysis of spectral power and coherence in the gamma spectral band and characteristics of

neuropsychological tests performance in the groups of normal subjects (38) and schizophrenic patients (F2 diagnostic category) – "acute" having their 1st episode (10) and chronic ones (10).
EEG recordings were done in the rest condition (with closed eyes) and in the cognitive task involving the activity of both hemispheres. Standard techniques were used to obtain spectral power and coherence indices of gamma-rhythm (20-40 Hz). Neuropsychological assessment battery included 28 tests according to A.R.Luria.
In "acute" patients in comparison with the control group there was significant increase in the gamma spectral power in prefrontal areas, especially of the left hemisphere, this increase being significant during the task performance. Chronic patients, on the opposite, showed significant decrease in the gamma spectral power in the right hemisphere in both conditions.
Both groups of patients revealed significant decrease of gamma coherence compared to the norm and absence of interhemispheric connections.
Acute patients fulfilled most of the neuropsychological tests significantly worse than healthy subjects ($P < 0.05$ by Mann-Whitney test). Chronic patients revealed similar results ($P < 0.001$). The poorest performance in both groups was obtained in the tasks demanding integration of the two brain hemispheres: immediate visual memory, kinesthetic praxis, painting without assistance, reproduction of Rey-Osterreith figure, verbal thinking and neurodynamic parameters.
Thus, in schizophrenia mostly disturbed are perception of an object as a whole (Gestalt) and interhemispheric integration requiring long connections.

27/02 - "Anomalous/paranormal detection using psi-reading tests (Phase II): New parapsychological, psychological and neuropsychological exploration data through seven tests with selected/non-selected subjects"
Instituição/*Institution:* Instituto de Psicologia Paranormal, Buenos Aires – Argentina
Duração/*Duration:* 2003/03 - 2005/01
Investigadores/*Researchers:* Dr. Alejandro Parra, Dr. Juan Carlos Argibay

Abstract: ***Objectives:*** From the beginning of scientific research in parapsychology in the 1880s, mediums and psychics have generated much interest. We conducted an investigation with special emphasis on psychics claimants taking a non-psychics (control) sample.

Method: Two hundred fifty nine participants were interviewed individually or in groups. Seventy four percent of them (N= 193) attended the tests. Participants ranged 18 to 76 years old (Mean= 44.87; SD= 12.66). Eleven self-questionnaires, such as Anomalous/Paranormal Experiences Inventory (AEI), NEO-PI-R, Constructive Thinking Inventory, Eysenck Personality Questionnaire, Creative Experiences Questionnaire, Seeking-Sensation Scale, Dissociation Experiences Scale, and Tellegen Absorption Scale were administered. The task of the groups included seven sessions, which consisted of seven psi-reading tests across two-months.

Results and Conclusions: A number of psi-reading tests were designed by us: (1) First ESP test was designed to be used with six of photos paired (three males and three females); (2) second ESP test used token objects into two conditions “at-a-distance” and “face-to-face” with two objects of two human-targets (male and female); (3) third ESP test was designed used objects token of four human-targets (two male and two female) medically diagnosed (i.e. Hiatal hernia; Diabetes mellitus; Arthrosis of the knee, and Varicose veins) in comparsion with four “control” objects; (4) fourth ESP test was designed with two texts containing a brief self-report written by four human-targets (two males and two females), who attended psychological interviews; (5) fifth ESP test was designed to be used with six recipients, three of them containing sand and water from “holy” places and “control” recipients. The experimenters delivered each participant (N= 193) the pairs (target and control), counterbalanced, and asked them to indicate their “impressions”. Two of five tests were significant (third and fifth psi-test). Many of the anomalous/paranormal experiences (using AEI) were significant with personality, creative thinking, extraversion-neuroticism, seeking sensation, and absorption and dissociation scales.

28/02 - **"Emotional factors in placebo analgesia: Psychophysiological Experiments"**
Instituição/*Institution:* Department of Clinical Research, University Hospital of North Norway – Norway
Duração/*Duration:* 2003/03 - 2006/03
Investigadores/*Researchers:* Prof. Magne Arve Flaten, Prof. Oddmund Johansen, Dr. Terje Simonsen, Dr. Jan Brox, Prof. Arnstein Finset

Abstract: ***Objectives:*** Cognitive, emotional, and social factors underlying placebo analgesia were investigated. This is a form of placebo response where pain is reduced after information that a painkiller has been administrated, even if an inactive substance has been provided. Experiment 1 investigated cognitive and emotional factors in placebo analgesia. Experiments 2 and 3 investigated how social context could modulate pain report, one important aspect of placebo analgesia.

Methods: In three experiments, ischemic or thermal pain was induced in healthy volunteers. Pain was measured by numerical rating scales. In Experiment 1, subjects were told that they received a painkiller or no painkiller. In Experiments 2 and 3, pain report in male and female subjects was obtained from male and female experimenters in a 2 x 2 design.

Results: The results from Experiment 1 showed a placebo response of reduced pain, but in males only. The placebo response was not mediated via emotional factors like reduced stress or arousal. Experiment 1 was conducted by female nurses, and Experiment 2 showed that males consistently reported lower pain to female experimenters, compared to males reporting to male experimenters. Like for placebo analgesia, the modulation of pain report was not related to modulation of emotional factors. Experiment 3 further investigated this issue by including measures of skin conductance and heart rate variability to unconditioned pain stimuli, and to conditioned stimuli signalling the pain stimulus. Physiological reactions were not as reactive to social context as pain report was. Finally, the conditioned

stimulus increased pain report and skin conductance, but did not interact with the gender of the subject or the experimenter.

Conclusions: Placebo analgesia seems to be a mainly cognitive and social phenomenon, and was not related to emotional processes in Experiment 1. Pain report, one very important aspect in placebo analgesia, was strongly influenced by the social context in which the report was given. Pain report from males was reduced by about 50% when they reported to female compared to male experimenters. Pain report in females was also influenced by social context. Thus, the interaction of subject and experimenter gender could play a role in placebo analgesia. Experiment 4 will manipulate subject and experimenter gender during placebo analgesia, while physiological measurements will be made to obtain objective measures of pain responses.

29/02 - "Psychokinesis and telepathy with hypnotised human"
Instituição/*Institution:* Institut International d'Immunologie, Bouguenais – France
Duração/*Duration:* 2003/06 - 2006/03
Investigadores/*Researchers:* Prof. René Peoc'h, Prof. Chauvin

Abstract: ***Telepathy and Hypnosis:*** We try to demonstrate that hypnosis increase human ability for telepathy. It seems that telepathy appears more frequently between two persons when one of them is hypnotised.
We use couples of mother and son to study hypnosis influence on telepathy. The son is hypnotised while his mother try to send him a telepathic message. This mother will look at pleasant or horrible pictures.
We study the son's feelings. We ask him what are his sensations ; horrible or pleasant. We experiment with 16 couples mother-son. The mother look at 50 pictures during 1 minute for each pictures and her son is into an other room at 2 kimometres of distance. By phone the son answer what is his sensation, horrible or pleasant. We do 10 cessions of 50 pictures per couple. Later, an other scientist analyses the totality of the results collected for each couple mother-son.

Results: Men hypnotised are able to receive a telepathy message coming from their mother. The results are better than with the same persons awaked. Statistics give good results $P < 0.01$ %.
Hypnosis increase the telepathy. Telepathy is more frequently observed under hypnosis than when sons are normally awaked.

Psychokinesis and hypnosis: We prove also that 16 young students have better results to attract towers them a robot moving at random on the floor. Previously we demonstate that men are able to attract a robot towards them if this robot is moved by a random number generator. We prove that their results are better when these 16 students are under hypnosis. Each of them is hypnotised alone and sit down in front of a robot (Tychoscope). Perhaps, hypnosis increases psychokinesis because the men are not afraid to miss this experiment when they are hypnotised.

30/02 - "Exploring the limits of human perception: The psychological and physiologica detection of normal and remote staring"

Instituição/*Institution:* Koestler Parapsychology Unit, University of Edinburgh - UK
Duração/*Duration:* 2004/01 - 2005/05
Investigadores/*Researchers:* Dr. Ian Baker, Prof. Paul Stevens

Abstract: This project represents the first piece of research to examine the possibility of the electrocortical processing of remote staring detection, and its potential relationship to face perception in general. This was achieved by conducting two experiments using different measures of electrocortical activity (ERP, EEG, etc), skin conductance, and questionnaire data. Participants were isolated, and an automated, double-blind, randomised and counterbalanced protocol was employed.
Experiment one involved a 2 x 2 design, where 20 participants were exposed to 48 repetitions of the following stimuli: viewing a blank screen, a blank screen plus a remote stare, viewing a face on the screen, and a face plus a remote stare. This experiment found that the addition of a remote stare had no effect on the processing of a blank screen, but

significantly reduced the amplitude of the global processing of faces. There was no correlation between these measures and questionnaire measures of private self-consciousness, social anxiety and paranoia.
Experiment two replicated the overall 2 x 2 design of experiment one, testing an additional 20 participants. However, in this experiment the blank screen conditions were replaced with pictures of objects, resulting in 60 repetitions of the following: viewing an object on the screen, an object plus a remote stare, viewing a face on the screen, and a face plus a remote stare. This experiment found that the addition of a remote stare significantly increased the amplitude of the global processing of faces and objects. There was no effect on skin conductance and no correlation with the questionnaire measures.
To summarise, this project suggests that remote staring detection may have a significant impact on the global processing of other stimuli, but further experimentation is needed in order to understand the nature of this effect, and to ensure the elimination of any potential artefacts.

42/02 - "The effects of exercise and meditation on the psychological stress level and Quality of Life of Cypriot men and women, a multimodal investigation"

Instituição/*Institution:* Multidisciplinary Rehabilitation Centre - PIKA, Nicosia - Cyprus

Duração prevista/*Estimated duration:* 2003/06 - 2004/12

Investigadores/*Researchers:* Dr. Michael Angastiniotis, Dr. Kypros Nicolaou, Dr. Georgia Panayiotou, Dr. Vladimir Demetriou

Abstract: ***Objectives:*** The beneficial effects of physical exercise and relaxation on stress relief and physical and emotional health in general, have been documented multiple times in prior research. However, the evidence regarding the effects on stress level and how lasting these effects are is not consistent. The objectives of this study are the following:
- To investigate the level of stress in the working population of Cyprus, that is men and women aged 35-50 years.
- To investigate the fitness level of the recently urbanised population of this country.

- To investigate whether physical activity history or other factors relate to participants adherence to the exercise prescription and the meditation programme.
- To investigate whether exercise or meditation, alone or in combination have any reducing effects on stress levels.

Methods

195 individuals were approached and invited to take part in this study. 40 accepted and were subjected to the following investigations:

1- All participants were given the PAR-Q (Physical Activity and Readiness Questionnaire), to exclude medically unfit people.

2- All were subjected to an exercise physiology examination to determine their fitness level and to allow an individualised exercise prescription. The following parameters were recorded for each participant: resting heart rate, blood pressure, anthropometric measurements, body composition analysis (skinfold callipers), cycle ergometry including maximal oxygen uptake (VO2max), flexibility tests (sit and reach) and upper body musculature endurance (push-ups, modified push-ups, and curl-up test).

3- The Quality of Life Questonnaire – WHOQol Bref.

4- The Perceived Stress Index Questionnaire.

5- The State Trait Anxiety Inventory (State Form - STAI).

The participants were then divided into randomly into groups:

1- The first group was given an exercise prescription, according to their fitness, strength and requirements to improve their exercise ability. The programme was to be adhered to 3-4 times per week for 20 weeks.

2- The second group followed a programme of meditation. The meditation technique followed is that of Taoist Tai Chi, described as meditation in motion and claiming to release stress and worries. The participants attended one session per week but were expected to practice at home.

3- The third group combined the exercise and the meditation programmes.

4- The control group received no structured intervention.

5- The questionnaires were repeated at 10 weeks and 20 weeks.

Because of the high drop-out the results of the evaluation at 10 weeks is considered for comparative studies.

Results: Baseline results at study initiation showed that the experimental groups and the control group did not differ from each except for on the WHO1 (physical health) measure where the control group and the combined group reported somewhat higher WHO1 than the meditation and exercise groups
Measures of quality of life (QOL) correlate negatively and significantly with measures of stress. Perceived stress correlates positively with STAI scores, proving convergent validation of all dependent measures.
At 10 weeks change in quality of life:
- Physical health (WHO1) – no significant change from baseline.
- Psychology and appearance (WHO2) - there was a significant effect of time showing an increase in WHO2 at 10 weeks as compared to baseline. No significant Time by Group interaction was observed.
- No significant effects of time, group or time by group interaction emerged for WHO3 (Social/personal relationships) or WHO4 (Environment financial resources).
At 10 weeks change in perceived stress:
A significant effect of Time, and a significant time by group interaction, emerged showing that there was significant decrease in stress with participation in the study. Planned post-hoc comparisons did not show any significant differences in Groups from baseline to 10 weeks, but inspection of the means shows that the significant drop in stress tended to be mostly accounted for by the combined and the exercise groups only. There were no significant Time or Time by Group effects on the STAI.

Overall wellbeing: In addition to the specific differences in each quality of life and stress dimensions, it was of interest to examine the effect of interventions on overall well-being. For this reason a composite well-being score was computed by adding the 4 dimensions of WHOQoL and subtracting from those the score of Perceived Stress and STAI. This score was entered as a dependent variable into a repeated measures ANOVA with group as the between subjects variable and Time1 - Time2 as the within subjects variable (only the first two Times were used since there was a significant dropout by Time3). The results showed a marginal effect of Time, $F(1, 17) = 4.17$, $p = 0.06$, with

participants at Time2 showing higher well-being than at Time1. There was no significant Time by Group interaction, indicating that this pattern of improved well-being after participation in the study was similar for all groups. However, inspection of the means indicates that the control group did not show significant improvement in well-being from T1 to T2.

Conclusions: Results indicated firstly a high dropout rate, which hinders solid inferences, but points to people's difficulty in adhering to such a program.
Tentative results pointed to positive effects of Combined Exercise and Meditation on reducing Perceived Stress, and of Exercise and Meditation individually on improving an overall index of well-being, calculated from all the measures combined. Results need to be substantiated by further studies using larger samples.
The lack of commitment to training and meditation programmes in the Cypriots of this age group, which is the majority of the working population is in itself an important observation. The commonest element in the justifications given for non-adherence, but also for not entering the programmes, is related to time. The tasks are regarded as time-consuming and not a priority in their daily routine. Job obligations suddenly appeared for many, while others complained that exercise fatigue was too much for them. Drop out has been reported in by other studies. Those that adhere to these programmes, appear to benefit from exercise and meditation. However, since they appear to be a minority, they may not represent the general population and be a self-selected group with personality characteristics favouring adaptation and stress reduction.

45/02 - "Exploring Psychomanteum as a psi-conducive state of consciousness: Psychological, neuropsychological and parapsichological research of anomalous cognition (ESP) using dynamic/non-dynamic (emotional) visual targets, observation /no-observation conditions, and psychomanteum /non-psychomanteum sessions"
Instituição/*Institution:* Instituto de Psicología Paranormal, Buenos Aires – Argentina

Duração/*Duration:* 2003/03 - 2005/01
Investigadores/*Researchers:* Dr. Alejandro Parra, Dr. Jorge Fernando Villanueva

Abstract: ***Objectives:*** This mirror gazing procedure termed "psychomanteum" was developed to facilitate reunions with deceased individuals, as a way of addressing feelings of bereavement. The purpose of the modern psychomanteum tends to be to facilitate reunions; the aim is not usually to seek ESP information about the future. However, the aim of this research project was explore if the psychomanteum is a psi-conducive state of consciousness above chance expectation, and –if it works– would be related to an altered state of consciousness or not.

Method: There are many similarities and differences between psychomanteum experiences and accounts of hypnagogic /hypnopompic imagery. The hypnagogic-like imagery could be psi-conductive. One hundred thirty-three participants (both 95 females and 38 males; Mean age= 47.44; SD= 12.02), were recruited by announcements in newspapers and magazines. Ninety six of them (78%) claimed to have had sometimes ESP experiences, 51 of them claimed to have ESP ability (41.8%), and 52 of them claimed have not ESP ability (42.6%). Each subject received seven questionnaires Pre–psychomanteum Questionnaire, Psi Previous Experiences, Betts's Vividness of Imagery Scale, Barrett's Hallucinations Questionnaire, Neo Personality Inventory –Revised, Schizotypal Personality Questionnaire, Revised Physical Anhedonia Scale and Phenomenology of Consciousness Inventory. Two conditions, psychomanteum and non-psychomanteum condition, were performed. Both conditions were blind to the experimenter, receiver, and sender.

Results: Under psychomanteum condition, psi-hitting was obtained (29.2%, notably above chance expected); however, under no-psychomanteum ("control") condition, 24.6% was obtained. The results differ significantly from mean chance expectation in psychomanteum condition (p= .03) in comparison with no-psychomanteum condition, but no significant differences were found.

Conclusions: These interesting results seems to suggest that psychomanteum condition somehow optimizes psi-communication. A number of positive correlations were also found, for instance, subjects who scored higher Auditory (p= .005) and Visual hallucination (p= .008) scores tended to score psi-hitting. Sixty six participats underwent by two type of targets, video-clip (dynamic) and image-picture (no-dynamic), but no significant results were found.

51/02 - "Psychological and Parapsychological Investigations of Alleged Alien Abductees: Phase I"

Instituição/*Institution:* Anomalistic Psychology Research Unit, Goldsmiths College, University of London – UK

Duração/*Duration:* 2003/10 - 2006/01

Investigadores/*Researchers:* Prof. Christopher Charles French, Dra. Julia Santomauro, Dr. Michael Thalbourne

Abstract: ***Objectives:*** Previous research has suggested certain psychological differences exist between those who report memories of alien contact ('experiencers') and the general population. This project aimed to replicate some previously reported findings with a UK-based sample of experiencers (N = 19) as well as to collect data on some hitherto uninvestigated psychological and parapsychological measures.

Methods: The project included the following components: (a) participants completed a batch of pencil-and-paper tests measuring psychological variables of interest; (b) they completed a word-list-based experimental test measuring susceptibility to false memories; and (c) they completed computerised tests of clairvoyance, precognition and psychokinesis. Responses from experiencers were compared with age- and gender-matched controls who did not have any memories of extraterrestrial contact.

Results: Experiencers scored significantly higher than the control group on the Australian Sheep-Goat Scale and on the Anomalous Experience, Paranormal Belief and Paranormal Ability sub-scales of the Anomalous Experience Inventory. They also scored significantly higher than the control group on the Launay-Slade Hallucination Scale, Tellegen's

Absorption Scale, Goldberg's Curious Experiences Survey (measuring dissociativity), the Inventory of Childhood Memories and Imaginings (measuring fantasy-proneness), and incidence of sleep paralysis. No differences were found between the groups on the experimental measure of susceptibility to false memories or the experimental tests of psychic ability.

Conclusions: In general our results are consistent with those psychological models of the alien encounter experience that posit that such anomalous experiences may be a reflection of problems with reality monitoring, i.e., our ability to distinguish between events which take place out in the real world and those that occur only in our subjective mental space (via imagination, fantasy, dreams and so on). Absorption, dissociativity and fantasy proneness have all been shown to be correlated with susceptibility to false memories.

52/02 - "A Qualitative Analysis of Rapport and Alignment in Experimenter-Subject Interaction in Ganzfeld Experiments"
Instituição/*Institution:* Department of Sociology, University of York – UK
Duração/*Duration:* 2003/07 - 2004/08
Investigador/*Researcher:* Dr. Robin Wooffitt

Abstract: ***Objectives:*** There is experimental and anecdotal evidence that the relationship between experimenter and subject may have an important bearing on the outcome of ganzfeld experiments. This project was designed to provide preliminary analyses of verbal interaction between experimenters and subjects so as to identify the primary characteristics by which different experimenter interactional styles could be identified. The primary goal of the project was to examine the communicative strategies through which experimenters and subjects establish rapport, or a mutually positive relationship during the mentation review phases of the ganzfeld experiment.

Data and Method: The data for the research was a corpus of recordings of mentation review sessions from ganzfeld experiments conducted at

the Koestler Parapsychology Unit, University of Edinburgh, in the mid 1990s.
The verbal communication between the experimenter and subject in these reviews was analysed using a specialised qualitative method for the analysis of interaction, known as Conversation Analysis (CA). CA is concerned to examine how turns turns at talk perform actions: greetings, questions, answers, requests, assessments, accusations, agreements, rebuttals, clarifications and so on. One of its distinctive features is the analysis of how any particular utterance is designed to 'fit' with, or show its relevance to, a prior turn. By using CA we were able to explore how successive turns cohere into action sequences which exhibit recurrent and stable properties.

Results and conclusions: The research has identified the primary sequential and communicative features of experimenter-subject interaction in the mentation review. It has so far has identified three discursive strategies through which alignment can be established: the use of 'you said' prefaces to introduce mentation items into the review; experimenter departures from the core tasks of the review; and subject initiated recipiency. It is anticipated that the results of this analysis will allow some preliminary investigation of variations in communicative practices employed by psi-conducive and psi-inhibitory experimenters.

54/02 - "Further developments and Applications of the Digital Ganzfeld"

Instituição/*Institution:* Psychology Department, University of Gothenburg - Sweden
Duração/*Duration:* 2003/02 - 2006/03
Investigadores/*Researchers:* Prof. Adrian Parker, Prof. Joakim Westerlund

Abstract: Recent meta-analyses of the psi-ganzfeld as well as the results of our own work at Gothenburg University indicate that the ganzfeld technique is still like other methods in parapsychology by being experimenter dependent and that it can produce both significant negative scoring or psi-missing as well as psi-hitting. Efforts are directed at Gothenburg towards an understanding of what the

determinants of success are. The use of the digital real time ganzfeld has enabled us to identify particularly impressive sequences of the ganzfeld imagery which correspond to and are synchronised with the content of the target film and as such which may lie behind the psi-hitting and missing. In collaboration with our colleagues at Stockholm University, we have studied the effects of subjective validation in evaluating what appear to be "remarkable hits" qualitative hits. The preliminary results suggest that much caution is needed in the use of qualitative hits without the accompanying clear evidence of psi-hitting and that more work is needed to distinguish possible true psi markers from subjective good but false correspondences. We have used the real time technique in order to conduct further studies which have examined learning effects in relation to a psi-conducive state, the arousing content of the target film, the importance of the sender-receiver relationship and the use of an experienced external judge. Although some significant findings have occurred, their meaning is as yet unclear. A study using the real time digital ganzfeld is being conducted with biological closely related pairs (monozygotic twins) of participants. The aim is two-fold: to evaluate previous claims and counter claims concerning "twin telepathy" using an appropriate technique and to collect qualitative data which might occur with potentially high scoring. In this latter case, we would be able to obtain qualitative data which would enable us to go further with identifying true psi-markers. Some preliminary work has also been done on developing a remote viewing technique which will be analogous to the real time ganzfeld and on possible links between successful target material and precognitive habituation.

57/02 - "Implicit Learning and Parapsychology: Exploring the boundaries of Unconscious Processes"

Instituição/*Institution:* Queen Margaret University College, Edinburgh – UK

Duração/*Duration:* 2003/04 - 2005/07

Investigador/*Researcher:* Dr. Stuart Wilson

Abstract: Two series of experiments were conducted investigating the potential role that implicit learning might play in parapsychology.

Series 1 looked at “hidden covariation detection”. Participants were asked to make a “psychic prediction” concerning the position of a target card. Covarying with the position of the target was a subtle cue that it was hoped would be unconsciously utilised. No effect was found initially, but participants performed above chance when there was no cue present. This was an artefact arising from the unintentional body language of the “dealer” in the video clips used. When the dealer was removed, no effects were found.

A second version of the hidden-covariation experiment involved asking participants to “psychically” determine personality features associated with faces, having previously been shown face/personality descriptions purportedly generated by psychics. Again, an unconscious cue was present to guide responses (e.g. long face = warm personality). Results suggested that, rather than using the cue that was present, participants had an implicit personality theory that led them to associate certain faces with certain traits.

Series 2 employed a “sequence-learning” paradigm, in which participants took part in a “psi” experiment in which they had to predict which one of 4 cards would appear next. The presentation of the cards was determined by a nonsalient repeating pattern. It was hypothesised that participants would implicitly learn the sequence and thus score more “hits”. Initial results suggested that participants were scoring above chance, but could not articulate the sequence. This effect was found to be the result of participants using a very simple heuristic that was unrelated to the sequence. Sequences were then developed that could not be exploited by means of any simple heuristic strategies. Further studies were conducted, finding some evidence of implicit learning, although significance was marginal. “Hits” were found to be faster than “misses” in the sequence condition, suggesting that implicit learning may have been having an influence. Extraversion was found to be positively correlated with performance on the sequence-governed trials, whilst the extent to which participants claimed to be responding “randomly” in the experiment also correlated with success on the sequence-governed trials.

It is concluded that implicit learning may be a factor in parapsychology, although questions remain concerning its pervasiveness.

66/02 - "Considering the sender in ostensible ganzfeld ESP studies to be a PK source"

Instituição/*Institution:* The University of Northampton – UK
Duração/*Duration:* 2003/12 - 2005/12
Investigadores/*Researchers:* Prof. Chris Roe, Ms. Nicola Holt

Abstract: Despite the success of clairvoyance designs in eliciting evidence for ESP when used with other protocols, it has been commonly assumed that a sender can make a positive contribution to the outcome of Ganzfeld studies. Few Ganzfeld experiments have adopted a clairvoyance design except where the objective of the study was to compare sender and no sender conditions (Honorton, 1995). Unfortunately, work which has focused on this issue (e.g., Williams et al., 1994) has involved rather crude manipulations of conditions in comparing success rates for sender and no-sender sessions. Perhaps unsurprisingly, then, such research has failed to provide a clear or consistent picture of the role of the sender.

This project took an alternative approach (after Roe, 1996) that used a novel means of measuring any sender effect by having an RNG serve as a 'proxy' receiver. In Study 1 in this series — essentially an assessment of the validity of the approach — a live RNG ran alongside the human receiver and produced a 'virtual mentation' by randomly selecting descriptors from a pool of statements. We found that an independent judge using virtual mentations was able to achieve a 32.5% hit rate. In study 2 we compared sender and no sender conditions and recruited a second independent judge to assess any possibility of psi on the part of the judge. They produced hit rates of 30.6% and 16.7%, with both giving higher hit rates for the sender trials than for the no sender trials, as predicted (42.1% vs 17.6%; 26.3% vs 5.9%), though these differences were not significant.

In Study 3 the human receiver was removed by accurately briefing participants about the RNG and asking them to focus on it while it gave immediate feedback by displaying selected statements to them, much as senders might hear feedback from a human receiver in the ganzfeld. Senders rated how well each statement corresponded with their sending experience. The lability of the target selection method was manipulated

to give three within-subjects conditions; using a random number table, a pseudorandom process, and a live RNG. It was hypothesised (following Braud, 1981) that the greatest psi effect would be with the most labile target, and (following Stanford's [1978] CBM model) that 'stable' senders would demonstrate higher psi hitting with the most labile target system and vice versa. Although we did not confirm Braud's claimed advantage for labile target systems, there was a significant interaction effect between target and sender lability, as predicted by CBM.
Study 4 was designed to replicate this interaction effect and also to consider the impact of sending strategy ('willing' versus passive absorption in the task) and feedback type (delayed versus item by item) upon success. The interaction effect was replicated and a non-significant interaction between feedback and sending strategy was found.

76/02 - "Extended Communication of Affective States: physiological and emotional responses to non-sensory stimuli" - only abstract available

Instituição/*Institution:* Koestler Parapsychology Unit, University of Edinburgh – UK
Duração/*Duration:* 2004/01 - 2005/05
Investigador/*Researcher:* Prof. Paul Stevens

Abstract: This study investigated emotional responses (physiological and self-report) to external, non-sensory stimuli. 20 right-handed subjects underwent a blind, pseudo-randomised schedule of 12 stimuli, consisting of 60s exposures to one of two MF types, a remote "Sender" undergoing autobiographical recall of positive or negative emotional states, or control periods (no stimulus). MF types 1 and 2 were sine waves of a frequency (range 8.3-12.2 Hz) corresponding to peak frontal EEG frequency previously recorded for that subject during autobiographical recall of negative or positive emotions respectively. After each period, subjects selected 4 words out of 16 (8 negative, 8 positive) which best described their current emotional state, and these used to construct a Reported Emotional State (RES) index.

During exposure periods, EEG data on twelve scalp positions (10-20 system: F3, F4, C3, C4, T3, T4, P3, P4, O1, O2, Fp1, Fp2) were used to calculate z-normalised Global Field Power (GFP). Power spectra were constructed for electrodes F3 and F4 and used to calculate alpha frontal asymmetry (FA). Mean normalised SC levels were calculated for each stimulus period.
For the MF stimuli, 57% of subjects showed a non-significant decrease in SC arousal relative to control stimuli (Wilcoxon V=98, p=0.41, 1-t) and 65% of subjects exhibited a significant decrease in GFP relative to control stimuli (Wilcoxon V=48, p=0.03, 1-t). There were no significant differences between MF types, hemispheric GFP, or FA. RES was significantly higher after MF stimulation (Wilcoxon V=189, p=0.002, 2-t), suggesting a more positive reported emotional state. There was no significant difference between MF types.
For the "Sender" stimuli, 45% of subjects showed a non-significant decrease in SC arousal relative to control stimuli (Wilcoxon V=110, p=0.58, 1-t) and 60% of subjects exhibited a non-significant GFP decrease relative to control stimuli (Wilcoxon V=316, p=0.11, 1-t). FA again showed no significant differences. RES index was again significantly higher after "Sender" stimulation (Wilcoxon V=176, p=0.001, 2-t), suggesting a more positive reported emotional state "Sender" affect type showed no significant differences.
It is suggested that subjects interpret the MF-induced or Sender-related change in arousal as an ambiguous emotional event, opting in the absence of any strong external cues for the default positive interpretation that the affect literature suggests is the human norm.

82/02 - "Comparative study of brain processes related to space-induced and clinical oculomotor disturbances"

Instituição/*Institution:* State Research Centre RF Institute for Biomedical Problems, Moscow – Russia

Duração/*Duration:* 2003/02 - 2005/01

Investigadores/*Researchers:* Prof. Inessa B. Kozlovskaya, Ms. Elena Tomilovskaya, Dra. Anna Kirenskaya, Dr. Vladimir Novototsky-Vlasov, Dr. Vadim Myamlin

Abstract: ***Objectives:*** The aim of the study is the quantitative investigation of the slow presaccadic potentials in tasks with visually-guided saccades and antisaccades under normal conditions, during simulated weightlessness and in schizophrenic patients.

Methods: The data of 14 control subjects (group C), 23 patients (group Sch) and 6 volunteers, exposed to 6-day dry immersion (group DI) were analyzed. All participants had right hand and right eye preference. EEG was recorded from 19 sites. Mean amplitude of averaged slow potentials (time constant – 5sec) was assessed at 600 ms before the target stimulus (TS) and at 600 to 100 ms before saccade onset. Statistics comprised MANOVA and t-test.

Results: The saccade characteristics did not differ in groups C and DI. Patients exhibited in the antisaccade task delays in the performance of correct saccades ($p<0,05$) and larger number of directional errors ($p<0,01$). In control subjects the TS was preceded by a vertex predominantly bilateral slow negative potential shift; left- and right-side saccades were preceded by PSN, that was larger at the midline and left sites, with parietal maximum before visually-guided saccades, and with frontal and parietal maximums before antisaccades. The asymmetrical topography of PSN distribution over the cortex reflects probably the peculiar selection of right-handed males with the right eye preference that participated in the study. The PSN amplitude declined significantly in the DI and Sch groups as compared with the C group. However, in immersed subjects the foci of negativity shifted to the right hemisphere so that the PSN amplitude decreased sharply in the left and increased in the right hemisphere, with significant decrease in Fz, F3, Pz and P3. Significant decline of the PSN amplitude was the most characteristic in schizophrenic patients in the sagittal frontal and central regions (Fz and _z).

Conclusions: We suppose that results obtained in microgravity reflect the left hemisphere and prefrontal cortex deactivation due to the alterations of sensory inputs' activities in DI (elimination of support loading and decline of proprioceptive activities); the changes revealed

in schizophrenic patients are linked to frontal disorders in schizophrenia. The results of study have shown that the characteristics of presaccadic potentials in antisaccadic task could be used as objective measures of frontal lobes functioning.

88/02 - "A neuropsychological examination of orbitofrontal cortex function in eating disorders"
Instituição/*Institution:* Institute of Psychiatry, King's College London – UK
Duração/*Duration:* 2003/03 - 2004/09
Investigadores/*Researchers:* Prof. Janet Treasure, Prof. Kate Tchanturia

Abstract: ***Objective:*** This study examined decision making ability in people currently ill and recovered from anorexia nervosa (AN). It tested the hypothesis that impaired decision making in AN is associated with the absence of anticipatory rises in skin conductance during high risk (disadvantageous) conditions.

Method: Patients with AN (n=29), healthy control (n=29), comparable in age and IQ, and women long term recovered from AN (n=14) completed the Iowa Gambling Task (IGT), during which skin conductance was measured.

Results: People with current AN made disadvantageous decisions in the IGT compared to both controls and recovered AN participants (ANOVA p=0.03). Patients who were currently ill, but not those who were recovered, showed significantly diminished anticipatory skin responses before making high-risk choices (p=0.04). Performance on the IGT was not related to current levels of depression or anxiety.

Conclusions: Results suggest that impaired decision making is a state marker of AN and improved decision making is associated with recovery. Blunted peripheral autonomic response to emotional stimuli is consistent with dysfunction of the ventromedial prefrontal circuits underlying the deficit in adaptive decision making seen in patients with AN.

90/02 - "Brain function, creativity, paranormal ideation and risk for psychosis"

Instituição/*Institution:* Brain Image Analysis Unit, Institute of Psychiatry, King's College London – UK

Duração/*Duration:* 2003/03 - 2005/04

Investigadores/*Researchers:* Dr. Alex Sumich, Prof. Michael Brammer, Dr. Dominic ffytch

Abstract: ***Background:*** Understanding the aetiology of psychosis may improve through examination of symptoms at varying levels on the schizophrenia spectrum. Studies support a syndrome-dependent relationship between brain function and schizotypy in healthy individuals. Sex differences may also exist. Event-related potentials (ERPS) offer a direct measure of brain function and have been used to study schizophrenia and genetically and/or psychometrically defined at-risk groups. Methods: Seventy-nine healthy individuals (n=42 men) completed an auditory oddball task and self-report questionnaires that measured supposed subclinical positive symptoms: Paranoia / Suspiciousness (PS) and Paranormal-Ideation (PI). N200 and P300 amplitudes were measured at 15 electrode sites. Mean-split methods were used to form groups based on high and low scores for PS and PI. Sex differences were also investigated. Results: High-PI women had enhanced N100 and anterior N200, but reduced anterior P300 amplitudes. High-PI men had enhanced N100 and reduced left P300 amplitudes. High-PS individuals had reduced anterior N100 amplitudes. N200 amplitudes were reduced in high-PS women and enhanced in high-PS men compared to their low-PS counterparts. Conclusion: The current results provide evidence for the existence of an association between basic cognitive functions and positive subclinical symptoms in healthy individuals without a family history of psychosis, similar to that seen in psychosis. The construct of a schizophrenia spectrum ranging from healthy to clinically relevant is supported for some aspects of the illness.

95/02 - "I just had this feeling...": A Somatic Marker Approach to Understanding Intuitive Decision Making"

Instituição/*Institution:* The University of Northampton - UK
Duração/*Duration:* 2003/04 - 2005/12
Investigador/*Researcher:* Prof. Richard Broughton

Abstract: In the search for physiological indicators of precognitive intuition recent work has shown that physiological responses of the emotional system appear to anticipate future emotional shocks. In conventional neuroscience Damasio and colleagues have shown that similar physiological responses play a role in decision making. In an experiment in which the participant learns to avoid 'risky' decisions the Damasio team demonstrated that the emotional system learns which decisions are risky before the participant is cognitively aware of the risk. Damasio has suggested that this role of the emotional system may underlie conventional intuitive decisions.

It is argued that if precognitive intuition exists as a human ability the efficiencies of evolution could have created a system that merges precognitive information input with existing (emotional) systems that support the decision-making process.

The first of two experiments to examine intuitive decisions making was a replication of the basic Bechara-Damasio experiment that additionally explored individual differences. Fifty participants completed the MBTI and NEO-FFI, and a computerized version of the Iowa Gambling Task (IGT) while skin conductance responses (SCR) were monitored. The second experiment, with 24 participants, was similar to the first, but the IGT was modified to be a test of true precognitive intuition (by randomizing the card deck placement).

In the first experiment the IGT behavioural results (card-selection) demonstrated a significant learning effect, but not as early in the run as expected. SCR results were broadly similar to that found by the Damasio team but failed to reach significance. None of the personality factors significantly discriminated IGT behavioural performance, though the NEO-FFI openness to experience factor was suggestive that participants high on openness preferred risky choices. MBTI judging-perceiving facet was negatively correlated with anticipatory SCRs suggesting that

judging oriented participants produced more SCRs. IGT behavioural performance most closely represented recent research that shows university education may attenuate emotional learning in the IGT, and it is suggested that something similar may be operative in this experiment.
The second experiment showed no overall evidence of precognitive intuition either in the behavioural or the SCR data. MBTI facets extroversion-introversion and judging-perceiving correlated significantly with the number of punishments received, with the extraverts and perceiving types receiving fewer punishments than introverts and judging types; however these significances must be treated cautiously.

114/02 - "The Measurement and Characterization of Charge Accumulation and Electromagnetic Emissions from Bioenergy Healers"
Instituição/*Institution:* Field and Matter Interactions Research Laboratory, Duke University – USA
Duração/*Duration:* 2003/05 - 2005/11
Investigadores/*Researchers:* Prof. William Joines, Prof. Stephen Baumann

Abstract: In this research study the subject under test is asked to focus and to direct their mental energy into a region of space. This energy may be in the form of healing intent directed toward another person, nearby or at a distance, or the subject may choose to focus or concentrate their mental energy onto one of the instruments measuring voltage or light. During the subject's directed or focused intent we measure charge build-up and decay that may occur on the skin surface. For this we use from 1 to 18 electrodes. To record even slight variations over time, the voltage or charge at electrodes arrayed on the body is measured using a very sensitive voltmeter (Keithley model 6514).
These measurements on volunteer subjects are conducted in an electrically shielded darkroom, where we also measure faint amounts of light that may be emitted from the subject. For this we use a cooled (-30 deg C) photomultiplier tube system that counts photons of light. In our study we also monitor the outputs from an argon-cooled IR camera and a gauss meter that are located near the subject.

In a pilot study conducted over a span of several months, long before the present research project was started, we repeatedly measured charge build-up and decay, and concurrent light emission from one subject during periods of healing intent. Subsequent to our pilot study, another research laboratory has measured charge build-up and decay on the body of several subjects during periods of focused intent, but they made no attempt to measure light emission.
In the present study we have tested 35 people (10 controls and 25 healers), many of them multiple times, and we have evidence that one of the healers did something quite extraordinary twice in one testing session. This subject emitted bursts of blue/ultraviolet light and concurrent charge buildup on the body as measured by our electrodes. Also, we have recorded evidence that many of the healers produced abundant heat emission (infrared light) from their hands and faces as recorded by our infrared camera.
The testing results that we have obtained to date have been presented as a paper at the 2005 Parapsychology Association Conference and as a slide presentation at the 2005 Society for Scientific Exploration Conference. As part of this final report to the BIAL Foundation, the paper presented at the PA Conference is included here and it begins on the next page.

118/02 - "Differential Responses to target vs. Non-Target Psi Stimuli: An Event-Related fMRI Study"
Instituição/*Institution:* Harvard University Psychology Department and NMR Center – USA
Duração prevista/*Estimated duration:* 2003/01 - 2006/03
Investigadores/*Researchers:* Prof. Stephen Kosslyn, Dr. Sam Moulton

Abstract: In its most conservative instantiation, the psi hypothesis asserts the anomalous presence of knowledge. A direct and promising strategy to quantify potential psi effects lies in powerful neuroimaging approaches that make few assumptions about the nature of such knowledge. By circumventing behavior and directly examining brain activity we can increase the likelihood of uncovering a psi effect, and decrease the ambiguity of a null result. In this study, we relied upon the

robust finding within cognitive neuroscience that prior knowledge of a stimulus biases the brain's response to that stimulus, even when the prior knowledge is unconscious. We hypothesized that knowledge acquired through psi would similarly prime neuronal activity, and that evidence of priming associated with psi stimuli would offer compelling evidence for the existence of psi. Sixteen participants completed a simple binary guessing task while their brain activity was monitored using functional magnetic resonance imaging (fMRI). In this task, participants viewed two sequential photographs and were asked to decide which image would shortly be randomly chosen by the computer; after participants indicated their choice on each trial, they were shown the target stimulus from the pair a second time. Outside the scanner, an emotionally-close or biologically-related partner viewed only the target stimuli. Our experimental protocol thus had three potential sources of psi stimuli, corresponding to the three hypothesized mechanisms of anomalous information acquisition: precognition, telepathy, and clairvoyance. We analyzed Blood Oxygen Level-Dependent (BOLD) activity associated with target versus decoy stimuli in search for evidence of psi. Results from this contrast – as well as several others of purely psychological interest – are discussed.

124/02 - "The Flexibility of Physical Body Boundaries and its Relationship to Out-of-Body Experiences"
Instituição/*Institution:* Liverpool Hope University College – UK
Duração/*Duration:* 2003/08 - 2004/12
Investigadores/*Researchers:* Dr. Craig Murray, Dr. Jezz Fox

Abstract: This project explored the relationship between Out-of-Body Experiences (OBE) and the flexibility of body boundaries. The project was built around the argument that OBEs occur due to the exacerbation of pre-existing body image disturbances. We further postulated that the nature of this experience was a generalised dissociation (as compared with non-OBE experients) between their self and body that can be assessed on a number of levels: perceptual, behavioural, affective and social.

This project consists of three main studies. In study 1 a survey was conducted with 243 respondents who completed a number of measures of body experience. As predicted, OBE experients (OBErs) (n=62) exhibited higher levels of body dissatisfaction, more Somatoform Dissociation, higher levels of self-consciousness, and lower levels of confidence in physical self-presentation than participants without prior OBEs (non-OBErs) (n=181).

In study 2 participants explored a computer-generated Virtual Environment (VE) by way of a head-mounted display and 3-d mouse. This particular VE did not include a visual representation of the participant's body. Such VEs have previously been found to be conducive to decreasing participants' degree of body awareness and has parallels with other aspects of OBEs. OBErs were found to experience less body awareness during use of the VE than non-experients non-OBErs, thus demonstrating the susceptibility of OBErs to certain techniques designed to undermine the experiential boundaries of the body.

In a third study, we examined OBErs and non-OBErs behavioural embodiment during immersion in a Virtual Reality system. It was hypothesised that OBErs would exhibit a disembodied behavioural interaction with the Virtual Environment (VE) (characterised by the proportion of time spent navigating the environment from an elevated position and the number of collisions with virtual objects) than non-OBErs. It was also hypothesised that OBErs would score higher on measures of absorption, dissociation and somatoform dissociation. There were no significant differences between OBErs (n=16) and non-OBErs (n=28) on Behavioural Embodiment, although there was a positive correlation with number of OBEs and proportion of trial time in an elevated position. OBErs were found to score significantly higher on measures of Absorption, Dissociation and Somatoform Dissociation than the non-OBErs.

We would argue that the differences between our OBE and non-OBE sample in these studies on these body experience dimensions are further examples of how the OBEer experiences a dissociation between the self and body, and as such lend further support to a dissociational model or theory of the OBE.

127/02 - "Pursuing Psi in a Non-EuroAmerican Culture: Behavioral DMILS in Bali" - only abstract available

Instituição/*Institution:* College of Arts and Sciences, Rollins College, Winter Park – USA

Duração/*Duration:* 2003/03 - 2005/03

Investigadores/*Researchers:* Prof. Hoyt Edge, Prof. Luh Ketut Suryaní

Abstract: ***Objectives:*** The grant funded two DMILS studies and a comparison of Balinese and American responses on the BVCQ. In the DMILS studies, we sought to get more successful results through using two special populations as Helpees.

Methods: The DMILS task was for the Helper to use psi to help an untrained meditator (Helpee) in a sensorily isolated room to increase their focus meditation on a lit candle. In the first DMILS study we used traditional Balinese healers (Balian) as Helpers.
The second DMILS study used trained Resident Interns in Psychiatry as Helpers.
The Balinese VCQ, an 82 question survey concerning volitional competence, was given to 280 Balinese and Americans and comparisons were made in their responses.

Results: In the first study, a total of 40 runs were carried out, half with Balian and half with meditators. We found no overall difference between presses in the control condition (mean = 2.6, SD = 2.46) and help condition (mean = 2.5, SD = 2.21), t(39) = 0.44,
ns, two-tailed, d = .05, power = .05. We also found a no significant difference in the pis scores between the meditators and Balians, t(38) = 1.74, p = .09, two-tailed, d = .56, power = .39.
In the second study, 80 runs were carried out. Again, there was no significant psi effect (Control (M = 3.07, SD = 2.57), Help (M = 2.91, SD = 3.04), t(68) = 0.61, ns, one-tailed).
A comparison of Balinese and American responses on the Balinese Volitional Competency Questionnaire yielded a number of differences. Most significant among these was support that the Balinese employ secondary control (the person tries to fit in with the world and "flow"

with it) that is tied closely with collectivism, while American responses indicated primary agency (people attempt to change the world so that it conforms to their needs and desires) that is correlated with individualism.

Conclusions: The two DMILS studies changed the types of Helpees in the DMILS studies, but we were unsuccessful in either manipulation in getting positive results. On the other hand, the BVCQ yielded important cultural differences in terms of volition and reflected differences between individualist and collectivist cultures.

136/02 - "Factors Affecting the Relationship Between Human Intentionality and the Hemolysis of Red Blood Cells"
Instituição/*Institution:* Rhine Research Center, Durham – USA
Duração/*Duration:* 2003/05 - 2005/03
Investigadores/*Researchers:* Prof. John Palmer, Prof. Stephen Baumann, Dr. Christine Simmonds, Dr. Colleen Rae, Ms. Anne Poole

Abstract: The purpose of the experiment was to see if participants (Ps) could psychokinetically retard the hemolysis of red blood cells from a nearby room. 20 spiritual healers completed 2 sessions and 40 non-healers 1 session. Hemolysis was induced by adding 50 ml of blood to 3ml of .425% physiological saline solution in a cuvette and was measured by a spectrophotometer. Each session included 2 counterbalanced test and baseline runs, each containing 8 1-min trials. Influence was attempted only on trials 4 and 5 of the test run; all other trials were control. Overall hemolysis scores were nonsignificant and did not differ significantly between healers and non-healers. Results were not directly affected by whether the manipulated DC component of the geomagnetic field (GMF) around the cuvette was present or absent. Consistent with a previous finding, ambient GMF 1 day before the test session was suggestively higher for retardation than acceleration 1st sessions ($p = .081$, 1-t). Healers scored significantly higher than non-healers on the Spiritual Transcendence Scale (STS), and high STS non-healers expressed the most confidence in task success. There were 2 significant post-hoc effects in 1st sessions. Non-healers under age 31

tended to retard hemolysis while those over 34 tended to accelerate it. The 2nd finding involved the combined test and baseline runs but is a psi effect because the hemolysis tester was blind to run order. Ps who received the test run 1st significantly retarded hemolysis if the manipulated GMF was off and significantly accelerated hemolysis if the GMF was on. The effect in the GMF-off condition was significantly stronger for Ps with "thin" boundaries on a short form of the Hartmann Boundary Questionnaire. All these results should be considered tentative pending replication (or, in the case of the ambient GMF finding, further replication).

139/02 - "Experimenter effects and Psi performance using a digital autoganzfeld system"

Instituição/*Institution:* Liverpool Hope University College – UK
Duração prevista/*Estimated duration:* 2003/10 - 2006/03
Investigador/*Researcher:* Prof. Matthew D. Smith

Abstract: The replicability of ganzfeld-ESP findings continues to be debated by parapsychologists and their critics. Similarly, the 'experimenter effect' (where some experimenters are consistently more successful than others in obtaining evidence for psi) continues to be a major challenge facing experimental parapsychology. This project addresses both of these concerns.
Sixteen experimenters were trained to use DigiGanz, a digital autoganzfeld system developed in the psychology department at Liverpool Hope University, in order to conduct 8 ganzfeld trials each. Experimenters were recruited on the basis of their prior attitudes towards psi, with the aim of recruiting those obtaining either high or low scores on a measure of attitudes towards psi. Experimenter expectancy regarding the likely success of the experiment was manipulated so that half the experimenters were given a positive expectancy of success and half were given a negative expectancy of success. The effects of these independent variables upon participants' confidence of success and actual performance on a ganzfeld-ESP task were assessed. No previous research has used this approach with the ganzfeld paradigm, nor has any previous research discriminated

between the experimenter's a priori attitudes towards psi and his or her more specific expectations about the outcome of the experiment.

147/02 - "The Manipulation of Ganzfeld ESP Performance by the Control of Implicit Percipient Variables"
Instituição/*Institution:* Rhine Research Center, Durham – USA
Duração/*Duration:* 2003/01 - 2005/09
Investigadores/*Researchers:* Prof. James Carpenter, Dr. Christine Simmonds

Abstract: ***Objectives:***
• To identify implicit aspects of percipient experience that discriminate ganzfeld performance
• To develop experimental manipulations that heighten psi-conducive aspects of experience, and diminish psi-disconducive aspects
• To demonstrate the effectiveness of these manipulations in new ganzfeld data
• To examine other questions of percipient experience and sender-receiver context

Methods: In Stage One, analyze 190 session transcripts of ganzfeld data previously collected in terms of a number of variables measuring aspects of implicit attitude, emotional adjustment and perceptual style. Design and pilot-test experimental manipulations intended to facilitate psi-conducive aspects identified in Stage One. In Stage Two, test 80 percipients (40 active in creative pursuits and 40 claiming previous psi experiences) randomly assigned to experimental or control conditions.

Results: Two variables, having to do with the experiences of positive self-transcendence and intellectualization were selected in Stage One, and conditions designed to heighten the first and diminish the second were designed and tested in Stage Two. Overall scoring in Stage Two data was not different from chance. The condition designed to raise scoring failed to do so, and it also failed to yield more psi-conducive aspects of experience. Other predictions were confirmed in that overall significant psi performance was observed in the form of an excess of

extreme ranks; performance was better in emotionally close sender/receiver pairs and in opposite-gender pairs; and when percipients experienced the session in a more “altered” way in terms of lowered bodily awareness and loss of body boundaries, performance was better.

Conclusions: The hypothesis that heightening a sense of merger and diminishing an intellectualized approach in ganzfeld percipients would be psi-conducive received no support, but actually the hypothesis failed to be tested in these data because these conditions were not successfully manipulated. While at chance in terms of scoring rate, as predicted the scores were significantly extreme and meaningfully related to percipient experience and interpersonal context.

2004

14/04 - "Detection and Utilization of Consciousness-Related Information Fields Stimulated in Coherent Group Environments (FieldREG)" - only abstract available

Instituição/*Institution:* Princeton Engineering Anomalies Research (PEAR), New Jersey - USA
Duração/*Duration:* 2005/01 - 2006/02
Investigadores/*Researchers:* Prof. Robert G. Jahn, Dr. Brenda J. Dunne, Dr. York H. Dobyns

Abstract: Previous study at the PEAR Laboratory has established a technology, protocol, and analytical strategy capable of assessing the degree of resonance prevailing in a broad variety of public and private group environments. Under this particular BIAL-sponsored project we have extended this work to include:
- development of an elaborate database management system capable of extracting many psychological and physical correlates of the anomalous effects appearing in such “FieldREG” experiments;
- formulation of theoretical hypotheses regarding the source of the effects and the conditions favoring their appearance and enhancement;
- design and implementation of a new generation of FieldREG equipment and software;

- acquisition of fresh experimental data to confirm and refine the theoretical models;
- investigation of possible pragmatic applications of the FieldREG effects in a variety of beneficial contexts.

Details of the experimental and analytical methods and the pertinent theoretical models are presented in our Interim and Final Reports, and in a number of archival articles that are downloadable from our website <www.princeton.edu/~pear/> (see especially items no. 26: “FieldREG Anomalies in Group Situations”; 28: “FieldREG II: Consciousness Field Effects: Replications and Explorations”; 40: “A Modular Model of Mind/Matter Manifestations (M5)”; 44: “Sensors, Filters, and the Source of Reality”; 6: “Endophysical Models Based on Empirical Data”; 7: “Consciousness, Information, and Living Systems”; and 5: “The PEAR Proposition”). The results of this project confirm that FieldREG responses, when produced in environments fostering relatively intense or profound subjective resonance among the participants, can show large deviations from chance expectations. Venues that appear to be particularly conducive include intimate gatherings, group rituals, ceremonies at sacred sites, musical and theatrical performances, and other charismatic events. Applications in certain aspects of allopathic and alternative medical diagnoses and treatment also display correlations with patient conditions and responses. In contrast, data generated in more mundane contexts, such as academic conferences or business meetings, show significantly less deviation from chance than expected theoretically or displayed in equipment calibrations. Thus, the FieldREG strategy holds high promise for further understanding of consciousness-related information fields and their beneficial utilization in a broad range of human endeavors. Extension of this work is planned.

19/04 - "Parapsychological Investigations: Reflections, Adventures, and Cautionary Tales"

Instituição/*Institution:* University of Maryland Baltimore County, Maryland - USA

Duração prevista/*Estimated duration:* 2005/02 - 2006/03

Investigador/*Researcher:* Prof. Stephen E. Braude

Abstract: Part 1 of the project is a series of cautionary tales based on my own case studies into spontaneously occurring macro PK. These investigations, both successful and unsuccessful, reveal obstacles and pitfalls of which every parapsychological field investigator must be aware and must take steps to avoid. The obstacles also shed light on the elusive nature and context-sensitivity of psi phenomena, features which any theory of psi must also take into account. They also demonstrate the importance of penetrating the psychodynamics of psi and understanding why phenomena take sometimes peculiar and often idiosyncratic forms. I focus on four cases: (1) the Agold leaf lady@ from Florida, whose body would break out spontaneously and at close range in a golden material that turned out to be brass, (2) the alleged PK superstar, Joe Nuzum, whom I caught cheating and whose attempts to circumvent controls are instructive, (3) Dennis Lee, a promising PK subject who was so damaged psychologically by his presumed sponsors that his ostensible abilities could not adequately be assessed, and (4) KR, a veteran policeman (and presumably credible subject) who sincerely (but incorrectly) believed he was witnessing extraordinary psychic happenings.

Part 2 of the project is a critique of the much abused and very confused concept of synchronicity (acausal meaningful coincidence). Contrary to what many claim, I show that synchronicity is a fundamentally causal concept, and that those who claim otherwise base their claim on a needlessly restrictive and quaint concept of causation. The result of that error is the promulgation of considerable confusion in the parapsychological and psychological literature. I also show that it=s incorrectCin fact, incoherentCto claim (as many do) that synchronicity is a principle in nature that organizes events in virtue of their intrinsic meaning. I show that events are meaningful only in relation to some agent(s) who bring that clustering about. The agent(s) in question must either be God (or some other entity outside the normal sphere of events) or else ordinary human beings. But then believers in genuinely nonfortuitous meaningful coincidences face an interesting dilemma. On the one hand they can posit what many would reasonably consider an implausibly anthropomorphic God (or supra-human agent). And on the

other hand, they must posit a refined, extensive, and very intimidating form of large-scale psychokinesis.

2**4/04 - "A Parapsychological Investigation of the I Ching: The Relationship Between Psi, Intuition, and Time Perception"**
Instituição/*Institution:* Anomalistic Psychology Research Unit, University of Adelaide - Australia
Duração prevista/*Estimated duration:* 2005/04 - 2006/03
Investigador/*Researcher:* Dr. Lance Storm

Abstract: Statistical evidence was sought that an anomalous effect might be involved in the ancient Chinese system of divination known as the I Ching. The I Ching user throws three coins, six times, to generate one of 64 possible six-line symbols or hexagrams, and then consults the associated divinatory reading. Previous studies have given some indication that first-hexagram outcomes can be determined in advance of generating the hexagram to a significant degree above MCE (Thalbourne & Storm, submitted). However, participants might not only target first-hexagrams (of which the associated reading is present-focused), but they may also second hexagrams (the associated reading of which is future-focused—second hexagrams are generated from the first hexagram). It is theorised that hexagram targeting may accord with the participant's time perspective. A present time perspective (PTP) refers to immediate events, whereas a future time perspective (FTP) refers to fate and what it has in store. PTP and FTP types are determined from scores on the Time Perspective Inventory (Zimbardo & Boyd, 1999). Since intuitive types are said to be future-oriented (i.e., typically looking for the possibilities or future state of things), it is hypothesized that (i) there are relationships between time-perspective and Intuiting using the Singer-Loomis Type Deployment Inventory (Singer & Loomis, 1996). It is also hypothesised that (ii) hexagram hit-rates are above MCE, (iii) time perspective determines a participant's influence on hexagram outcomes (i.e., PTP types hit more often on first-hexagrams than FTP types who hit more often on second hexagrams), and (iv) intuition predicts hexagram outcomes. There were significant relationships between PTP scores and (a) extraverted intuiting (EN), and (b)

introverted intuiting (IN). There was a significant relationship between FTP scores and IN. Hit-rates were significantly (or marginally significantly) above chance on the I Ching measures—first-hexagram hit-rate (N = 180), P = .26, where PMCE = .25 (p = .067); second-hexagram hit-rate (first-hexagram hitters only: n = 38), P = .34, where PMCE = .238 (p = .048); and second-hexagram hit-rate (first-hexagram missers only: n = 100), P = .29, where PMCE = .254 (p = .063). PTP types did hit more often on first-hexagrams (27.2%) compared to FTP types (22.7%), whereas FTP types did hit more often on second-hexagrams (32.7%) compared to PTP types (29.2%)—the differences were not significant. Neither IN nor EN correlated significantly with hexagram outcomes.

28/04 - "Paranormal Effects Using Sighted and Vision-Impaired Participants in a Quasi-Ganzfeld Task: A Replication Study"

Instituição/*Institution:* Anomalistic Psychology Research Unit, University of Adelaide - Australia
Duração/*Duration:* 2005/02 - 2005/08
Investigadores/*Researchers:* Dr. Lance Storm, Dr. Mikele Barrett-Woodbridge

Abstract: A replication study of an earlier study by Storm and Thalbourne (2001; N = 84) was conducted to test the hypothesis that totally blind people compensate for their vision-impairment by developing superior psi ability compared to sighted people. Participants were required to describe a concealed line drawing, and then rank four pictures (1 target plus 3 decoys) from 'most likely' (rank #1) to be the target picture in the envelope to 'least likely' (rank #4). The concealed picture was removed from its envelope and assigned its corresponding rank number. Previously, Storm and Thalbourne (2001) found an above-chance success-rate of 28% (where MCE = 25%) for the totally blind (n = 18), which was superior (not significantly) to the hit-rate of 26% for the rest of the sample (i.e., sighted and partially blind participants combined; n = 66). In the replication study (N = 76), the same procedure was followed, but only totally blind and sighted participants were used. The totally blind group and the sighted group both scored

at the same below-chance hit-rate of 21% (p = .45, z = -0.51, p = .695). There was no evidence that psi compensates for blindness. When the dataset from the present study was combined with Storm and Thalbourne's (2001) dataset (total N = 160), the totally blind group (n = 56) and the sighted group (n = 80) both scored below chance, P = 23% (p = .47, z = -0.38, p = .648), and the sighted/partially-sighted group combined (n = 104) also scored below chance, P = 20% hit-rate (p = .43, z = -1.17, p = .879). Again, psi compensation was not found in the blind group. It was concluded that if there is compensation for blindness, it might work in ways other than paranormal. It is also possible that blind people may prefer targets that are not of a visual nature.

74/04 - "High performance REG array with simultaneous Read-Out - Exploration of a new REG design, involving self-selective amplification and EEG triggered read-out for PK studies"
Instituição/*Institution:* Institute for Environmental Medicine and Hospital Epidemiology, University Hospital Freiburg - Germany
Duração prevista/*Estimated duration:* 2005/01 - 2006/03
Investigadores/*Researchers:* Prof. Harald Walach, Dr. Tilmann Faul, Dr. Holger Bösch, Dr. Matthias Braeunig

Abstract: ***Objectives:*** Construction of a physical random event generator (REG) with fast parallel processing based on FPGA technology, achieving local coupling with the environment through an explicit trigger-feedback loop. To deliberately influence the outcome of random bit generation by means of internal and external trigger signals. while still maintaining the null-hypothesis. We will argue in favor for systemic closure as the key to unify the views of influence and data selection theories.

Methods: Electronic transistor noise is amplified and digitized, oscillating randomly between two states, a frequency divider assures exact 50% unbiased duty cycle at a mean frequency about 1.3 MHz. Comparison of three triggering modes: Internal triggers drawn from the output bit sequence itself, external EEG modulated triggers, and fixed

frequency triggers. Sampling is realized in 4-5 channels at a mean frequency about 1 kHz for trials of 200 bit samples drawn in parallel from the stream, hits and transitions are counted. Acoustic feedback is given by ascending and descending tones based on the cumulative deviation score, while a participant is asked to keep an intention of increasing, or lowering the tones.

Results: 15 human subjects participated in experiments each comprising 13 sets of combinations of trigger modes and intention conditions. Each set generated 1.2 MBits of data in 4 channels. Autocorrelation is less than 1%. Control runs satisfy the normality condition imposed to maintain the null-hypothesis. Larger deviations were observed in sets where participants were allowed a new method of partially inverting the meaning of hits and misses (M-switch), indicating a pre-cognitive factor present.

Conclusion: It seems that significant hit rates can be achieved by operationally closing a system made of an entropy source and a conscious observer, suggesting that the triggered REG is more sensitive to local interaction with the environment than more conventional setups. The main ingredient is an explicit trigger-feedback loop that involves a participant who attributes meaning to a sequence of events.
Keywords: Triggered REG, meaning, bio-feedback, operational closure, parapsychology

110/04 - "Reports of distressing anomalous experiences to UK parapsychology units: a survey, analysis and the creation of a practical response tool"
Instituição/*Institution:* Koestler Parapsychology Unit, Department of Psychology, Edinburgh - UK
Duração/*Duration:* 2005/03 - 2005/12
Investigadores/*Researchers:* Dra. Cláudia Coelho, Dr. Peter Lamont

Abstract: This project was concerned with contacts from individuals to UK academic parapsychology or anomalous experiences research units reporting distressing anomalous experiences. It focused on contacts (via

telephone, letter or e-mail) by individuals who were a) distressed or confused by their experiences, and b) seeking from these units information, help or verification of the paranormal nature of their experiences. This study was encouraged by 20 years of experience at the KPU with contacts of this kind, and motivated by: a) recent research on the effect of early identification and delay in the treatment of psychosis; and b) the strong possibility that some of the distressed individuals who contact these units before or instead of seeking medical advice about their distress are premorbid for psychotic illness. The aims of this project were to: 1) assess the extent of records of such contacts to UK units; 2) assess what information was available in these records; 3) estimate what proportion of such contacts to UK units the existing records represent; and 4) research current procedures for dealing with such contacts in these units. Two studies were developed: a survey of records of distressed contacts to participating units, and an interview study with members of staff at each of these units. Data resulting from each study was treated, respectively, by quantitative content analysis and by thematic analysis. Outcomes from both studies suggest that: 1) when academic units declare interest in anomalous experiences or parapsychology this is likely to attract a small number of distressed individuals who are prodromal for or in first episode psychosis, and who have not contacted health or mental health advisors; 2) while there are benefits to units from media exposure, this exposure may entail some responsibilities; 3) these may be addressed by establishing a procedure in units which ensures that distressed individuals are dealt with in a structured, informed and efficient way, which should probably involve clinical advisors; 4) there are socially/culturally patterned ways of talking about unusual experiences, displaying distress and making requests, which should be considered for this purpose; and 5) the benefits of such a procedure to the affected individuals, their families and communities, and to the National Health Service are likely to be significant.

134/04 - "Investigating the multidimensional nature of body image, sensorial representation, and phenomenology in relation to different forms of out-of-body experience"

Instituição/*Institution:* Manchester University - UK
Duração prevista/*Estimaded duration:* 2005/06 - 2006/03
Investigadores/*Researchers:* Dr. Craig Murray, Dr. Jezz Fox

Abstract: Many people report having had an 'out-of-body' experience (OBE) in which they felt as if their phenomenal self was separated from their physical body. Previous work has found OBE experients (OBErs) to score higher on measures of dissociation and to differ in regards to the perceptual experience of their body than non-experients (non-OBErs). These findings have been interpreted as supporting a dissociational theory of the OBE. More recent work (project 124/02) has suggested that an examination of other dimensions of body experience might reveal further aspects of such dissociational experience. In this work OBErs have been found to score higher on a measure of body dissatisfaction, and lower on a measure of confidence in their physical self-presentation than non-OBErs. However, this prior research did not distinguish between those who had had a spontaneous OBE or an OBE as part of a near-death experience (NDE).

In a preliminary study we recruited participants via email advertisements to on-line discussion groups dedicated to topics of either near-death experiences or out-of-body experiences. A total of 39 participants who reported either a spontaneous OBE (n=28) or OBE as part of a near-death experience took part (we also received a small number of responses from people who had an OBE when under the influence of drugs or alcohol, but given the small number these were not included in the analysis). It was hypothesised that the causes of the spontaneous OBE and the OBE which takes place as part of an NDE have different causal psychological mechanisms. It was predicted that people reporting a prior spontaneous OBE would score more negatively on a variety of dimensions of body image, such as body dissatisfaction, than people reporting an OBE as part as a Near-death experience. Not all of the hypotheses were supported, but spontaneous OBErs (n=28) were found to score significantly higher on measures of body dissatisfaction and self-consciousness than NDErs (n=11).

In our main project study we have gathered nearly 2000 completed web questionnaires to examine further differences between OBErs and non-OBErs. This includes a comparison of OBEs experienced spontaneously, as part of and NDE, and as part of a meditatively, drug or alcohol induced experience. Analysis of this data is underway and will be reported when available.

163/04 - "Effects of different Biofeedback training procedures on quantitative Electroencephalographic parameters of healthy subjects"

Instituição/*Institution:* Brain Resource Company B.V., Nijmegen - The Netherlands

Duração prevista/*Estimaded duration:* 2005/01 - 2006/03

Investigadores/*Researchers:* Dr. Martijn Arns, Dr. Wytze van der Zwaag, Dr. Erica Heesen, Dr. Rien Breteler

Abstract: ***Objectives:*** Both GSR and SCP have been associated with epilepsy. These measures may be different expressions of a single underlying neurophysiological (arousal) system. Learning curves of volitional control of GSR and SCP after feedback will be investigated, and compared to changes in simultaneously measured spontaneous SCP and GSR. Effects of training on QEEG profiles and neuropsychological functioning will be assessed.

Methods: Eight men and 11 women were randomly assigned to either a GSR biofeedback condition or an SCP neurofeedback condition. GSR and SCP were measured in both conditions. Each subject participated in 24 sessions of four runs, consisting of 40 trials each, in which pseudo-randomly increases and decreases in SCP or GSR were trained.

Results: For the GSR feedback a polynomial curve was found to fit the data best. An increase in arousal (increase in GSR) appeared to be learned slightly better than a decrease. Four out of 10 subjects showed evidence of discrete self-regulation of their GSR.

For the SCP feedback, preliminary analyses suggest a flat linear learning curve (regression coefficient r=0.033). Again, an increase in arousal (cortical negativity) appeared to be learned slightly better than a

decrease. Three out of 9 subjects showed evidence of discrete self-regulation of their SCP´s.

Conclusions: Interrelationships of GSR and SCP grand averages over sessions and changes from pre to post QEEG and neuropsychology will be discussed.
The polynomial curve for the GSR group suggests GSR was easier to learn, further supported by the individual results. Future studies should focus on these effects in epilepsy patients.

PALESTRANTES E MODERADORES
SPEAKERS AND CHAIRMEN

ALCINO SILVA Professor de Neurobiologia, Departamento de Neurobiologia, Psiquiatria e Psicologia, Universidade da Califórnia, Los Angeles, EUA. Presidente Fundador da Sociedade de Cognição Molecular e Celular. Membro do corpo editorial de várias revistas científicas e consultor de vários organismos oficiais na área da Neurologia. Interesses científicos: memória e circuitos neuronais, memória e condicionantes genéticos.

Professor of Neurobiology, Department of Neurobiology, Psychiatry and Psychology, University of California, Los Angeles, USA. Founding President of the Molecular and Cellular Cognition Society. Member of the editorial board of several scientific journals and advisor for several official organizations in the area of Neurology. Research interests: memory and neuronal circuits, memory and genetic restrictions.

ALEXANDRE CASTRO-CALDAS Professor de Neurologia, Director do Instituto de Ciências da Saúde da Universidade Católica Portuguesa, Lisboa. Foi Presidente da *International Neuropsychological Society*. Interesses científicos: literacia/iliteracia e ortografia e substratos neurobiológicos cerebrais, afasia e doença de Parkinson.

Professor of Neurology, Director of the Health Sciences Institute of the Catholic University of Portugal, Lisbon, Portugal. Past President of the International Neuropsychological Society. Research interests: literacy/illiteracy and orthography and brain neurobiological substrates, aphasia and Parkinson's disease.

CAROLINE WATT Investigadora Sénior, Departamento de Psicologia, Universidade de Edimburgo, Escócia. *Past President da Parapsy-chological Association* e autora de artigos em livros e revistas científicas na área da parapsicologia e de crenças paranormais. Interesses cientí-ficos: efeitos da expectativa do experimentador e do participante, aspectos psicológicos de experiências e crenças paranormais evidentes e intensificação do desempenho remoto em tarefas cognitivas e comportamentais.

Senior Lecturer, Psychology Department, University of Edinburgh, Scotland. Past President, Parapsychological Association, and author of book and journal articles on parapsychology and paranormal beliefs. Research interests: experimenter and participant expectancy effects, psy-chological aspects of ostensible paranormal experiences and beliefs and remote performance enhancement with cognitive and behavioural tasks.

CHRISTOPHER FRENCH Professor de Psicologia Anomalística, *Goldsmiths College*, Universidade de Londres, Inglaterra. Co-editor da revista "The Skeptic". Interesses científicos: psicologia do paranormal, crenças relacionadas, experiências ostensivamente paranormais, processamento de informação e cognição, psicologia da consciência, neuropsicologia humana, distinção entre ciência e pseudociência, aspectos psicológicos e parapsicológicos de alegados abduzidos por "aliens".

Professor of Anomalistic Psychology, Goldsmiths College, University of London, England. Co-editor of the magazine "The Skeptic". Research interests: psychology of the paranormal, related beliefs, ostensible paranormal experiences, information processing and cognition, the psychology of consciousness, human neuropsychology, distinction between science and pseudoscience, psychological and parapsychological aspects of alleged "alien" abductions.

FERNANDO LOPES DA SILVA Professor Jubilado de Fisiologia Geral, Amesterdão, Holanda e Professor Convidado de Medicina, Lisboa. Interesses científicos: electrofisiologia do cérebro, origens do fenómeno epiléptico, redes neuronais em relação com a memória, atenção e consciência.

Emeritus Professor of General Physiology, Amsterdam, Holland and Invited Professor of Medicine, Lisbon, Portugal. Research interests: electrophysiology of the brain, origin of epileptic phenomena, neuronal networks in relation to memory, attention and consciousness.

GUILLÉN FERNÁNDEZ Investigador Principal e Director do Laboratório de Neurologia Cognitiva e Investigação da Memória, F.C. *Donders Center for Cognitive Neuroimaging, Radboud University*, Nijmegen, Holanda. Professor Visitante, *Swammerdam Institute for Life Sciences*, Universidade de Amesterdão. Neurologista. Interesses científicos: formação da memória humana e hipocampo, relações entre a memória e o córtex nasal e o núcleo caudado em estratégias de reconhecimento.

Principal Investigator and Head of the Cognitive Neurology and Memory Research Laboratory, F.C. Donders Center for Cognitive Neuroimaging, Radboud University, Nijmegen, the Netherlands. Visiting Professor, Swammerdam Institute for Life Sciences, University of Amsterdam. Neurologist. Research interests: human memory formation and the hippocampus, relations between memory and the nasal cortex and the caudate nucleus during route recognition.

HOWARD EICHENBAUM Professor de Psicologia e Director do *Center for Memory and Brain*, Departamento de Psicologia, Universidade de Boston, Boston, EUA. Editor de revistas científicas na área do Hipocampo e da Neurologia do Comportamento. Autor de livros sobre música e cognição. Interesses científicos: técnicas de captação de sinais através de telemetria e de sistemas de microeléctrodos multicanais, processamento superior de informação olfactiva, plasticidade e representação da memória no hipocampo e córtex cerebral.

Professor of Psychology and Director of the Center for Memory and Brain, Department of Psychology, Boston University, Boston, USA. Editor of scientific journals in the area of the Hippocampus and the Neurology of Behavior. Author of books on music and cognition. Research interests: signal capture techniques using telemetry and multichannel microelectrode arrays, higher order olfactory information processing, plasticity and memory representation in the hippocampus and cerebral cortex.

JOHN AGGLETON Professor de Neurociência Cognitiva, Faculdade de Psicologia, Universidade de Wales, Cardiff, Reino Unido. Membro do conselho editorial de várias revistas científicas na área das Neurociências. Presidente da *European Brain and Behaviour Society*. Autor de livros sobre a Amígdala e seus aspectos neurobiológicos. Interesses científicos: doença de Alzheimer e córtex cingulado, memória espacial, substratos neuronais da memória, em especial o Hipocampo.

Professor of Cognitive Neuroscience, School of Psychology, University of Wales, Cardiff, UK. Member of the editorial board of several scientific journals in the area of Neuroscience. President of the European Brain and Behaviour Society. Author of books on the amygdala and its neurobiological aspects. Research interests: Alzheimer's disease and the cingulated cortex, spatial memory, neural substrates of memory, especially the hippocampus.

JOHN PALMER Investigador Associado Visitante, Clínica de Neurologia, Hospital Universitário de Zurique, Suíça. Doutorado em Psicologia da Investigação. Editor do *Journal of Parapsychology* e consultor de várias revistas científicas na área da Parapsicologia. Interesses científicos: factores psicológicos associados com experiências anómalas, desenvolvimento e uso de testes para medir capacidades extrasensoriais ostensivas, desenho de investigação experimental em Parapsicologia, correlatos entre disfunções dos lobos temporais e experiências paranormais.

Visiting Research Associate, Neurology Clinic, University Hospital Zurich, Switzerland. PhD. in Research Psychology. Editor of the Journal of Parapsychology and consultant for several scientific journals in Parapsychology. Research interests: psychological factors associated with anomalous experiences, development and use of controlled tests to measure ostensible extrasensory abilities, design of experimental research in Parapsychology, correlates in temporal lobe dysfunction and paranormal experiences.

LIA KVAVILASHVILI Professora Associada de Psicologia Cognitiva, Universidade de Hertfordshire, Inglaterra. Consultora de várias revistas científicas na área da memória e psicologia cognitiva de idosos. Interesses científicos: memórias autobiográficas autoinduzidas e espontâneas, memórias prospectivas em crianças e idosos, supressão de pensamento na esquizofrenia, efeito placebo no desempenho de memória, evocação involuntária (mind-popping e flashbulb), respectivamente de certos itens (*p.ex. palavra, canção*) ou de acontecimentos complexos (p.ex. 11 de Setembro).

Reader in Cognitive Psychology, University of Hertfordshire, England. Reviewer for several scientific journals in the area of memory and cognitive psychology of the elderly. Research interests: autoinduced and involuntary autobiographical memories, prospective memory in children and elderly, thinking suppression in schizophrenia, the placebo effect on memory performance, involuntary conscious occurrences (mind-popping and flashbulb), namely of certain items (e.g. word, tune) or of complex events (e.g. September 11).

MÁRIO SIMÕES Professor de Psiquiatria, Director de Cursos de Pós--Graduação em Hipnose Clínica e (Psico)Terapia pela Reestruturação Vivencial e Cognitiva (sob estado modificado de consciência), Lisboa. Interesses científicos: psicologia e fisiologia dos estados modificados de consciência, etnomedicina e experiências excepcionais humanas.

Professor of Psychiatry, Director of Post-Graduation Courses in Clinical Hypnosis and (Psycho)Therapy by Cognitive and Experiential Restructuralization (under altered states of consciousness), Lisbon, Portugal. Research interests: psychology and physiology of altered states of consciousness, ethnomedicine and human exceptional experiences.

MENNO WITTER Professor de Anatomia e Embriologia, Departamento de Anatomia, *VU University Medical Center*, Amesterdão, Holanda. Professor Visitante de Neuroanatomia, *Center for the Biology of Memory*, NTNU, Trondheim, Noruega. Director Científico do *Research Institute Neuroscience Vrije Universiteit*, Amsterdão. Interesses científicos: neurobiologia da aprendizagem e memória em indivíduos saudáveis (sistema cortico-hipocampo e suas relações com o tálamo e córtex pré-frontal) e estudos comparativos com doentes de Alzheimer e Epilepsia.

Professor of Anatomy and Embryology, Department of Anatomy, VU University Medical Center, Amsterdam, the Netherlands. Visiting Professor of Neuroanatomy, Center for the Biology of Memory, NTNU, Trondheim, Norway. Scientific Director of the Vrije Universiteit Neuroscience Research Institute, Amsterdam. Research interests: neurobiology of learning and memory in the healthy (cortico-hippocampal system and its relationships with the thalamus and prefrontal cortex) and comparative studies in the sick with Alzheimer's and Epilepsy.

MITCHELL EISEN Professor Associado de Psicologia, *California State University*, Los Angeles, EUA. Director do Programa de Psicologia Forense. Consultor do Ministério Público, do Estado da Califórnia, para assuntos relacionados com a memória de testemunhas oculares, pós-trauma e efeitos de abusos em crianças. Interesses científicos: memória e sugestibilidade, aspectos forenses de relatos de entrevistas a crianças abusadas física e sexualmente, memória de testemunhas oculares, trauma, dissociação, sugestibilidade e memória de crianças abusadas.

Associate Professor of Psychology, California State University, Los Angeles, USA. Director of the Forensic Psychology Program. Consultant for the Prosecuting Counsel of the State of California on matters related to eyewitness memory, post-trauma and the effects of abuse on children. Research interests: memory and suggestibility, forensic aspects of reports from interviews to children physically and sexually abused, eyewitness memory, trauma, dissociation, suggestibility and memory of maltreated children.

NUNO GRANDE Professor Jubilado de Anatomia Normal do Instituto de Ciências Biomédicas de Abel Salazar da Universidade do Porto. Interesses científicos: anatomofisiologia pulmonar e cardiocirculatória, mecanismos de clearance respiratória, doença dos metais pesados, fibrose pulmonar, doença vibroacústica, linfogénese.

Emeritus Professor of Normal Anatomy of the Abel Salazar Institute for the Biomedical Sciences, University of Oporto, Portugal. Research interests: pulmonary and cardiocirculatory anatomophysiology, airway clearance mechanisms, heavy metal-related diseases, pulmonary fibrosis, vibroacoustic disease, lymphogenesis.

PATRICK CHAUVEL Professor de Fisiologia, Director do Laboratório de Neurofisiologia e Neuropsicologia, Faculdade de Medicina de Marselha II, França. Neurologista. Presidente do Conselho Científico da Sociedade de Neurofisiologia Clínica de Língua Francesa. Interesses científicos: cirurgia da epilepsia, epilepsia e cognição, linguagem e processamento da memória e funções superiores cognitivas, fenómeno "déjà vu".

Professor of Physiology, Director of the Laboratory of Neurophysiology and Neuropsychology, Faculty of Medicine Marseille II, France. Neurologist. President of the Scientific Board of the Society of Clinical Neurophysiology of French Language. Research interests: epilepsy surgery, epilepsy and cognition, language and memory processing and higher cognitive functions, "déjà vu" phenomena.

REX STANFORD Professor de Psicologia, St. John's University, New York, EUA, e autor de mais de 100 publicações. Actualmente Presidente da *Parapsychological Association* (PA). Recebeu o *PA's Outstanding Contribution Award*. Interesses científicos: trabalho teórico-prático num modelo técnico para um desempenho não intencional, mas servindo as necessidades, de uma resposta instrumental mediada por "Psi" (modelo PMIR), estratégias de investigação para aumentar o desenvolvimento conceptual e a replicação em parapsicologia.

Professor of Psychology, St. John's University, New York, USA, and author of over 100 publications. Currently President Elect of the Parapsychological Association (PA) and recipient of the PA's Outstanding Contribution Award. Research interests: theoretical and experimental work on an experimentally testable model of nonintentional, but need-subserving, psi-mediated response (PMIR model), research strategies for enhancing conceptual development and replicability in parapsychology.

RICHARD BROUGHTON Investigador Sénior de Psicologia, Universidade de Northampton, Inglaterra. Autor de um livro e artigos sobre experiências anómalas e parapsicologia. Interesses científicos: o papel das respostas emocionais na tomada de decisão intuitiva e a aplicação prática da intuição no trabalho, na medicina e no cumprimento da lei. Os sistemas fisiológicos subjacentes às experiências anómalas.

Senior Lecturer in Psychology, University of Northampton, Northampton, UK. Author of a book and articles on anomalous experiences and parapsychology. Research interests: the role of emotional responses in intuitive decision making and the practical application of intuition in business, medicine and law-enforcement. Physiological systems underlying anomalous experiences.

RUI MOTA CARDOSO Professor de Psicologia, Faculdade de Medicina, e Psiquiatra, Porto. Fundador da Sociedade Portuguesa de Psicossomática e da Sociedade Portuguesa de Psicodrama. Interesses científicos: relação terapêutica e medicina psicossomática.

Professor of Psychology, Faculty of Medicine, and Psychiatrist, Oporto, Portugal. Founder of the Portuguese Society of Psychosomatics and Portuguese Society of Psychodrama. Research interests: the therapeutic relationship and psychosomatic medicine.

STEPHEN BRAUDE Professor de Filosofia, Director do Departamento de Filosofia, Universidade de *Maryland, Baltimore County*, EUA. Autor de vários livros na área da Parapsicologia, Filosofia da Mente e Sobrevida após a Morte. Consultor científico de várias revistas na área da Filosofia, Parapsicologia, Dissociação e de várias editoras universitárias. Interesses científicos: dissociação, experiências fora do corpo, personalidades múltiplas, sobrevida após a morte e psicoquinese.

Professor of Philosophy, Chair of the Department of Philosophy, University of Maryland, Baltimore County, USA. Author of several books in the area of Parapsychology, Philosophy of Mind and Life After Death. Referee for several scientific journals in the area of Philosophy, Parapsychology, Dissociation and for several university presses. Research interests: dissociation, out-of-body experiences, multiple personalities, life after death and psychokinesis.

FUNDAÇÃO
Bial

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6º Simpósio da Fundação Bial

A Fundação Bial é uma instituição sem fins lucrativos, considerada de utilidade pública pelo Governo português, que tem como missão incentivar o conhecimento científico do Homem, tanto do ponto de vista físico como espiritual.

Constituída em 1994 pelos Laboratórios Bial e pelo Conselho de Reitores das Universidades Portuguesas, tem os altos patrocínios do Senhor Presidente da República e da Ordem dos Médicos.

A actividade da Fundação Bial desenvolve-se através da atribuição do Prémio Bial, um dos maiores galardões na área da saúde em toda a Europa, e do lançamento de Bolsas de Investigação Científica na área das Neurociências.

Bianualmente, a Fundação Bial organiza os simpósios Aquém e Além do Cérebro, um espaço de diálogo que reúne alguns dos mais prestigiados especialistas mundiais nas áreas da Psicofisiologia e da Parapsicologia e os seus bolseiros.

Nestes encontros, através da exposição de posters e das sessões de comunicações orais de um conjunto de projectos, a Fundação Bial apresenta à comunidade científica os resultados - preliminares nalguns casos, definitivos noutros - das investigações dos seus bolseiros.

O livro de actas que agora se publica é uma compilação dos textos das palestras apresentadas durante o 6º Simpósio da Fundação Bial organizado em torno do tema geral “Memória”.