

## **THE MRC APPLIED PSYCHOLOGY UNIT**

The transcript of a Witness Seminar held at the Wellcome Trust Centre for the History of Medicine at UCL, London, on 12 June 2001

**Edited by L A Reynolds and E M Tansey**

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#### Key

##### **Front cover photographs, top to bottom:**

Professor Richard Gregory (1923–2010, chair)  
Professor Robert Logie, Professor John Groeger  
Professor Alan Baddeley, Dr Megan Davies  
Professor William Marslen-Wilson, Professor Patrick Rabbit

##### **Inside front cover photographs, top to bottom:**

Mr David Duncan, Professor Richard Gregory (1923–2010, chair)  
Professor Patrick Rabbitt, Dr Phil Bamard  
Dr Derek Bangham (1924–2008), Dr Ivan Brown, Professor Graham Hitch  
Dr Geoff Bunn, Professor Sergio Della Sala

##### **Back cover photographs, top to bottom:**

Mr David Duncan, Sir Christopher Booth  
Dr David Tyrrell (1925–2005), Mr Harald Leyrer  
Dr Brandon Lush, Professor Graham Richards  
Professor John West, Dr Hal Cook, Sir Christopher Booth

##### **Inside back cover photographs, top to bottom:**

Dr David Tyrrell (1925–2005) with Mr Stephen White (1949–2010)  
and Ms Alice Nichols  
Dr Geoff Bunn, Professor Richard Gregory (1923–2010, chair)  
Professor Graham Hitch  
Professor John West, Dr David Tyrrell (1925–2005)

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# INTRODUCTION

The Wellcome Trust Centre for the History of Medicine at UCL hosted a Witness Seminar on the Applied Psychology Unit (APU) at Cambridge on 12 June 2001. The result of the discussions held that day, which follows, provides a fascinating insight into the origins and consequences of the unit's work. For over half a century the APU has been a dominant influence on the nature and direction of British psychology. As one participant put it, the APU was the crucible for 'an enormously productive ferment of ideas' (p. 24).

## What was its secret?

For a start, no aspect of human action was considered to be either too profound or too innocuous to escape scientific scrutiny by the unit's psychologists. Experimental subjects endured great discomfort [Figures 6, 10, 12, 13] in the pursuit of knowledge of human behaviour, often at the request of the armed services, a government department or an industrial firm. Some subjects were deprived of sleep and social contact, others endured high temperatures, unusual atmospheric pressures or loud noises. Experiments were set at the bottom of the sea, the tops of mountains, or more frequently, in a suburban house on Chaucer Road, a quiet corner of Cambridge and the home of the APU since 1952 [Figure 8, p. 12].

In the mid-1960s the study of extreme environments quietly mutated into the study of extreme neurological conditions. Naval ratings as subjects were replaced by housewives, and in place of jet-fighter pilots were vigilant drivers. Telephone operators, deep-sea divers and air traffic controllers were studied with the same patient enthusiasm as right-justified typefaces, Post Codes and nonsense syllables.

Adept at the reconciliation of opposites, the APU was also a site where extremes were carefully balanced. An essential tension came from 'trying to be relevant to current problems ...and trying to generate good theory' (p. 26). Another was between the producers and consumers of the data, where commercial or military sensitivity sometimes intruded to prevent, or, at least to delay the publication of APU work. The Medical Research Council requirement of publication for grant-aided research was occasionally used as a lever, although some clients 'didn't want an answer about a particular product, ... they weren't actually buying a result, they were trying to make a difference to the way in which their companies functioned' (p. 71).

One participant noted 'how similar the unit was to a library' (p. 73). It was certainly a studious place. Yet the APU was also filled with bright and ambitious scholars and clinicians from across the world. They religiously attended coffee and tea breaks to discuss their common interests; seminar debates could quickly become heated.

The unit also fostered change in their visitors: ‘I arrived in the early 1980s at the unit and I learned through questioning, “What does this mean? What is it about?”, to question the symptoms in some more depth. I learned to use the cognitive approach, the theories, to understand the symptoms a bit more, to the point that I thought eventually it was a better approach and I swapped from medical school to psychology.’ (p. 50)

## Influences

A succession of charismatic and inspirational leaders cultivated a research culture that relentlessly explored apparently unpromising territory. For nearly a decade Sir Frederic Bartlett [Figure 1, p. 4] was a strong influence in the direction of the APU, from its creation in 1944 to the choice of Kenneth Craik as the first head [Figure 7, p. 12]. Bartlett became the second director following Craik’s tragic death a year later. Dr Norman Macworth [Figure 2, p. 4] ensured the unit’s future location with the unauthorized purchase of Chaucer Road (note 7, p. 5). The two longest-serving directors from 1958 to 1997, Donald Broadbent and Alan Baddeley [front cover], took the unit from its narrow industrial preoccupations to the recent applications in acoustics, ageing, deep dyslexia, depression, emotions, form design, mental lexicon and semantics, among many others.<sup>1</sup> William Marslen-Wilson [front cover] was brought in to redevelop the unit’s scientific programme, and to concentrate research around the functions of attention, memory, and emotion in the brain (p. 51–55).

Of course, financial support was not solely derived from the MRC, and funding was unpredictable: ‘if you didn’t have an attractive acronym you didn’t get the research funding’ (p. 36). The APU has been fortunate indeed to enjoy the benevolence and cooperation of organizations such as British Telecom, IBM and Xerox. APU authors continue to be efficient (p. 26) and prolific (3714 papers before its name changed in 1998). There have been other views on their expenditure of public money, of course: ‘What on earth is the MRC doing spending its money on toy trains?’ asked the *Cambridge Evening News* in the late 1960s. The train in question might have looked like ‘the carcass of an ancient baby Austin at the bottom of the garden’ (p. 72) but it was, in fact, a working model of missile control. One particular feature that is rarely reported in academic journals or annual progress reports came up time and again: working at the APU was enormous fun.

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<sup>1</sup> Including visual perception and modern art (p. 69). Professor Richard Gregory wrote: ‘The business about jazzing in pop art was recently discussed by Professor Semir Zeki, a neurophysiologist at University College London, and myself. [Gregory R L. (1995) Brain-created visual motion: an illusion? *Proceedings of the Royal Society London B* 260: 167–168.]’ E-mail to Mrs Lois Reynolds, 27 February 2003. Some of the subjects mentioned in the transcript can be found in Gregory R L. (ed.) (1987) *The Oxford Companion to The Mind*. Oxford, New York: Oxford University Press.



In the following pages, then, can be found a manual of how a world class research unit of applied psychology was created. The unit will certainly prove troublesome for any future historian of psychology seeking to encapsulate its influence in a neat thesis. Nevertheless, some important questions remain. Our participants were generous during the Witness Seminar; it was a day of pleasant reminiscences. But tensions – if not downright hostilities – must have occasionally disrupted the calm at Chaucer Road. If this was the case, then what were the APU’s most contentious issues? What role has the director played, over the years, to balance the opposing forces of practical and theoretical work? How has the style of research changed, and what social, economic and political forces brought them about? We hope that the evidence presented at the meeting, and recorded here,<sup>2</sup> will go some way to helping future historians grapple with these larger issues.

*Geoff Bunn*

*Liverpool Hope University College*

*Lois Reynolds*

*Wellcome Trust Centre for the History of Medicine at UCL*

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<sup>2</sup> The correspondence during the editorial process will be deposited along with the records of the meeting in Archives and Manuscripts, Wellcome Library, London.



# WITNESS SEMINARS: MEETINGS AND PUBLICATIONS<sup>1</sup>

In 1990 the Wellcome Trust created a History of Twentieth Century Medicine Group, as part of the Academic Unit of the Wellcome Institute for the History of Medicine, to bring together clinicians, scientists, historians and others interested in contemporary medical history. Among a number of other initiatives the format of Witness Seminars, used by the Institute of Contemporary British History to address issues of recent political history, was adopted, to promote interaction between these different groups, to emphasize the potential of working jointly, and to encourage the creation and deposit of archival sources for present and future use. In June 1999 the Governors of the Wellcome Trust decided that it would be appropriate for the Academic Unit to enjoy a more formal academic affiliation and turned the Unit into the Wellcome Trust Centre for the History of Medicine at University College London from 1 October 2000. The Wellcome Trust continues to fund the Witness Seminar programme via its support for the Centre.

The Witness Seminar is a particularly specialized form of oral history, where several people associated with a particular set of circumstances or events are invited to come together to discuss, debate, and agree or disagree about their memories. To date, the History of Twentieth Century Medicine Group has held over 30 such meetings, most of which have been published, as listed on pages xiii–xvi.

Subjects for such meetings are usually proposed by, or through, members of the Programme Committee of the Group, and once an appropriate topic has been agreed, suitable participants are identified and invited. These inevitably lead to further contacts, and more suggestions of people to invite. As the organization of the meeting progresses, a flexible outline plan for the meeting is devised, usually with assistance from the meeting's chairman, and some participants are invited to 'set the ball rolling' on particular themes, by speaking for a short period of time to initiate and stimulate further discussion.

Each meeting is fully recorded, the tapes are transcribed and the unedited transcript is immediately sent to every participant. Each is asked to check their own contributions and to provide brief biographical details. The editors turn the transcript into readable text, and participants' minor corrections and comments are incorporated into that text, while biographical and bibliographical details are added as footnotes, as are more substantial comments and additional material provided by participants. The final scripts are then sent to every contributor, accompanied by forms assigning copyright to the Wellcome Trust.

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<sup>1</sup> The following text also appears in the 'Introduction' to recent volumes of *Wellcome Witnesses to Twentieth Century Medicine* published by the Wellcome Trust and the Wellcome Trust Centre for the History of Medicine at University College London.

Copies of all additional correspondence received during the editorial process are deposited with the records of each meeting in Archives and Manuscripts, Wellcome Library, London.

As with all our meetings, we hope that even if the precise details of some of the technical sections are not clear to the nonspecialist, the sense and significance of the events are understandable. Our aim is for the volumes that emerge from these meetings to inform those with a general interest in the history of modern medicine and medical science; to provide historians with new insights, fresh material for study, and further themes for research; and to emphasize to the participants that events of the recent past, of their own working lives, are of proper and necessary concern to historians.

### Members of the Programme Committee of the History of Twentieth Century Medicine Group

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The Group's activities are overseen by the Programme Committee, which includes professional historians of medicine, practising scientists and clinicians. The Programme Committee during 2002–03 comprised:

**Dr Tilli Tansey** – Historian of Modern Medical Science, Wellcome Trust Centre at UCL, and Chair

**Sir Christopher Booth** – Wellcome Trust Centre at UCL, former Director, Clinical Research Centre, Northwick Park Hospital, London

**Dr Robert Bud** – Head of Life and Environmental Sciences, Science Museum, London

**Dr Daphne Christie** – Senior Research Assistant, Wellcome Trust Centre at UCL and Organizing Secretary

**Professor Hal Cook** – Director, Wellcome Trust Centre at UCL

**Dr Mark Jackson** – Reader, Centre for Medical History, Exeter

**Professor Ian McDonald** – Harveian Librarian, Royal College of Physicians, London

**Dr Jon Turney** – Head of the Department of Science and Technology Studies, University College London

# HISTORY OF TWENTIETH CENTURY MEDICINE WITNESS SEMINARS, 1993–2003

- 1993      **Monoclonal antibodies**<sup>1</sup>  
Organizers: Dr E M Tansey and Dr Peter Catterall
- 1994      **The early history of renal transplantation**  
Organizer: Dr Stephen Lock
- Pneumoconiosis of coal workers**<sup>2</sup>  
Organizer: Dr E M Tansey
- 1995      **Self and non-self: a history of autoimmunity**<sup>1</sup>  
Organizers: Sir Christopher Booth and Dr E M Tansey
- Ashes to ashes: the history of smoking and health**<sup>3</sup>  
Organizers: Dr Stephen Lock and Dr E M Tansey
- Oral contraceptives**  
Organizers: Dr Lara Marks and Dr E M Tansey
- Endogenous opiates**<sup>1</sup>  
Organizer: Dr E M Tansey
- 1996      **Committee on Safety of Drugs**<sup>1</sup>  
Organizers: Dr Stephen Lock and Dr E M Tansey
- Making the body more transparent: the impact of nuclear  
            magnetic resonance and magnetic resonance imaging**<sup>4</sup>  
Organizer: Sir Christopher Booth
- 1997      **Research in General Practice**<sup>4</sup>  
Organizers: Dr Ian Tait and Dr E M Tansey
- Drugs in psychiatric practice**<sup>4</sup>  
Organizers: Dr David Healy and Dr E M Tansey
- The MRC Common Cold Unit**<sup>4</sup>  
Organizers: Dr David Tyrrell and Dr E M Tansey
- The first heart transplant in the UK**<sup>5</sup>  
Organizer: Professor Tom Treasure

- 1998     **Haemophilia: recent history of clinical management**<sup>6</sup>  
 Organizers: Professor Christine Lee and Dr E M Tansey
- Obstetric ultrasound: historical perspectives**<sup>7</sup>  
 Organizers: Dr Malcolm Nicolson, Mr John Fleming and Dr E M Tansey
- Post penicillin antibiotics**<sup>8</sup>  
 Organizers: Dr Robert Bud and Dr E M Tansey
- Clinical research in Britain, 1950–1980**<sup>9</sup>  
 Organizers: Dr David Gordon and Dr E M Tansey
- 1999     **Intestinal absorption**<sup>10</sup>  
 Organizers: Sir Christopher Booth and Dr E M Tansey
- The MRC Epidemiology Unit (South Wales)**<sup>11</sup>  
 Organizers: Dr Andy Ness and Dr E M Tansey
- Neonatal intensive care**<sup>12</sup>  
 Organizers: Professor Osmund Reynolds and Dr E M Tansey
- British contributions to medicine in Africa after the Second World War**<sup>13</sup>  
 Organizers: Dr Mary Dobson, Dr Maureen Malowany,  
 Dr Gordon Cook and Dr E M Tansey
- 2000     **Childhood asthma, and beyond**<sup>14</sup>  
 Organizers: Dr Chris O’Callaghan and Dr Daphne Christie
- Peptic ulcer: rise and fall**<sup>15</sup>  
 Organizers: Sir Christopher Booth, Professor Roy Pounder and Dr E M Tansey
- Maternal care**<sup>16</sup>  
 Organizers: Dr Irvine Loudon and Dr Daphne Christie
- 2001     **Leukaemia**<sup>17</sup>  
 Organizers: Professor Sir David Weatherall, Professor John Goldman,  
 Sir Christopher Booth and Dr Daphne Christie
- The MRC Applied Psychology Unit**<sup>18</sup>  
 Organizers: Dr Geoff Bunn and Dr Daphne Christie
- Genetic testing**<sup>19</sup>  
 Organizers: Professor Doris Zallen and Dr Daphne Christie
- Foot And Mouth Disease: The 1967 outbreak and its aftermath**<sup>20</sup>  
 Dr Abigail Woods, Dr Daphne Christie and Dr David Aickin

- 2002      **Environmental toxicology: The legacy of *Silent Spring***<sup>21</sup>  
Organizers: Dr Robert Flanagan and Dr Daphne Christie
- Cystic fibrosis**<sup>22</sup>  
Organizers: Dr James Littlewood and Dr Daphne Christie
- Innovation in pain management**  
Organizers: Professor David Clark and Dr Daphne Christie
- 2003      **Thrombolysis**  
Organizers: Mr Robert Arnott and Dr Daphne Christie
- Beyond the asylum: Anti-psychiatry and care in the community**  
Organizers: Dr Mark Jackson and Dr Daphne Christie
- The Rhesus factor story**  
Organizers: Professor Doris Zallen and Dr Daphne Christie
- 

<sup>1</sup> Tansey E M, Catterall P P, Christie D A, Willhoft S V, Reynolds L A. (eds) (1997) *Wellcome Witnesses to Twentieth Century Medicine*, vol. 1. London: The Wellcome Trust, 135pp.

<sup>2</sup> Hart, P D'Arcy, edited and annotated by Tansey E M. (1998) Chronic pulmonary disease in South Wales coalmines: An eye-witness account of the MRC surveys (1937–1942). *Social History of Medicine* 11: 459–468.

<sup>3</sup> Lock S P, Reynolds L A, Tansey E M. (eds) (1998) *Ashes to Ashes – The history of smoking and health*. Amsterdam: Rodopi B V, 228pp.

<sup>4</sup> Tansey E M, Christie D A, Reynolds L A. (eds) (1998) *Wellcome Witnesses to Twentieth Century Medicine*, vol. 2. London: The Wellcome Trust, 282pp.

<sup>5</sup> Tansey E M, Reynolds L A. (eds) (1999) Early heart transplant surgery in the UK. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 3. London: The Wellcome Trust, 72pp.

<sup>6</sup> Tansey E M, Christie D A. (eds) (1999) Haemophilia: Recent history of clinical management. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 4. London: The Wellcome Trust, 90pp.

<sup>7</sup> Tansey E M, Christie D A. (eds) (2000) Looking at the unborn: Historical aspects of obstetric ultrasound. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 5. London: The Wellcome Trust, 80pp.

<sup>8</sup> Tansey E M, Reynolds L A. (eds) (2000) Post penicillin antibiotics: From acceptance to resistance? *Wellcome Witnesses to Twentieth Century Medicine*, vol. 6. London: The Wellcome Trust, 71pp.

<sup>9</sup> Reynolds L A, Tansey E M. (eds) (2000) Clinical research in Britain, 1950–1980. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 7. London: The Wellcome Trust, 74pp.

<sup>10</sup> Christie D A, Tansey E M. (eds) (2000) Intestinal absorption. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 8. London: The Wellcome Trust, 81pp.

<sup>11</sup> Ness A R, Reynolds L A, Tansey E M. (eds) (2002) Population-based research in south Wales: The MRC Pneumoconiosis Research Unit and the MRC Epidemiology Unit. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 13. London: The Wellcome Trust Centre for the History of Medicine at UCL, 150pp.

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- <sup>12</sup> Christie D A, Tansey E M. (eds) (2001) Origins of neonatal intensive care in the UK. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 9. London: The Wellcome Trust Centre for the History of Medicine at UCL, 84pp.
- <sup>13</sup> Reynolds L A, Tansey E M. (eds) (2001) British contributions to medical research and education in Africa after the Second World War. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 10. London: The Wellcome Trust Centre for the History of Medicine at UCL, 93pp.
- <sup>14</sup> Reynolds L A, Tansey E M. (eds) (2001) Childhood asthma and beyond. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 11. London: The Wellcome Trust Centre for the History of Medicine at UCL, 74pp.
- <sup>15</sup> Christie D A, Tansey E M. (eds) (2002) Peptic ulcer: Rise and fall. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 14. London: The Wellcome Trust Centre for the History of Medicine at UCL, 143pp.
- <sup>16</sup> Christie D A, Tansey E M. (eds) (2001) Maternal care. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 12. London: The Wellcome Trust Centre for the History of Medicine at UCL, 88pp.
- <sup>17</sup> Christie D A, Tansey E M. (eds) (2003) Leukaemia. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 15. London: The Wellcome Trust Centre for the History of Medicine at UCL, 86pp.
- <sup>18</sup> Reynolds L A, Tansey E M. (eds) (2003) The MRC Applied Psychology Unit. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 16. London: The Wellcome Trust Centre for the History of Medicine at UCL, 94pp.
- <sup>19</sup> Christie D A, Tansey E M. (eds) (2003) Genetic testing. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 17. London: The Wellcome Trust Centre for the History of Medicine at UCL, in press.
- <sup>20</sup> Reynolds L A, Tansey E M. (eds) (2003) Foot and Mouth Disease: The 1967 outbreak and its aftermath. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 18. London: The Wellcome Trust Centre for the History of Medicine at UCL, in press.
- <sup>21</sup> Reynolds L A, Tansey E M. (eds) (2004) Environmental toxicology: The legacy of *Silent Spring*. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 19. London: The Wellcome Trust Centre for the History of Medicine, in press.
- <sup>22</sup> Christie D A, Tansey E M. (eds) (2004) Cystic fibrosis. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 20. London: The Wellcome Trust Centre for the History of Medicine, in press.



# ACKNOWLEDGEMENTS

'The MRC Applied Psychology Unit' was suggested as a suitable topic for a Witness Seminar by Dr Robert Bud. Dr Geoff Bunn provided many of the names of individuals to be invited, and assisted us in planning the meeting, and deciding the topics to be discussed. We are very grateful to them for their input. We are particularly grateful to Dr Geoff Bunn for writing such a useful Introduction to these published proceedings. We are equally grateful to Professor Richard Gregory for his excellent chairing of the occasion. Our particular thanks go to Dr Rhodri Hayward and Dr Alan Collins, who read through earlier drafts of the transcript, and offered us helpful comments and advice. For additional help, we thank Dr Ivan Brown, Dr Jim Milledge and the MRC Cognition and Brain Sciences Unit (CBU) for the photographs; Professor Richard Gregory and Professor Alan Baddeley for describing some of the experiments associated with the apparatus depicted in the photographs; Mr Kevin Symonds, Librarian of the CBU, Miss Nicola Weston from the Library of the National Institute for Medical Research, and Dr R Conrad, Dr John Duncan and Professor John Morton for additional help with the text.

As with all our meetings, we depend a great deal on our colleagues at the Wellcome Trust to ensure their smooth running: the Audiovisual department, the Medical Photographic Library and Mrs Tracy Tillotson of the Wellcome Library; Ms Julie Wood, who has supervised the design and production of this volume; our indexer, Mrs Liza Furnival, and our readers, Ms Ellen Clarke and Mr Simon Reynolds. We are particularly grateful to our Information Technology colleagues, Mr Gwyn Griffiths, Ms Vera Man and Mr Adam Wilkinson, for their help with both hard and software used in preparing this publication. Mrs Jaqui Carter is our transcriber, and Mrs Wendy Kutner and Dr Daphne Christie assist us in running the meetings. Finally we thank the Wellcome Trust for supporting this programme.

*Tilli Tansey*

*Lois Reynolds*

*Wellcome Trust Centre for the History of Medicine at UCL*

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The transcript of a Witness Seminar held at the Wellcome Trust Centre for the History of Medicine at UCL, London, on 12 June 2001

**Edited by L A Reynolds and E M Tansey**

# THE MRC APPLIED PSYCHOLOGY UNIT

## Participants

Professor Alan Baddeley

Dr Philip Barnard

Sir Christopher Booth

Dr Ivan Brown

Dr Geoff Bunn

Professor Sergio Della Sala

Mr David C Duncan

Professor Richard Gregory (Chair)

Professor John Groeger

Professor Graham Hitch

Dr Peter Hunter

Professor Robert Logie

Dr Brandon Lush

Professor William Marslen-Wilson

Professor Peter McLeod

Professor Pat Rabbitt

Professor Graham Richards

Dr Tilli Tansey

Dr David Tyrrell

Professor John West

**Others attending the meeting:** Dr David Allen, Dr Derek Bangham, Mr Robert Bennett, Dr Tim Boon, Dr Gordon Cook, Professor Hal Cook, Dr Megan Davies, Dr Harald Leyrer, Mr Bob Moore, Ms Alice Nicholls, Mr Stephen White

**Apologies:** Professor Michael Argyle,\* Dr Robert Bud, Professor Paul Bertelson, Mrs Margaret Broadbent, Professor John Brown, Dr Alan Collins, Professor Peter Colquhoun, Dr R Conrad, Dr Ann Dally, Dr James Le Fanu, Professor John Morton, Professor Sir Michael Rutter, Professor Timothy Shallice, Dr Fraser Watts, Dr Robert Wilkinson, Professor Alan Wing

\*deceased 6 September 2002

**Dr Tilli Tansey:** Good afternoon, ladies and gentlemen, and welcome to this meeting on the MRC's Applied Psychology Unit (APU).<sup>1</sup>

I am sorry for the delayed start; there have been some transport difficulties, and as you can see some people have still not arrived, but we think it would be best to get this meeting underway. My name is Tilli Tansey. I am the convenor of the History of Twentieth Century Medicine Group. This was established by the Wellcome Trust about ten years ago to try to increase interactions between scientists, clinicians, historians of medicine and medical sciences, and others who are interested in the recent history of medicine. To try to popularize these meetings we publish the entire proceedings. Therefore everything that is said today will be recorded, so I would ask you when you are going to contribute, and we hope that everybody here will contribute as and when they wish to; please raise your hand and wait for a roving mike to come to you. Will you please also say your name every time you contribute. It's important to know who has spoken, so that we can keep in touch with you afterwards and make sure that we have copyright clearance from you to publish what you have said and so that we can come back to you for any clarification of details or jargon, or anything else that we would like to make more transparent in the published transcript. We have invited one or two people to start the ball rolling on a number of set topics, but please don't feel that this prevents anybody else from contributing in these areas. We do want to hear from everybody here today, your views and your opinions.

The meeting today has been organized by Dr Geoff Bunn, from the Science Museum, and my colleague, Dr Daphne Christie, from the History of Twentieth Century Medicine Group. The idea was suggested by Dr Robert Bud of the Science Museum, and we are also indebted to Mr Stephen White of the British Psychological Society<sup>2</sup> for his assistance in advising and organizing the meeting. A very important part in the meeting is identifying an appropriate chairman, someone who can contribute personally and steer the meeting authoritatively, and we are delighted that today Professor Richard Gregory from Bristol has agreed to chair this meeting. Professor Gregory is perhaps one of the best-known

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<sup>1</sup> The unit was known as the MRC Applied Psychology Research Unit, and, like many others, the term 'research' was dropped from its title in 1959–60. See Thomson A L. (1975) *Half a Century of Medical Research: The Programme of the Medical Research Council (UK)*, vol. 2. London: Medical Research Council, 354, 172–177. See also, for example, Ness A R, Reynolds L A, Tansey E M. (2002) Population-based Research in South Wales: The MRC Pneumoconiosis Research Unit and the MRC Epidemiology Unit. *Wellcome Witnesses to Twentieth Century Medicine*, vol. 13. London: Wellcome Trust Centre for the History of Medicine at UCL.

<sup>2</sup> The British Psychological Society was established in 1901 at University College London to advance scientific research in psychology and promote cooperation among the different branches of the subject. Members were initially limited to recognized teachers of psychology or those having published work of recognizable value. See Edgell B. (1947) The British Psychological Society. *British Journal of Psychology* 37: 111–132. See also [www.bps.org.uk/about/history.cfm](http://www.bps.org.uk/about/history.cfm) (visited 5 November 2002).

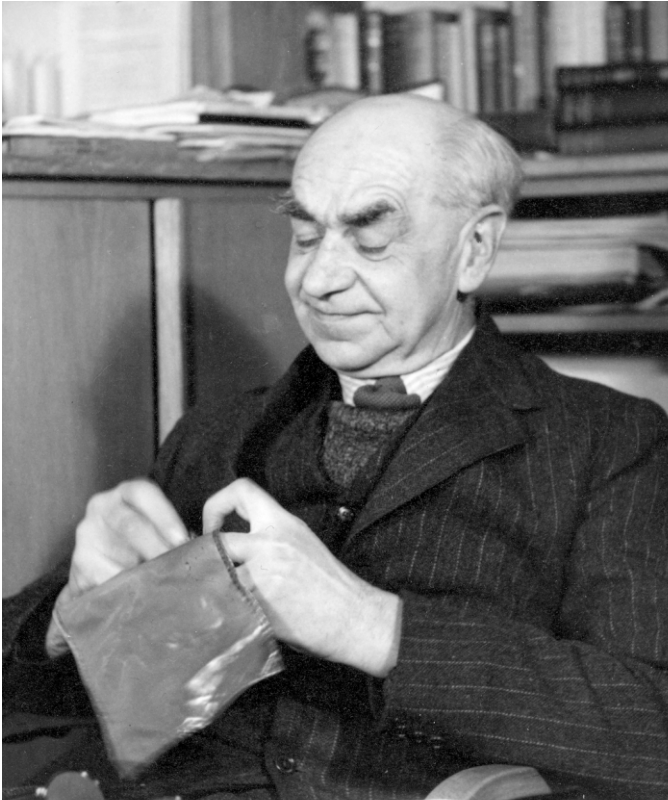


Figure 1: Professor Sir Frederic Bartlett (1886–1969).

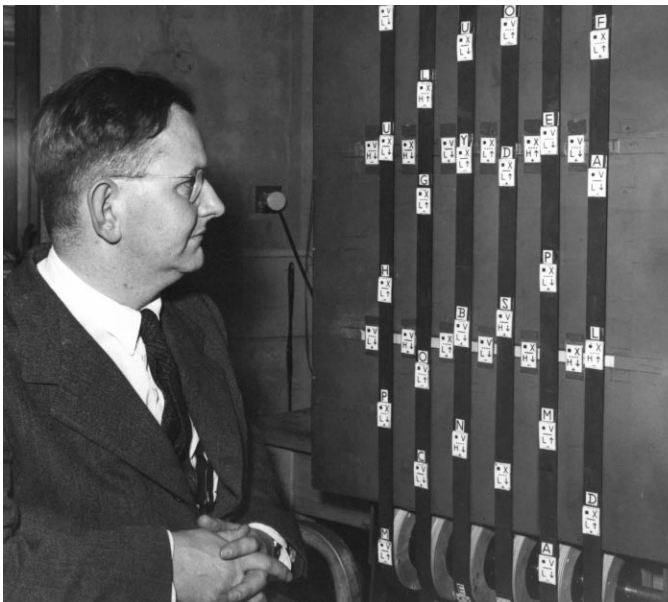


Figure 2: Dr Norman Mackworth.

psychologists, not only in this country, but in the world, and he is particularly well-known for his work on sensory perception, but equally known for his work on what we would now call public understanding of science in his own writings, his edited books, and in particular in the Bristol Exploratory Science Centre.<sup>3</sup> So we are particularly pleased and grateful to Professor Gregory for coming here today to chair this meeting, and so without further ado I will hand over to the chairman, Professor Gregory.

**Professor Richard Gregory:**<sup>4</sup> Thank you very much. I am really very honoured to be chairing this meeting, and I think it is a lovely occasion, where we can get together. We all had a tremendous amount of fun in the APU and I don't know about you, but I absolutely adored it. I thought it was wonderful, it really was, and still is, I am sure. I started my own career there, very briefly, exactly 50 years ago, with the changeover from Sir Frederic Bartlett<sup>5</sup> to Dr Norman Mackworth,<sup>6</sup> Mac as we called him. Mac was my boss and I got on with him extremely well. A very nice man, a medical doctor; he was jovial, he was delightful, and the story is (and I think Geoff Bunn will probably corroborate this) that he actually bought Chaucer Road for the MRC without them knowing it, but we will probably hear about that later. I was only in the APU for about three years, and it wasn't in Chaucer Road!<sup>7</sup> It was when the APU was in the psychology building in Downing Street. It moved to the APU slightly later. Bartlett was my professor. I rather liked teaching, so I got a lectureship in the department in 1956. I had this choice to make, and I have never known whether I made the right choice or not, but I certainly adored the APU.

One of the things that I liked about it so much was that it absolutely linked practical work (you were given a really practical problem, I will just mention what I was given in a minute), with any theoretical work you wanted to do. I wrote an essay that went on and on, about the brain as an induction machine, which I am sure nobody ever read. I was also lent to the Navy. I was seconded to work on escaping from submarines, with a year at Whale Island in Portsmouth, going down in the submarines and working in a pressure chamber where you

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<sup>3</sup> The Exploratory Science Centre in Bristol was a try-it-yourself science and technology centre where children and adults had the opportunity to discover principles of science and how things work, with hands-on interactive working models, demonstrations and simple experiments. It closed on 1 September 1999. See [www.exploratory.org.uk/](http://www.exploratory.org.uk/) (visited 11 November 2002).

<sup>4</sup> For biographical notes, see Appendix starting on page 81.

<sup>5</sup> See Figure 1. For details of Bartlett's role in the creation of the unit, see also Thomson A L. (1975), note 1, 173.

<sup>6</sup> See Figure 2.

<sup>7</sup> A Victorian house at 15 Chaucer Road, Cambridge, has been the home of the MRC Applied Psychology Unit since its purchase by Dr Norman Mackworth in 1952. See Figure 8, page 12. The APU was re-named the Cognition and Brain Sciences Unit (CBU) in 1998. See also [www.mrc-cbu.cam.ac.uk/](http://www.mrc-cbu.cam.ac.uk/) (visited 21 October 2002) and note 121.



Figure 3: 'The recording device shown in the photograph, which I called Thoth, after the Egyptian god of writing and wisdom, was built to record the results (which were signalled with electrical contacts on the escape controls) of the simulation of a disabled submarine. It was based on a pallenotype machine, with a keyboard and the letters of the alphabet – used for shorthand. In front of the curved strips (below the vertical push rods) was the keyboard working the pallenotype. Unlike a typewriter any number of keys could be pressed at once. To the left is a clock, in the box, which was used by RAF fighter command for allocating 15-second periods for transmission and reception with four different “coloured” squadrons sending pulses to the uni-selector – which is the curved object in front of the box. This part was from telephone exchanges - we used whatever we could get hold of – and was wired up to code letter keys to indicate time, I think to the nearest second. The experiment lasted ten hours. Later I redesigned the time system using a mechanical counter like the mileometer on a car, with the typeface on the wheels, which printed time in numbers. This was quicker to read. The device was then made a lot smaller and neater, later manufactured by the Cambridge Instrument Company. It was not a total success as it was not entirely reliable, but it was a start towards automatic recording.' Professor Richard Gregory, 3 and 5 December 2002.

could control the oxygen and carbon dioxide in submarine escape.<sup>8</sup> We recommended changes to the submarines. We had lost the *Affray*, which was the reason for doing this. So that was a completely practical problem, but, of course, like any practical problem, it ended up with some theoretical issues.

My own theoretical contribution was being a subject for W E Hick, in his great 'Rate of gain of information' experiment.<sup>9</sup>

I was actually the only subject. Hick's Law relating reaction time to the number of choices is based on my nervous system. He was the only other subject and he got bored doing it, so he never finished the experiment, which went on for months.

We were given practical problems, being allowed a lot of initiative and responsibility. I think Mac visited me once down at Portsmouth and he didn't like the idea of going in a submarine. I was very much left to myself, with 40 submariners working under me. I worked with Christopher Poulton, on blinking and tracking,<sup>10</sup> which sounds a pretty boring thing to do, but in actual fact it was fascinating and Chris was a wonderful experimenter. I wish I had learned more from him. He was a much better experimenter than I have ever been, but there you are.

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<sup>8</sup> See Figure 3 on page 6. Professor Richard Gregory wrote: 'Mackworth (always known as Mac) was shrewd, with a good sense of purpose, and was a good judge of people and projects suitable to them. He gave us incredible freedom, once one was trusted. The apparatus [Figure 3] was the recording machine that I designed and made with my technician – for the submarine escape experiments following the sinking of the submarine *Affray*, with two crews on board, all of whom died. There was a large pressure chamber (built by the Navy) that would hold ten people at a time; the atmosphere could be changed, simulating reduced oxygen and increased carbon dioxide as for a submarine disabled at the bottom of the sea. It had dummy escape gear in the chamber, which went through, I think, 42 operations for each man to escape, as the atmosphere got worse and worse. This recording machine was soon superseded by other devices and, of course, computers. I think it was the first recorder to separate the movement of the paper from the printed record, as it printed time (to the nearest tenth of a second) and the channels (kinds of events) were represented by different letters. Up until then we had to read times from wiggly lines on continuously moving paper.' E-mail to Mrs Lois Reynolds, 3 and 5 December 2002.

<sup>9</sup> Hick W E. (1952) On the rate of gain of information. *Quarterly Journal of Experimental Psychology* 4: 11–26. Professor Richard Gregory wrote: 'There were only two subjects, William Hick and myself. Actually, I was the one who completed all the testing. The logarithmic function used fitted the recently invented (or discovered) mathematical theory of information, by C E Shannon. This was published in readable form by Shannon C E, Weaver W. (1949) *The Mathematical Theory of Communication*. Urbana: University of Illinois Press.' E-mail to Mrs Lois Reynolds, 5 December 2002. See Figure 4, page 8.

<sup>10</sup> Poulton E C. (1950) Perceptual anticipation and reaction time. *Quarterly Journal of Experimental Psychology* 2: 99–112. Poulton E C, Gregory R L. (1952) Blinking during visual tracking. *ibid.* 4: 57–65. Poulton E C. (1952) The basis of perceptual anticipation in tracking. *British Journal of Psychology* 43: 295–302. Gregory R L. (1952) Variations in blink rate during non-visual tasks. *Quarterly Journal of Experimental Psychology* 4: 165.





Figure 4: 'This apparatus had little lights, which were fixed with pins to the circular cork mat. The keys are morse code keys. One had to learn the position of each light (which was randomized) for each finger key, until the response was fast and automatic. Then the number of lights – that is the number of choices – was varied from 1 to 10. Reaction time was measured and integrated (with a mechanical integrator that W E Hick [left] made himself). The result was that the response time increased to the logarithm base 2, +1. This is called Hick's Law, which predicts the increased time a human will take to react to a stimulus as the number of possibilities increases. He attributed the number 1 to the choice of not pressing a key.'  
Professor Richard Gregory,  
5 December 2002.

I would just like to say that I thought it was a fantastic way of starting a career, in that lovely house with its grounds, and exciting people, with links, of course, to the university department, which were extremely strong. Many people in the APU supervised students. Violet Cane was somebody who particularly linked the two together. I worked with her on a mathematical series of sensory thresholds<sup>11</sup> and she did a lot of work advising on statistics for the APU and also for the university. There was really a great deal of collaboration. And lastly, I think I should just mention the panels, the subject panels.<sup>12</sup> There were originally

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<sup>11</sup> Gregory R L, Cane V. (1955) A statistical information theory of visual thresholds. *Nature* 176: 1272.

<sup>12</sup> Thirty-five years of panel work was celebrated by the CBU (formerly APU) in 1999. The Research Panel at the unit had nearly 1200 members in 1998, ranging in age from 16–97, made up of ordinary people from all walks of life, willing to volunteer on a regular basis to take part in psychological experiments. See [www.mrc-cbu.ac.uk/Common/PanelNews/panelnews.shtml](http://www.mrc-cbu.ac.uk/Common/PanelNews/panelnews.shtml) (visited 6 November 2002).

people from the Navy and also probably from the Royal Air Force, who were posted to Cambridge for two weeks at a time, as subjects for experiments. Then there were housewives in Cambridge who volunteered. It is rather a shame that we don't have a subject in this meeting who could tell us what it was like actually sitting in a chair, hour after hour, looking at little flashing lights or twiddling knobs round, or whatever it was they had to do, and whether they thought we were totally mad. They were highly motivated and much valued.

Now Geoff Bunn is going to give us a brief introduction. I would like to say that Geoff has set up a trail – a psychology trail – in the Science Museum. There are several exhibits around the museum on flying and navigating and the psychological problems of aviation, especially. So there's a trail being developed by Geoff in which psychology takes its place in the technology and science of the Science Museum. Perhaps Geoff could tell us about this. At any rate, he is now going to give a historical introduction to the APU. Thank you, Geoff.

**Dr Geoff Bunn:** My all too brief historical remarks have been greatly assisted by Alan Collins from Lancaster, who has started to work on a history of the APU.<sup>13</sup> Alas he can't be here with us today, due to examination duties, but my thanks are to him for help with these notes.

The origins of the APU lie with the Industrial Fatigue Research Board, established during the First World War to investigate the productivity of munitions workers.<sup>14</sup> A second tradition of applied psychology in Britain can be traced to HMS Crystal Palace, where Charles Myers led a team of psychologists researching selection tests for hydrophone operators in 1917. Bartlett had been associated with this, and also with the subsequent

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<sup>13</sup> Dr Alan Collins wrote: 'I provided Geoff [Bunn] with a fair amount of material, as I was unable to attend myself. I did not proceed with my own research on the unit as I was told (correctly) that a former member of the unit was going to do a history. News of his involvement altered my direction, so I didn't pursue it and I haven't published anything. Despite abandoning the project, I think this is a very important area and an excellent topic for a historical publication.' E-mail to Mrs Lois Reynolds, 20 January 2003.

<sup>14</sup> Health of Munition Workers Committee. (1918) *Industrial Health and Efficiency: Final Report*. Cmd 9065. London: HMSO. Sir George Newman, Chairman. The MRC's Industrial Fatigue Research Board, established in 1918 as a national scheme for industrial medical research and to encourage industrial hygiene, was renamed the Industrial Health Research Board in 1928 as better expressing the Board's purpose. See Schilling R S F. (1944) Industrial health research: the work of the Industrial Health Research Board, 1918–44. *British Journal of Industrial Medicine* 1: 145–152. See [www.mrc-cbu.cam.ac.uk/Common/contact/contact-history.shtml](http://www.mrc-cbu.cam.ac.uk/Common/contact/contact-history.shtml) (visited 17 October 2002). Dr Geoff Bunn wrote: 'The papers for the IFRB/IHRB are held at the Public Record Office, Kew. See, for example, "Correspondence between Sir George Newman and Sir Walter Fletcher concerning the Health of Munition Workers Committee, 1918" (PRO FD3/120) and "Sir Charles Sherrington and Sir Walter Fletcher: Correspondence concerning composition of Industrial Fatigue Research Board, 1918" (PRO FD3/121).' Note on draft transcript, 11 December 2002.



Figure 5: Dr Christopher Poulton with a subject.



Figure 6: Tracking experiment.

National Institute of Industrial Psychology (NIIP),<sup>15</sup> and he had observed at first hand how the institute's ambitions had been compromised by its perennial financial crises.

If Myers took psychology into industry, then Bartlett brought industry back into psychology. Moves to found a unit for applied psychology began in 1943, at the Industrial Health Research Board.<sup>16</sup> Bartlett maintained that although he saw the work of such a unit as being a form of industrial psychology, he did not consider vocational selection to be a suitable topic for research. The complexity of weaponry and equipment increased the importance of designing machines and tasks that took account of human capabilities. Bartlett highlighted work by Kenneth Craik,<sup>17</sup> on the design of machines, and by Norman Mackworth on the design of displays,<sup>18</sup> as examples of the kind of research that an applied psychology unit could undertake.

<sup>15</sup> Dr C S Myers, Director of the Cambridge Psychological Laboratory, founded the NIIP and became its first Director in 1921, supported by a group of businessmen led by H J Welch. It was a non-profit-making scientific organization for the study of industry and commerce that promoted the use of psychology and physiology within these fields. The Institute stopped work in 1973, was later revived with funds from the North East London Polytechnic, and closed in 1976. A collection of its papers from 1919–77 (GB 0097 NIIP) is held by the British Library of Political and Economic Science, London. See Welch H J, Myers C S. (1932) *Ten Years of Industrial Psychology: An account of the first decade of the National Institute of Industrial Psychology*. London: Pitman, 1932. Dr Geoff Bunn wrote: 'See "Proposed school of Industrial Efficiency and Physiological Organization: Correspondence 1918–22" (PRO FD3/116). Doyle D C. (1979) *Aspects of the Institutionalization of British Psychology: The NIIP 1921–39*. Unpublished PhD dissertation, Manchester University. Bunn G. (2001) 'A flair for organization': Charles Myers and the establishment of psychology in Britain. *History and Philosophy of Psychology* 3: 1–13.' Note on draft transcript, 11 December 2002.

<sup>16</sup> For details of the establishment of the unit, and progress reports for the period 1943–53, see FD1/4143, /4144, /4145 at the Public Record Office, Kew. See also Poulton E C. (1964) The MRC's Applied Psychology Unit. *MURMUR* [Cambridge University Medical Society Magazine] 10: 3–7.

<sup>17</sup> See, for example, Craik K J W, Vince M A. (1943) *Psychological and Physiological Aspects of Control Mechanisms: with special reference to tank gunnery*. Part I. MRC Military Personnel Research Committee report. London: MRC. [also published in *Ergonomics*, 1963, 6: 1–33.] *idem* (1944) *ibid*. Part II. MRC Military Personnel Research Committee report. London: MRC. [also published in *Ergonomics*, 1963, 6: 419–440.] See also Green F H K, Covell G. (1953) *Medical Research: History of the Second World War, United Kingdom Medical Series*. London: HMSO, 23–33. For details of Craik's unpublished reports for the Flying Personnel Research Committee, see Hayward R. (2001) 'Our friends electric': mechanical models of mind in postwar Britain, in Bunn G C, Lovie A D, Richards G D. (eds) *Psychology in Britain: Historical essays and personal reflections*. Leicester: British Psychological Society Books, note 15, 307.

<sup>18</sup> Mackworth N H. (1956) Vigilance. *Nature* 178: 1375–1377. See also *idem* *The Design of Equipment for Human Use*. Full summary of a meeting held in Cambridge at the Applied Psychology Unit of the MRC on 26–28 March 1957. Copy held in CBU Library. Bartlett F C, Mackworth N H. (1950) *Planned Seeing: Some Psychological Experiments*. I. Visibility in the control rooms of Fighter Command. II. The synthetic training of Pathfinder air bombers in visual centring on target indicators. Air Ministry Air Publication 3139b. London: HMSO.



Figure 7: Kenneth Craik [left] was described as 'very adept at making things with his hands and would carry about with him a tobacco tin containing several small model steam engines that he had made, the smallest being truly minute...Craik's interest in gadgets, both electrical and mechanical, ...was unusual at that time among people who worked in biological departments. Craik joined Bartlett's group at a time when ability of this kind was just what was needed and possessing it put him in a key position. A new building came into use while Craik was still a research student, and Bartlett relied on him heavily for the equipping of the workshop and the ordering of materials for it.' See Mollon J, Pelah A. (1998) *The Kenneth Craik Club: a short history. Programme for Craik Club 25th Anniversary Meeting, Cambridge, 22–23 August 1998*. Cambridge: Craik Club. This includes 'Some assessments of Kenneth Craik', from which the above quote is taken. See Wilkes M. (1985) *Memoirs of a Computer Pioneer*. Boston: MIT Press, page 16.



Figure 8: Number 15 Chaucer Road, Cambridge: home of the MRC Applied Psychology Unit since 1952. The building on the left was added after the unit changed its name in 1998. See note 7.

Although the university was initially lukewarm about the idea, in 1944 the Unit for Research in Applied Psychology was established within the Cambridge Psychological Laboratory. Eric Farmer, Cambridge Reader in Industrial Psychology, was initially proposed to head the unit, but it was Bartlett's protégé, Craik, who got the job.<sup>19</sup> Craik had pioneered the use of computers as models for human information processing, developing a computational model of skill and applying it to the task of gun aiming.<sup>20</sup> He argued that the purpose of the unit was to extract more general principles from apparently local problems concerning psychological capabilities. Alas, he was tragically killed in a cycling accident in 1945, the year after the unit was founded, and was succeeded by Bartlett. Bartlett retired in 1951 and was replaced as director by Norman Mackworth. Mackworth found that the unit had outgrown the space available within the Psychology Department and the annexe that it had acquired. Noticing a pleasant Victorian house on the outskirts of the city, with a very large garden, he bought it, only later informing the MRC. This building in Chaucer Road with its subsequent extensions has remained the home of the unit ever since.

The community of experimental psychologists in Britain in the early 1950s was small, but growing rapidly, and the APU was a focal point amidst that expansion. In the 1949–50 progress report the principal research areas were listed as: bodily and mental skills, climatic psychology, problems and methods in the use of mental tests, and road research problems. A more detailed breakdown gave: experiments on thinking and perceiving, researches on skilled muscular movements, visual display problems, effects of abnormal environments, new devices, temperament and social studies, and statistical investigations.

In 1953 the word *information* appeared in the annual report, defining two main areas of interest. Information theory was acknowledged as a potentially important new approach, especially in the quantitative treatment of skill. Memory had also emerged as a key area of the research programme. This arose in two ways: first through attempts to explain the performance of tasks using the information-processing approach. Second, through the particular practical demands of research projects, such as those sponsored by the Ministry of Transport and the Post Office, on remembering telephone numbers.<sup>21</sup> Memory was a perfect fit for the stated objectives of the APU: to observe and measure human behaviour in order to establish general principles about healthy human performance, and to ensure that such principles should be of general scientific interest, and also of practical value.

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<sup>19</sup> Craik K J W. (1945) A new Cambridge unit of research in applied psychology. *Occupational Psychology* **19**: 15–19. *idem* (1944) Medical Research Council unit for applied psychology. *Nature* **134**: 476–477.

<sup>20</sup> Craik K J W with Holden H C L, Langford B A. (1942) *Note on Optimum Dimensions of Trigger and Grip*. BPC (Military Personnel Research Committee). See also Craik K J W. (1943) *The Nature of Explanation*. Cambridge: Cambridge University Press.

<sup>21</sup> See note 89.



Figure 9: The Applied Psychology Unit in 1956.

In 1958 after Mackworth's resignation, Donald Broadbent became director. Broadbent's classic book *Perception and Communication*,<sup>22</sup> published a few months later, was to be a major influence during the period when cognitive psychology became a dominant

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<sup>22</sup> Broadbent D. (1958) *Perception and Communication*. Oxford and New York: Pergamon Press.

theoretical paradigm. One historian has defined the period of the late 1950s as a breakthrough era for the APU. The 1960 report organizes the work of the unit under four headings. These were intended to reflect the general account of what has to be understood in order to explain a man doing a job: (1) the background conditions of work, such as heat and humidity, loud noise, loss of sleep, level of arousal; (2) the intake of information, selection of stimuli, dual task performance, specific types of stimulation; (3) the central processes in the handling of information, such as choice reaction times, short-term memory and so on; and (4) the execution of actions.

Broadbent concluded that tentatively, one might say that all these results, taken together with work from other laboratories, suggest a preference for a mechanism for a short-term memory quite different from that used for long-term memory. Such a distinction has, of course, long been familiar in theories of the brain put forward by cyberneticists, since computing machines have often two types of memory.

By 1964 the unit employed 18 academic staff, and about the same number of support personnel. The main areas of research were: performance in adverse environmental conditions, such as high altitude and shift work; measures of perception, motor skills, driving, and the demands on mental capacity; intellectual processes, including the relationships between the intake of sensory information and short-term memory; issues around rehearsal and memory; and work on the problems that would arise from decimalization.

Alan Baddeley succeeded Broadbent in 1974. Baddeley ensured that the strengths in the major areas of cognitive psychology were maintained, while simultaneously seeking practical areas of application that appealed to potential sponsors. With the increasing importance of computers in the workplace in the 1970s and 1980s, a large part of the unit's work was conducted in association with industry, although this later diminished as recession affected industrial research and development budgets.

In the beginning of the 1990s health-related problems provided the major basis for applying and enriching cognitive psychology, which was extended to encompass the relationships between cognition and emotion, itself part of the vigorous area of interaction between academic and clinical researchers. Work was also undertaken in the new field of cognitive neuropsychology.

William Marslen-Wilson was appointed [the sixth] Director in 1997, following Alan Baddeley's move to Bristol. He joined at a time of substantial scientific and organizational change. His remit was to redevelop the unit's scientific programme, and to concentrate research round the task of constructing theories of major human psychological functions, such as attention, memory, and emotion, by marrying together psychological and computational accounts of cognitive function and architecture with increasingly detailed analyses of how these functions are realized in the brain. In order to reflect these changes, the unit was renamed in April 1998 the Cognition and Brain Sciences Unit. For over 50



years the APU has had a major impact on psychology, both in Britain and abroad. It is a history we look forward to hearing about.

**Gregory:** Thank you very much, Geoff. Does anybody have any comments to make, or contributions?

**Dr Brandon Lush:** I was secretary of the Industrial Health Research Board (IHRB) for some years, from 1951, and was then succeeded by Dr Joan Faulkner, wife of Sir Richard Doll, as we all know. The board started as the Industrial Fatigue Research Board in about 1915, if I remember rightly, because the government of the day was very worried about the falling-off in performance of workers on production lines. As the hours got longer and longer, some of them were working as much as 12 hours a day. And the board was then renamed the IHRB as the efforts moved from repetitive work to much wider conditions of work practice. The IHRB finally folded up<sup>23</sup> because the MRC appointed a whole lot of new committees and working parties on working efficiency, occupational health, and the Armed Services Committee, because the Applied Psychology Research Unit (APRU) did a great deal of work, as the Chairman has already said, for the armed services.

**Gregory:** Has anybody got any funny stories of that time? A bit of humour is in order I am sure.

**Professor Graham Richards:** Really this is a question to the APU veterans. I am particularly interested in the military connection that the APU had and I would like to understand a bit more about how that connection declined, if it did, and their views on the impact of this connection on the nature of psychological research and so on. I'd like to know whether they felt having a connection with the military and being connected in a sense with the military subculture, in doing that research – as our chairman mentioned, going into submarines – what kind of a background effect they thought that connection would have?

**Gregory:** I agree. I found that very interesting. I was actually on a bunch of committees, I think most of us were. I was on a radar committee and I used to fly around in Nimrods over the Arctic. One time, the whole of the air-conditioning broke down – the computer broke down, so we had to navigate back using school navigating dividers and protractors. It was really quite something. I think that a lot of us had exciting experiences linked with the military; I certainly did. Did other people?

**Mr David Duncan:** I started by going through the RAF selection battery for aircrew, but I joined rather too late and was made redundant at the age of 18. I found myself in a situation

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<sup>23</sup> Dr Brandon Lush wrote: 'The work initiated by the IHRB continues to this day. It is only the board that was disbanded in 1953.' Letter to Mrs Lois Reynolds, 18 November 2002. See Thomson A.L. (1975), note 1, 165–179.

where aircrew were employed on monotonous clerical work and morale was very poor. I discovered that the only people who seemed to write about morale were called industrial psychologists,<sup>24</sup> so I decided that I must become an industrial psychologist when I got back to college. So my subsequent studies moved from English and history to economics, psychology and philosophy. Then I approached the National Institute of Industrial Psychology (NIIP), which was my hero on the subject of morale and motivation. They said, ‘Get yourself some business experience and come back in a few years’ time’, which I did. Eventually I joined the NIIP as a research psychologist to work on a Medical Research Council project that was on the selection, training, and promotion of supervisors. My first publication was with John Handyside in *Occupational Psychology*, in January 1954.<sup>25</sup> It was the validation of an assessment procedure for supervisors based on War Office selection board lines, which was a very successful event.

The NIIP was quite a contrast to the APU. In fact we envied them, because they were civil servants with three-year contracts, and we had no contract, not even a pension scheme. But what made up for this was that when our time at the NIIP came to an end, we could always get good jobs in industry and that suited me because my aim was not particularly to be a research psychologist, but to investigate the problems of morale in factories. And the APU did tremendous work on the development of models of man – what I call the hypothetico-deductive approach – and it seemed to me that they had competition within Cambridge from people such as John Maynard Keynes, who when they needed a new theory just invented it.<sup>26</sup>

The APU did a lot in the way of developing models that fitted crisis situations, and particularly in flying. I think it was Clement Freud<sup>27</sup> who said, ‘Anybody can fly a plane, it takes a genius to land it.’ Anyone who has flown knows that the crisis point comes when

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<sup>24</sup> Mr David Duncan wrote: ‘Two books influenced my decision: Maier N R F. (1947) *Psychology in Industry*. London: Harrap. Oakley C A. (1947) *Men at Work*. London: Hodder and Stoughton, University of London Press.’ Note on draft transcript, 19 November 2002.

<sup>25</sup> Handyside J D, Duncan D C. (1954) Four years later: a follow-up of an experiment in selecting supervisors. *Occupational Psychology* 28: 9–23.

<sup>26</sup> Lord John Maynard Keynes (1883–1946) outlined his ‘general theory’ in 1936, which revolutionized economic theory by showing how unemployment could occur involuntarily and argued that the lack of demand for goods and rising unemployment could be countered by increased government expenditure to stimulate the economy. His views influenced President Roosevelt’s New Deal and Britain’s postwar Labour Government. He was made a baron in 1942 and acted as Chief British Negotiator on Lend-Lease and the US loan to Britain shortly before his death. Keynes J M. (1936) *The General Theory of Employment Interest and Money*. London: Macmillan.

<sup>27</sup> Mr David Duncan wrote: ‘I have been unable to contact Sir Clement Freud to confirm my attribution, but I am certain that, for me, he is the originator.’ Note on draft transcript, 19 November 2002.



Figure 10: Card-sorting experiment during the Silver Hut expedition 1960–61 undertaken by Dr Tom Nevison, a US climber who was a physician with NASA and a member of the expedition, later an anaesthetist. The experiment was devised by the APU to test the effects of altitude and fatigue on performance. See note 32.



Figure 11: Silver Hut laboratory of the Himalayan Scientific and Mountaineering Expedition, 1960–61, location of the card-sorting experiment pictured above.

you come in to land. When I was training, Tiger Moths landed at 40 miles an hour; by the time I left the RAF in 1947, they were landing at 150 miles an hour, because jets really pushed up speeds. So the man-machine interaction, the improvement of display and control, became quite crucial. Nowadays I am amazed at the way in which the Tornado fighters go about 200 feet above the ground and they go whizzing up the valley of the River Tay, below the level of the hills; how they manage not to run into the hills I just don't know.

**Gregory:** Thank you very much. I expect we all remember Bernard Gibbs.<sup>28</sup> He was a great man on tracking, combining engineering with psychology, wasn't he? It's a shame he's not with us now. Well, we have to leave that topic and move on to neuropsychology. Tim Shallice should have been here, and sadly he isn't. John West has very kindly offered to take his place, which is very noble of you, John, thank you.

**Professor John West:** Well, I am not a professional applied psychologist, but I have some interest in neuropsychological testing, and in particular at high altitude, which was briefly mentioned before. It turns out that some of the best work on that was done in Cambridge, or rather by someone from Cambridge, and that's Sir Joseph Barcroft, who led an expedition to Cerro de Pasco in Peru in 1920–21, and wrote an account of that which included some very good material on neuropsychological testing. He pointed out that, of course, people at high altitude are impaired. In fact, he said, 'All dwellers at high altitude are persons of impaired physical and mental powers,'<sup>29</sup> and he was absolutely right. But one of the fascinating things was that when the expedition members measured neuropsychological function, for example, cognitive function, of people at high altitude, they had a tremendous problem in showing any impairment. They knew they were impaired, for example they made lots and lots of errors at high altitude, but when they sat somebody down in front of the testing equipment, if they were a competitive type A personality,<sup>30</sup> they could concentrate so hard on the tests that it was not possible to pick up impairment, and I think that's a very interesting problem that still exists today. We have done quite a lot of

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<sup>28</sup> Gibbs C B. (1951) Transfer of training and skill assumptions in tracking tasks. *Quarterly Journal of Experimental Psychology* 3: 99–110. Gibbs C B, Baker J C. (1951) Free-moving versus fixed control levers in a manual tracking task, in Department of Scientific and Industrial Research. (1952) *Automatic and Manual Control*. Papers and discussions from the Conference on Automatic Control held in July 1951 at Cranfield. London: Butterworth Scientific Publications, 467–472. Gibbs C B. (1952) *Car Turning Signals and Delays in Responding to Visual Information*. Applied Psychology Unit Report no. 176. Cambridge: Applied Psychology Unit.

<sup>29</sup> Barcroft J. (1925) *The Respiratory Function of the Blood. Part I: Lessons from High Altitudes*. Cambridge: Cambridge University Press, 176.

<sup>30</sup> A competitive type or coronary-prone person was first described in 1959. See Friedman M, Rosenman R H. (1959) Association of specific overt behaviour pattern with blood and cardiovascular findings. *Journal of the American Medical Association* 169: 1286–1296.

neuropsychological testing at high altitude and it's actually quite difficult to find objective tests that will show impairment,<sup>31</sup> although everybody realizes they are, in fact, impaired. So that's one of the paradoxes. (Barcroft, incidentally, for those of you who don't know, was an eminent physiologist and biochemist, at Cambridge in the teens and the twenties of the last century.) Barcroft referred to people bumbling at high altitude and that was absolutely right.

We used a Cambridge test, I guess it was from the APU, although I didn't realize that [at the time]. In 1960 Griffith Pugh, a well-known British high-altitude physiologist, had an expedition of which I was a member – the Silver Hut expedition – led by Sir Edmund Hillary, and we made measurements of neuropsychological function when we were living at an altitude of 5800 m, that's 19 000 ft. This was done using card sorting, and was organized by people at Cambridge.<sup>32</sup> What we found was, as Barcroft himself reported, that you could do accurate work at high altitude, but it took longer. You could do it if you had a well-defined protocol.

In 1981 we had an expedition to Mount Everest, where we also did neuropsychological testing and there we made an interesting finding that I will just mention briefly. At that time we were interested in whether there would be residual impairment of neuropsychological function, for example cognitive function, after a period spent at very high altitude [over 6300 m], and in fact we showed that that was the case.<sup>33</sup> It was one of the first demonstrations and has since been shown a number of times. The impairment was in short-term memory, and also manipulative skills, for example finger tapping. Our measurements were made immediately after we got back from the expedition, and also a year later. The short-term memory returned quickly, within a year, but the manipulative skills were still abnormal for at least a year after the expedition. So you can't go to these very high altitudes and escape completely unscathed.

An interesting sidelight on that, for those of you who may be interested in respiratory physiology, is that when you go to high altitude the most important change that occurs is the increase in pulmonary ventilation. The more air you move, the higher you maintain the

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<sup>31</sup> Gerard A, McElroy M K, Taylor M J, Grant I, Powell F L, Holverda S, Sentse N, West J B. (2000) Six per cent oxygen enrichment of room air at simulated 5000 m altitude improves some aspects of neuropsychologic function. *High Altitude Medicine and Biology* 1: 51–61.

<sup>32</sup> Gill B M, Poulton E C, Carpenter A, Woodhead M M, Gregory M H P. (1964) Falling efficiency at sorting cards during acclimatization at 19 000 feet. *Nature* 203: 436. Pugh L G C E. (1962) Physiological and medical aspects of the Himalayan Scientific and Mountaineering Expedition 1960–61. *British Medical Journal* ii: 621–627. See Figures 10 and 11, page 18.

<sup>33</sup> Townes B D, Hornbein T F, Schoene R B, Sarnquist F H, Grant I. (1984) Human cerebral function at extreme high altitude, in West J B, Lahiri S. (eds) *High Altitude and Man*. Bethesda MD: American Physiological Society, 31–36.

level of oxygen in your lungs, so the increased ventilation is extremely important. What we found, though, was that people with the highest ventilation at high altitude had the greatest residual impairment of neuropsychological function, and that was a paradox. However, it's likely that the people with very high ventilations had the lowest  $PCO_2$ s (carbon dioxide partial pressures), and that caused cerebral vasoconstriction, so that the partial pressure of the oxygen of brain tissue was actually lower, even though the partial pressure of oxygen in the arterial blood was higher.<sup>34</sup> So that was interesting.

Finally, we are very interested at the present time in how to improve neuropsychological function at high altitude and to do this we are adding oxygen to the ventilation of the rooms of people who are working at high altitude.<sup>35</sup> For example, there is a group from the California Institute of Technology, who have put in a radio telescope at an altitude of 5050 m (about 16 600 ft) in north Chile. Now, at that altitude you are certainly impaired. The workers live in four containers like those on building sites, the kind that are used for shipping, and we have raised the oxygen concentration in the containers to 27 per cent, as opposed to the 21 per cent at sea level. The result is a dramatic improvement in efficiency, a tremendous reduction in the amount of fatigue, and they find they can work much more effectively. What we are doing in fact is reducing what we call the equivalent altitude, that is, the altitude for the same inspired  $PO_2$  during air breathing. We are reducing that to 3200 m from about 5050 m. Actually I was just contacted the other day by a group from Cornell University and the University of Texas at Austin and they want to put in a radio telescope at 5800 m (that's 19 000 ft). Now there's not a hope that you can operate at those altitudes without oxygen enrichment, and I think this oxygen enrichment of room air is going to be the secret to working at high altitude. All you need is some electrical power to generate the oxygen using oxygen concentrators. It's interesting that the whole thing started, as far as I am concerned, from Cambridge, not from the psychologists, but from a physiologist, Joseph Barcroft.

I was just going to mention very briefly another interesting fact here that you may not be aware of – that there are high mines in Chile at altitudes of about 4600 m, that is 15 000 ft or so. The miners live at sea level and they commute up to the mine. They spend seven days there and then they take a bus down to sea level, where their families live and spend seven days there.<sup>36</sup> So they oscillate between these two altitudes like yoyos. Of course they

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<sup>34</sup> Hornbein T F, Townes B D, Schoene R B, Sutton J R, Houston C S. (1989) The cost to the central nervous system of climbing to extremely high altitude. *New England Journal of Medicine* **321**: 1714–1719.

<sup>35</sup> West J B. (1995) Oxygen enrichment of room air to relieve the hypoxia of high altitude. *Respiratory Physiology* **99**: 225–232.

<sup>36</sup> Richalet J-P, Vargas Donoso M, Jiménez D, Antezana A-M, Hudson C, Cortès G, Osorio J, León A. (2002) Chilean miners commuting from sea level to 4500 m; a prospective study. *High Altitude Medicine and Biology* **3**: 159–166.

are not acclimatized to any altitude particularly, and there's a great deal of interest in their neuropsychological function. They are impaired, of course, at high altitude, but it's another example of an innovative way of dealing with high altitude.

**Professor Robert Logie:** I was at the APU during the 1980s, and to follow on from some of the comments just made about high altitude, one of the topics which I worked on was the impact of the hyperbaric environment, which was simulated. So rather than going off on expeditions up mountains and under the sea, this was a laboratory with a simulated high-pressure environment with quite interesting studies looking at the impact of nitrogen and oxyhelium on cognitive performance.<sup>37</sup> One of these studies was following up on a previous report, suggesting that while nitrogen doesn't seem to have any effects at atmospheric pressure, it has rather interesting effects at increased pressure, causing drunkenness and so on. There was one paper that we followed up which suggested that perhaps the nitrogen in air at atmospheric pressure was having similar sorts of effects, and our hypothesis was that if you give people an oxyhelium environment without the nitrogen from air at atmospheric pressure, then perhaps their performance could improve.<sup>38</sup> Perhaps this was a sort of divine method of control of humankind, by introducing nitrogen into the air, this would keep us in a slightly soporific state. We could perhaps investigate this by testing people without nitrogen in the air and seeing what happens to their performance. With the great expectation that was involved in these experiments, sadly, yes, there were performance improvements, but they were all due to practice effects on the tests. So this hope that if we were to walk around breathing oxyhelium instead of breathing nitrogen in air, then perhaps we would all instantly be much better at cognitive tests, and much better at exams and so on – all that sort of faded away, and it was rather disappointing, but an interesting story none the less. The other impact of nitrogen in those experiments was a particular idea that suggested that under very great pressure, at great depth, there are all sorts of problems about compressing people to great pressures, and introducing a small amount of nitrogen into a mixture of oxygen and helium would alleviate some of the problems of a hyperbaric environment.<sup>39</sup> I went along with the cognitive tests to have a look at what happens to people when you put them in these very high-pressure environments. Unlike many other very subtle effects that there are in the changes in these environments, at about 420 m under this new regime, not only was there very poor performance in the tests, in fact the people who were

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<sup>37</sup> Logie R H, Baddeley A D. (1985) Cognitive performance during simulated deep-sea diving. *Ergonomics* **28**: 731–746.

<sup>38</sup> Winter P M, Bruce D L, Back M J, Jay G W, Eger E I. (1975) The anaesthetic effect of air at atmospheric pressure. *Anaesthesiology* **42**: 658–661.

<sup>39</sup> Logie R H, Baddeley A D. (1983) A trimix saturation dive to 660 meters: studies of cognitive performance, mood and sleep quality. *Ergonomics* **26**: 359–374.

participating couldn't even concentrate long enough to find the pencils, far less undertake the cognitive tests. So those were two occasions when some of the suggestions about nitrogen having other beneficial or non-beneficial effects on cognitive performance turned out to be sadly disappointing. But they were interesting findings nevertheless.

**Professor Alan Baddeley:** Well, simply to point out a change in focus around the 1970s at the unit [APU], from being essentially concerned with normal people under abnormal environments, to being concerned with people who had neuropsychological problems as a result of brain damage; the way in which that happened is perhaps somewhat interesting. As a new director I had proposed when being interviewed that the unit would look at cognitive skills that were of educational interest, basically reading, writing and arithmetic. I was required to produce a programme for the Neuroscience Board, which they promptly turned down on the grounds that this was not a Medical Research Council area at all, this was the Economic and Social Research Council (ESRC) or the Social Science Research Council (SSRC) in those days,<sup>40</sup> so I was told to go away and think of something else. I was quite interested in neuropsychology, having been introduced by Elizabeth Warrington to the theoretical power of the neuropsychological study of patients.<sup>41</sup> It wouldn't have been a plausible path to choose to place before the board initially, because there weren't very many suitable patients in Cambridge, and indeed Oliver Zangwill, who had pursued this for many years had to go to London to find his patients. So I suspect that if I had started with this proposal, it would have been turned down. In fact the board agreed and we all started to gradually look at neuropsychological patients as a possible way forward. That has continued and I am happy to say I think it is still continuing strongly.

**Gregory:** Thank you very much, Alan. Of course another factor that was looked at surely was alcohol. There were quite a lot of experiments on driving and the effects of alcohol, driving around Bourne airfield with loads of whisky. There was one police driving instructor who crashed the car in the middle of the airfield, because he went round too fast and turned the car over. Disturbing. There were quite a number of studies like that, weren't there? Another lot of studies surely were how much information people should have,<sup>42</sup> like in the military situation, they kept most of the information away from the captain in a ship, because he got overloaded. Information was filtered by his underlings, rightly or wrongly, to protect, if you like, the decision-making of the chap on top. That was an interesting area I thought. And vigilance was another, wasn't it? After about 20 minutes the radar sonar

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<sup>40</sup> The SSRC was founded under a Royal Charter in 1965 and has been known as the ESRC since 1983.

<sup>41</sup> Baddeley A D, Warrington E K. (1970) Amnesia and the distinction between long- and short-term memory. *Journal of Verbal Learning and Verbal Behaviour* 9: 176–189. *idem* (1973) Memory coding and amnesia. *Neuropsychologia* 11: 159–165.

<sup>42</sup> For example, see Broadbent D E. (1964) Vigilance. *British Medical Bulletin* 26: 17–20. (APU 479)



operators lost their attention, even in battle conditions, when their ship was under threat; it was absolutely extraordinary. They just missed signals. All sorts of work like that, done at the APU surely at that time, was linked to neuropsychology. I think we should move on to the next topic, which is human–computer interaction (HCI), and Phil Barnard is going to lead us into this one.

**Dr Philip Barnard:** Certainly for me one of the most engaging features of working at the APU was that it was simply an environment that was full of a lot of interesting people who had what might best be described as intellectual ‘attitudes’. There were lots of intellects and lots of different attitudes, because it produced this sort of enormously productive ferment of ideas that pushed you forward. Once, probably more than 15 years ago, we had a series of seminars on ‘how to do applied psychology’, in which probably ten members of staff spoke. They all came up with different targets, different methods, and different models for pursuing applied research, and much of the research on HCI was cast in that mould.

Human–computer interaction wasn’t a topic as such; it was a lot of different individuals acting as a collective chasing a wide variety of problems across different cognitive domains. Many of the initial problems were recognized by people like John Morton, John Martin and Pat Wright, when they identified some of the problems of computerizing medical record systems, form filling and complex documentation that comes with computers.<sup>43</sup> Major human factors problems posed interesting cognitive challenges. In subsequent years, from 25 or 30 years of doing research on HCI, two things became very clear. First of all, this kind of technology was not really like the technologies that we had before. It wasn’t like vigilance in sonar monitoring and it wasn’t really like acoustic confusions in short-term memory, because all of the different cognitive skills needed to be brought to bear in this complex information environment. We were really faced with the challenge of figuring out how it was that you would integrate different cognitive capabilities, like memory, problem solving, language and so on. And we were faced with an additional problem that computers were not like naval destroyers, which last on station for 20 years. You were lucky if a particular generation of technology lasted 20 months. So, in the time from planning a research project, through to delivering it and publishing it, the landscape into which you were delivering your results was probably completely different in design terms from the landscape where you started it. So unsurprisingly, it wasn’t the next particular issue on which we all focused, but rather trying to develop methods, tools and techniques for studying cognition in integrated environments.<sup>44</sup>

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<sup>43</sup> Wright P. (1988) Issues of content and presentation in document design, in Helander M. (ed.) *Handbook of Human–Computer Interaction*. Amsterdam: North-Holland, 629–652.

<sup>44</sup> Landauer T. K. (1988) Research methods in human–computer interaction, in Helander M. (ed.), note 43, 905–928.

A lot of the research tracked through all of the generations of computer technology, from the early programming and command languages through manual systems, iconic interfaces, auditory interfaces, and all the way up to modern multimodal interfaces and virtual environments. There are lots of little things that came out in the research – things like, for example, if people performed well in a particular situation, they almost inevitably preferred doing it in a different, less efficient way, and that would come out in different environments. If you asked them to name their computer files, they would very often adopt naming schemes that they were subsequently hopeless at remembering. Getting them to adopt more systematic and consistent schemes was obviously really quite difficult.<sup>45</sup> The work was done under a huge number of different projects; through the Alvey initiative,<sup>46</sup> the Joint Council's Initiative,<sup>47</sup> and the ESPRIT programme,<sup>48</sup> covering different topics. It was highly interdisciplinary work, covering computer science, interaction design and we worked with lots of different industries, the defence agency on various aspects of computerization in the military. We also worked with Alcatel, Xerox, IBM and many of the major computer manufacturers. After a lot of work, a lot of results, and a lot of experiments, the big question went something like this, for my money: What did it achieve? Did it have an impact in the applied environment? Would it have an impact on cognitive psychology in general? I think the jury has to be out on those instances. It's certainly true that over the course of 30 years of research a huge number of papers were produced. If you look in the bibliography of HCI research<sup>49</sup> you'll see that APU authors are

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<sup>45</sup> Carroll J M. (1982) Creative names for personal files in an interactive computing environment. *International Journal of Man-Machine Studies* 16: 405–438.

<sup>46</sup> The Alvey Initiative was a collaborative research and development programme funded by the Science and Engineering Research Council (SERC), the Ministry of Defence (MOD) and the Department of Trade and Industry (DTI). See Alvey Committee. (1982) *A Programme for Advanced Information Technology: The report of the Alvey Committee*. London: HMSO. See also Oakley B, Owen K. (1990) *Alvey: Britain's strategic computing initiative*. Cambridge, MA: MIT Press.

<sup>47</sup> From 1989 until 1996 three of the UK Research Councils – the ESRC, the MRC and the SERC (later renamed Engineering and Physical Sciences Research Council) – ran a special Joint Council Initiative (JCI) in 'Cognitive Science and Human-Computer Interaction' with a budget of £12 million. As part of the initiative, an evaluation was commissioned and the final evaluation report is available, with appendices, including a report from an international Peer Review Panel (David Rumelhart, John Carroll, Clayton Lewis, William Newman and Mark Steedman). See [phoenix.herts.ac.uk/pub/R.M.Young/jci-evaluation/intro.html](http://phoenix.herts.ac.uk/pub/R.M.Young/jci-evaluation/intro.html) (visited 16 December 2002).

<sup>48</sup> Barnard P J, Harrison M D. (eds) (1991) *Assimilating Models of Designers, Users and Systems (AMODEUS): Periodic progress report no. 2, and associated deliverables*. Brussels: CEC. (APU 2755) MacLean A, Young R M, Bellotti V M E, Moran T P. (1992) Design space analysis: Bridging from theory to practice via design rationale, in *Proceedings of Esprit Conference 1991* (Brussels, 25–29 November 1991). Brussels: CEC, 720–730. (APU 2692)

<sup>49</sup> The HCI Bibliography first appeared on the Internet in 1991 and on the World Wide Web in 1998. Gary Perlman, director of the project, organized the data with funding for the hardware and software infrastructure from ACM SIGCHI, Apple and Ohio State University. Processing of data has attracted no outside financial support. See [www.hcibib.org](http://www.hcibib.org) for further details (visited 28 November 2002).

well represented in the list of most frequently published authors. Textbooks, compendiums, and handbooks contained a disproportionate representation of APU authors, compared with the overall amount of funding available to unit. Whereas IBM and other institutes very often had huge technological resources behind them, we were very often doing research with BBC micros<sup>50</sup> and simulating displays using slides. In terms of what we actually produced (and probably not following our best advice on abbreviation and naming things), after 25 years we produced a large range of different modelling techniques, notations, and design tools. Unfortunately they have abbreviations of the sort that we ourselves despised, like TAG, CTMs, PUMs, TPDs and DRs, which decoded as task action grammars, cognitive task models, programmable user models, transition path diagrams and design rationales, for example. On top of that we produced mathematical models, using funny names like 'syndetic' models and 'interaction' models. All of these things are out there, open for display, and by and large the product of differentiated people with intellectual attitude. It's very difficult to assess, I think, the extent to which psychology gained from all of those different labels. There are understandable doubts being expressed as to whether or not a lot of research on HCI produced a major impact on the discipline. It wasn't like acoustic confusions leading to theories of short-term memory.

I think one of the big problems for a lot of us working in the area was the tension between trying to be relevant to current problems in interface design, which were changing at an extraordinary rate, and trying to generate good theory that develops only very slowly. I suppose it's just like the classic story about the race between the hare and tortoise. A lot of the HCI research work was guided in the short term by choosing problems closely coupled to current interface designs. This made the work relevant to current design practice as it moved rapidly forward, but may not have helped to demonstrate long-term theoretical coherence and progress. But having chased that hare for a long time, I think we need to watch out for the tortoise, a theoretical idea originating in HCI research but whose long-term theoretical potential is only recognized later. One good example, although not from APU research, came from work at Bell Labs, where 20 years ago they started studying information retrieval problems with computers. All of the initial research was basically done on how people used names to describe things. It also involved some clever mathematics applied to how word use is distributed across different contexts. It explored how such mathematics might help people to recover from computer databases the information they really needed as opposed to a whole lot of stuff not relevant to their current concerns. The initial research was on practical problems of information retrieval and human interfaces to

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<sup>50</sup> The BBC released the model A micro with their computer literacy drive in late 1981, which was an 8-bit 6502 with 16K RAM, manufactured by Acorn Computers. Model B, released at the same time, had more memory, 32K RAM, for an extra £100 at £399.

databases.<sup>51</sup> Fifteen years later the real theoretical potential of the conceptual approach, latent semantic analysis, appeared in *Psychological Review* as a possible associative theory for long-term memory mechanisms.<sup>52</sup> Hopefully some of the other research on HCI may also eventually gain more recognition in the domain of mainstream psychological theory.

**Gregory:** I wonder if I can ask you a question? Do the computers affect children, when children have computers in schools? Does that affect their cognitive processing?

**Barnard:** Graham is probably better able to answer that than I.

**Professor Graham Hitch:** I was at the APU in the 1970s, but my work on computers in schools wasn't done at the APU. Enormous claims have been made about the effects of computers on cognitive development in children, but the last time I looked at it, the claims were very much over-inflated in relation to anything you could objectively measure. I think there has been a lot of hype in that area, although my knowledge is now about ten years out of date.

**Gregory:** A very interesting issue. Now who would like to contribute on this? It really is a fascinating area, don't you think? Has anybody else worked on it?

**Duncan:** Can I make a comment from the point of view of psychometric testing? I did my first selection of computer programmers in 1970, but of course the nature of the demand has changed, and has had to change. One of the things we used to say was, 'Well, this is a high-level, non-verbal, non-numerical operation, so we will just use a pure g test<sup>53</sup> as the nearest approximation'. But then we found that there was significant difference between good programmers and poor ones, not accounted for by g, and it led the psychometricians to develop rule-following tests. The point about programming is that it requires the operator to follow often quite a complex series of rules, which a lateral thinker is poorly

<sup>51</sup> Dumais S T, Landauer T K. (1983) Using examples to describe categories, in *Proceedings of CHI 1983 Human Factors in Computing System*, Boston, 12–15 December 1983. New York: ACM, 112–115.

<sup>52</sup> Landauer T K, Dumais S T. (1997) A solution to Plato's problem: the latent semantic analysis theory of acquisition, induction and representation of knowledge. *Psychological Review* **104**: 211–240.

<sup>53</sup> Mr David Duncan wrote: 'Charles Spearman (1863–1945) was Reader in Experimental Psychology at University College London from 1904 and Professor from 1911–31. His formulation of the two-factor theory of intelligence in 1904 was a central influence in setting psychometric testing in Britain on a very different path from that in America. His successors at UCL, Cyril Burt (1883–1971) and Philip Vernon (1905–87), developed the hierarchical theory of abilities, which led in practice to short test batteries centred on pure g intelligence, which throughout the occupational field is of great importance. Pure g is always expressed in lower case, in spite of a possible confusion with g for gravity force. See Spearman C E. (1904) General intelligence objectively determined and measured. *American Journal of Psychology* **15**: 201–213. Vernon P E. (1950) *The Structure of Human Abilities*. London: Methuen.' Note on draft manuscript, 19 November 2002.



Figure 12: 'Heat, noise, sleep deprivation and other "stressors" were of interest because of their effects on human performance in military contexts, and vigilance tasks were studied because they simulated prolonged periods spent monitoring sonar displays for infrequent target signals. These tasks and stressors were also relevant to human performance in industrial contexts.' Professor Graham Hitch, 4 December 2002.

equipped to negotiate. I speak as one who is no good on computers at all because of this; one knows the rules, but one keeps forgetting them. The rule-following tests for computer programmers have been really very successful in practice. I think Peter Saville and Roger Holdsworth were the people who really investigated this in detail and produced some really very good solutions.<sup>54</sup>

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<sup>54</sup> Mr David Duncan wrote: 'Drs Peter Saville and Roger Holdsworth were founders of Saville and Holdsworth Limited. They were always alert to new demands from their customers, which new tests could meet. Increasing complexity of computer programming in the 1970s led Dr Peter Saville to develop the DA5 Diagramming Test, which was the first adequate test of rule following. It was published by Saville and Holdsworth in 1980. Subsequently, NFER-Nelson Ltd commissioned Dr Stephen Blinkhorn to produce a computer commands test and a computer rules test, under the title New Technology Tests, which were published in 1987.' Note on draft transcript, 19 November and 12 December 2002.

**Gregory:** It would be interesting to know if the Mac operating system was designed by a psychologist, in the sense of making it more intuitive. Does anybody know that? The APU didn't work on operating systems did it?

**Barnard:** No. We had quite a lot of contact with the people in Xerox who actually developed the precursor graphical interface design and were involved with some of the early studies of evaluating those kinds of interface in the practical domain. Psychologists at Xerox Park were actually responsible for the precursor for the WYSIWYG ['What you see is what you get', pronounced whizziwig] interface that was ultimately adopted by the Mac, although it originated in the interface style from Xerox.

**Professor Peter McLeod:** Is it not the case that Don Norman<sup>55</sup> had a major input into the design of the Apple Mac interface?

**Barnard:** He came in rather later. Don Norman as far as I know wasn't involved in the earlier generation of interfaces; he became an Apple Fellow much, much later on, in the late 1990s and went around telling them how to do it.

**Sir Christopher Booth:** Mine is a general question. I wondered if all of you who worked at the APU throughout the years could give me an idea about the work for the armed services. You have talked about the origins of the unit in 1944, with its commitment to doing work for the armed services. One wonders the extent to which that has continued since that time. I was Chairman of the Royal Naval Personnel Research Committee for some years, which is a committee of the Medical Research Council. We always had a sub-committee of that group working on human factors, which was deeply involved with some work that was done at the APU. The question that really interests me is the extent to which the research that was carried out through the years was in fact contracted research; in other words, responding to requests for advice or was it 'blue skies' research that you were doing, because you wanted to do it yourselves. Can you give me some idea as to the extent to which it was applied or basic?

**Baddeley:** In response to your first question, the amount of work done for the military was I think fairly substantial as late as the 1970s, but had already begun to taper off, partly because the funding for military work was less, partly because the problems that it threw up seemed to fit less readily with the theoretical developments. Work on stress, for example, was giving way to work on things like HCI, telephony and so forth.<sup>56</sup> In terms of the balance between how much work was commissioned and how much was self-generated, it's

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<sup>55</sup> Norman D. A. (1988) *The Psychology of Everyday Things*. New York: Basic Books.

<sup>56</sup> Broadbent D. E. (1963) Differences and interactions between stresses. *Quarterly Journal of Experimental Psychology* **15**: 205–211.

quite difficult to draw a clear line. At least it was in the 1970s and 1980s, because to some extent one would choose a problem for both its practical interest and potential theoretical interest, and then perhaps the interests of a possible sponsor. There were other occasions when the unit was requested to provide advice, for example an inquiry into the issue of whether financial trials were too complex for juries, where there was a very clearly specified remit.<sup>57</sup> A whole range of people did some work on that. I think it was really a mix of the two, but the military involvement became less, particularly when we ceased to have naval ratings as subjects.<sup>58</sup> The Navy was finding it harder to recruit people and eventually they asked if we could manage without. By then, fortunately, I think it was Conrad who had set up a subject panel,<sup>59</sup> which turned out to be much more effective and easier to use than the rather limited number of sailors.

**Gregory:** Maybe we should move on to the next thing, unless anybody has got something burning to say.

**Hitch:** Just a quick comment really. When I first went to the APU, I was a lowly research student and I was amazed to find that there was some sort of naval unit in Cambridge. Its sole purpose, as far as I could work out, was to provide, I think, 6 naval ratings per fortnight, to be tested at the unit. Whilst a large amount of their time was spent 'cooking' them<sup>60</sup> to see how well they performed when they were hot, or locking them in a room for hours on end and having them perform vigilance tasks,<sup>61</sup> there was plenty of spare time left for people

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<sup>57</sup> See Logie R H, Duncan J, Baddeley A D. (1986) Difficulties with comprehension, memory and concentration in listening to complex information, in Lord Chancellor's Department. *Report of the Fraud Trials Committee*. London: HMSO. See section, 'Improving the presentation of information to juries in fraud trials', 41–51. (APU 1857). Lord Roskill chaired the committee from 1983–86. Its main recommendation was to set up a new unified organization responsible for the detection, investigation and prosecution of serious fraud cases.

<sup>58</sup> The first experiments using naval ratings at the APU were carried out in 1961. They were tested for approximately two hours per day and received an hourly honorarium of 7/6 [35 1/2p]. Additional information from Jackie Harper, Panel Manager, CBU, e-mail, 11 December 2002.

<sup>59</sup> The volunteer panel to test members of the public was set up in 1963 and formal testing began in 1964. At the time this was unique in the UK. There were 1983 active panel members at the end of 2002. Volunteers receive an information sheet every 2–3 years. See recent *Panel News* on [www.mrc-cbu.cam.ac.uk/Common/PanelNews/panelnews.shtml](http://www.mrc-cbu.cam.ac.uk/Common/PanelNews/panelnews.shtml) (visited 6 November 2002). For another example of use of subjects, see Tansey E M, Reynolds L A. (eds) (1998) *The MRC Common Cold Unit*, in Tansey E M, Christie D A, Reynolds L A. (eds) *Wellcome Witnesses to Twentieth Century Medicine*, vol. 2. London: Wellcome Trust, 209–268.

<sup>60</sup> See Figure 12. Poulton E C, Edwards R S, Colquhoun W P. (1974) The interaction of the loss of a night's sleep with mild heat: task variables. *Ergonomics* 17: 59–73.

<sup>61</sup> Colquhoun W P, Edwards R S. (1970) Practice effects on a visual vigilance task with and without search. *Human Factors* 12: 537–545.

like me. I was doing a PhD which had no military application, but was told that subjects were available and was asked whether I would like to use them. I think quite a large proportion of their time was used on non-military research while I was there. I wouldn't like to put a percentage on it, but it was certainly an important use of this resource.

**Gregory:** Of course, the Americans financed a tremendous lot of theoretical European research from their military budget, in order to get it through, and a lot of Cambridge fundamental research was financed by the American Naval Office in Brussels.<sup>62</sup> There was a very good link between theory and applied research. Now I think we should move on to human factors. Ivan Brown is going to introduce this.

**Dr Ivan Brown:** Could I just pop in an amusing incident here, because I managed the subject panel for some time, and I remember early on that there was a time when we tested both naval ratings and Cambridge housewives. I knew someone who lived across the road from the building we were in and he would leave in the morning as naval ratings came to get tested and, when he came home for lunch, they were apparently walking out as Cambridge housewives, and he wondered what on earth our research was doing to them.

I had served in the Royal Engineers and I had worked for several years at the Cambridge Instrument Company, so I suppose it was fairly natural that I fell into human factors research when I joined the APU in 1953, although human factors might not have been the term used at the time. In the States it was probably called engineering psychology; it later became human factors and in Europe it tended to be called ergonomics.<sup>63</sup> I think most people use the terms interchangeably now because they deal with all kinds of interactions between people, work and technological systems, although some people prefer human factors to concern the more cognitive issues and ergonomics the more basic physical issues. Anyway, I had a look through some of the early reports of the first 30 years of APU research before I came here, and it was fairly clear that the early human factors work concentrated

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<sup>62</sup> Professor Richard Gregory wrote: 'There was a great deal of fundamental science financed by the Americans – from their military budget (it was a way of getting the research money through their Government). I think it was mainly the Navy but also the Air Force. I don't remember the Army coming into it. I had several research grants through this, and so did Larry Weiskrantz (now Emeritus Professor of Psychology at Oxford). He worked on monkey brains, I think with absolutely no military significance. My work was related, as it concerned the moon landing, and also photographing the moon through a system reducing loss of image quality through turbulence of the atmosphere, though this may have been funded separately: it was tested with the USAF in New Mexico and Arizona. These were just two examples: a great deal of basic research was financed and it really got university research going after the war. It was very important.' E-mail to Mrs Lois Reynolds, 28 February 2003. See, for example, Weiskrantz L, Cowey A. (1963) Striate cortex lesions and visual acuity of the Rhesus monkey. *Journal of Comparative and Physiological Psychology* 56: 225–231.

<sup>63</sup> See Osborne D J. (1991) Tipping the balance towards ergonomics, in Lovesey E J. (ed.) *Contemporary Ergonomics*. London: Taylor and Francis, 2–12.



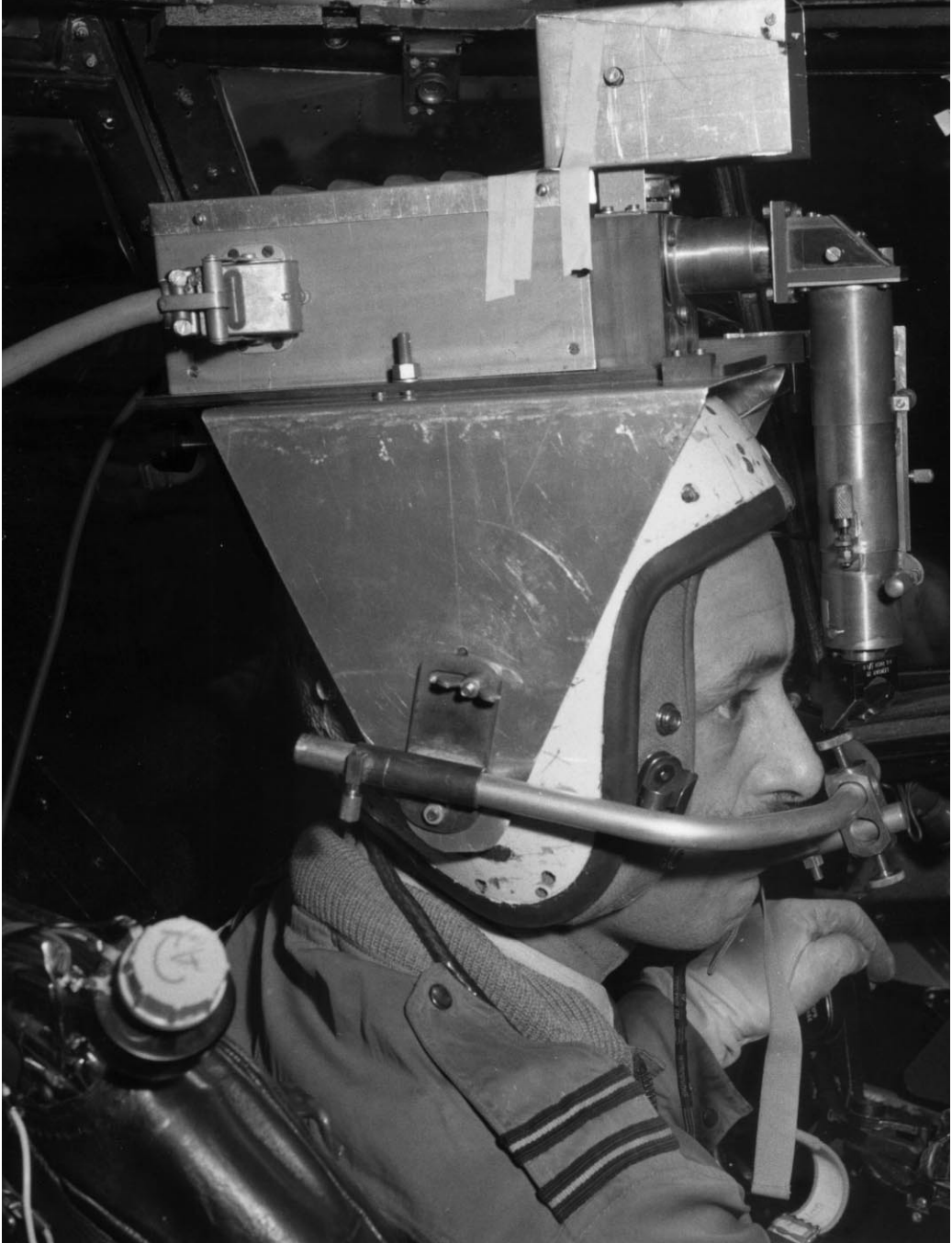


Figure 13: Head-mounted device for recording pilots' eye movements, c. 1960.



Figure 14: Instrumented car used by Dr Ivan Brown to measure drivers' spare mental capacity, c. 1965.

on these interactions between people and informational systems at the sensory and perceptual level. I was rather surprised, for example, to find that Kenneth Craik was researching the visibility of radar echoes and the design of instrument knobs before his untimely death,<sup>64</sup> and William Hick was researching the effects of friction and muscular forces required in manual controls such as handwheels. Even in the late 1950s, I suppose, when I started doing some human factors research, I was working on the discriminability of visual signals used in air-sea rescue operations. I believe we worked on some of the first studies of flashing lights as turning indicators for cars.<sup>65</sup> These issues seem very mundane

<sup>64</sup> Craik K J W. (1945) *A Note on the Design and Manipulation of Instrument Knobs*. MRC Applied Psychology Unit Report no.14. See also note 17.

<sup>65</sup> Baker C H, Brown I D. (1956) *Relative Conspicuity of the Royal Naval Raft Light when Steady and Flashing*. MRC Royal Naval Personnel Research Committee Report no. 56/877-SS83. Gibbs C B, Brown I D. (1958) *Flashing versus Steady Lights as Car Turning Signals: The effects of frequency and duration of flash*. MRC Applied Psychology Unit Report no. 245.

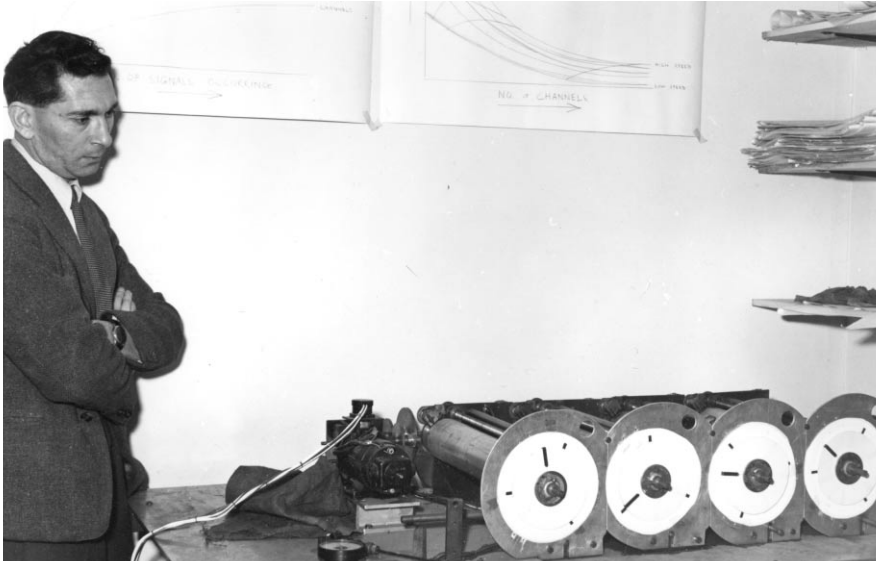


Figure 15: The equipment was used in Conrad's PhD work, in which subjects had to monitor various numbers of dials, which had pointers continuously moving at different rates. Subjects responded when each pointer crossed a mark. When a dial reached 12 o'clock it stopped and had to be re-started. Professor Alan Baddeley, 4 December 2002.



Figure 16: In the early 1960s mechanization of letter-sorting envisaged a keyboard operator copying postcodes to enable mail to be sorted electronically, as illustrated above. I was engaged by the Post Office in research concerned with alphanumeric keyboards. Dr R. Conrad, 10 February 2003.

now, but of course they were necessary at the time, because engineers tended not to give too much attention to the interface problems of the systems they were designing. To be fair, the amount of data they had available on human capabilities and limitations was extremely limited until the APU and other people got going.<sup>66</sup> Well, obviously as time went on the human factors issues that the APU staff were researching got more and more complex, and they tended early on to come from the armed services, via various personnel research committees, presumably because the armed services had the money to buy more sophisticated systems, and they presented more interesting problems for applied psychologists. For example, I recall in around 1960 working on some of the first head-up displays for fighter aircraft, when it became obvious that low-flying pilots had very little chance to look inside the cockpit at a time when they were too busy looking where they were going.<sup>67</sup> There is an amusing incident there. I think the work we were doing was related to Mackworth's research on eye movement recording, using head-mounted devices for recording where people were looking at all times.<sup>68</sup> I think he had established contact with one of the directors of Smith's Instruments, who were producing some of the technology, and it's interesting that one of the devices we compared with others was based on what we called streaming lights. This came about because the director was driving home from a dinner where the wine had been served rather generously and, as he drove down the middle carriageway of a three-lane road, he realized that as he weaved off to one side, reflections from the two lines of cat's eyes streamed differentially, and all he had to do was to keep the two lines streaming at the same rate and he would get home safely. At least until he got off the cats'-eyed carriageway. That's one of the few times when intoxication has helped in the design of rather sophisticated equipment. And then with the invention of night-vision devices, I found myself involved in the design of visual displays for use in the remote control of, say, shut-down army tanks, or the remote control of other military vehicles.<sup>69</sup> At the same time several of the unit staff, obviously Christopher Poulton, Brian Shackel and Max Hammerton, were researching the desirable characteristics of manual control systems, particularly for use in moving platforms such as aircraft and ships, but also looking at small

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<sup>66</sup> Brown I D, Batts V, McGougan C E. (1970) Ergonomics in Action. I: The work of the Medical Research Council Applied Psychology Unit. *Applied Ergonomics* 1: 169–176. See also Poulton E C. (1964), note 16.

<sup>67</sup> Brown I D, Holmqvist S D, Woodhouse M C. (1961) A laboratory comparison of tracking with four flight-director displays. *Ergonomics* 4: 229–251.

<sup>68</sup> Mackworth N H, Kaplan I T, Metlay W. (1964) Eye movements during vigilance. *Perceptual and Motor Skills* 18: 397–402. See Figure 13, page 32.

<sup>69</sup> Brown I D, McFaddon S M. (1986) Display parameters for driver control of vehicles using indirect viewing, in Gale A G, Freeman M H, Haslegrave C M, Smith P, Taylor S P. (eds) *Vision in Vehicles*. Amsterdam: North-Holland, 265–274. Cats' eyes were invented by a businessman from Halifax by the name of Percy Shaw in 1934. They were first used on British roads in 1935.

hand-held systems for the control of guided missiles.<sup>70</sup> As we have heard I think in time the majority of the APU human factors problems were coming from civilian sources, partly because of very large organizations recognizing the importance of fitting their equipment, or their products, to the people who were going to use them, rather than hoping for the best. Very early on, I think I inherited this human factors job from Christopher Poulton. We were advising and carrying out research for the Ministry of Transport and the Road Research Laboratory, as those two organizations were called then, and a lot of that work was concerned with accident reduction and prevention, and the need to understand a bit more about human capabilities and limitations.<sup>71</sup> I think it was largely driven by the theoretical interest within the unit in information processing.

Moving on a bit, in 1986–87 several members of the unit were very much involved in launching a programme of research on cognitive issues influencing driver behaviour. This programme followed a plan outlined by Donald Broadbent, after a seminar held near Oxford in 1986.<sup>72</sup> The aim was to draw in experimental psychologists, from the universities, who had probably not thought of, or had no intention of working on road safety issues, but were doing various sorts of things that would provide the information that the Department of Transport needed. That research programme actually continues to the present day and some people here, John Groeger, for example, have been very much involved in it. I still find myself the psychology consultant to the people running the programme in the Department of Transport. The unit's work has carried on for a long time in that direction and that line of research on driver behaviour and road safety led eventually, again largely by John Groeger, to our involvement in the European DRIVE programme.<sup>73</sup> I think that acronym means something like 'dedicated road infrastructure for maximizing vehicle safety in Europe', but rumour had it that if you didn't have an attractive acronym you didn't get the research

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<sup>70</sup> Poulton E C. (1963) Compensatory tracking with differentiating and integrating control systems. *Journal of Applied Psychology* 47: 398–402. Shackel B. (1954) *The Human Limbs in Control. 1. Apparatus: optimum experimental conditions*. MRC Applied Psychology Research Unit Report no. 214. Cambridge: MRC Applied Psychology Research Unit. Hammerton M, Tickner A H. (1966) An investigation into the comparative suitability of forearm, hand and thumb controls in acquisition tasks. *Ergonomics* 9: 125–130.

<sup>71</sup> Brown I D. (1972) The place of psychological research on driver behaviour, in *Proceedings of Conference on 'Medical, Human and Related Factors Causing Traffic Accidents, including Alcohol and Other Drugs'*, 30–31 May 1972, Montreal. Ottawa: Traffic Safety Research Foundation of Canada, 162–172.

<sup>72</sup> Broadbent D E. (1986) The prospects for behavioural research on road safety: Notes on the seminar of 10–11 March 1986, in *Behavioural Research in Road Safety: Background Papers and Summary of Extramural Programme*. Unpublished report by the Transport Research Laboratory, Crowthorne, Berkshire.

<sup>73</sup> Groeger J A. (1991) Acquiring and retaining driving skills, in Kuiken M J, Groeger J A. (eds) *Report on Feedback Requirements and Performance Differences of Drivers. Deliverable ADA2, DRIVE Project 1041: Generic Intelligent Driver Support Systems*. The Netherlands: Traffic Research Centre, University of Groningen, 3–14. (APU 2666)

funding. This work was applied to the development of intelligent technological systems for driver support in vehicles and traffic management on roads.

If I could just go back to 1960, I think it was largely due to Conrad's initiative that the unit started human factors research for what was then the Post Office, running both the mail business and telecommunications.<sup>74</sup> This was a good move, because I believe the Post Office was the largest employer in the country, and clearly they had lots of human factors problems in the places where the technology was being used in-house, and because most people at some stage were going to be using telephones or what have you. They also had a large variety of human factors problems to be solved in the production of the systems they were selling. I recall that one of the things that Conrad initiated was research on the human interface with letter-sorting machines in the Post Office, and with telephone switchboards.<sup>75</sup> He also initiated research on the design of postcodes and on the key pads on key phones that were replacing the old dialling systems. That consultancy arrangement, which I took over from Conrad when he moved in 1974, continued with British Telecom until it was privatized in the 1980s<sup>76</sup> and quite a number of the unit staff, Phil Barnard included, were researching various issues for British Telecom over those years.

I would like to make a couple of general points and finish with this topic. One is that I think this broad range of human factors research was obviously drawing on, but also hopefully contributing to, theory development within the unit. The other point which I think is equally important is the collaborative way in which we set up research with outside organizations such as British Telecom, British Rail, the Post Office, the Coal Board, and so on. I think these groups not only benefited from the theoretical concepts that were being researched and developed within the unit; they also benefited from the methodology that unit staff were developing. When they formed their own human factors research groups they could go away with confidence and use these techniques that they had learnt from

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<sup>74</sup> Professor Alan Baddeley wrote: 'Figure 15 shows equipment used in Conrad's PhD work. On the basis of this, he was able to advise the General Post Office (GPO) on optimal workload levels for staffing telephone exchanges, the first of many very successful studies he carried out with the Post Office (who at that time were also responsible for telephones).' E-mail to Mrs Lois Reynolds, 4 December 2002.

<sup>75</sup> Conrad R. (1956) Performance of telephone operators relative to traffic levels. *Nature* **178**: 1480–1481.

<sup>76</sup> The General Post Office was the government department responsible for both UK postal and telephone services until it became a public corporation in 1969 as Post Office Telecommunications. British Telecom became a separate corporation in 1981, and was privatized in 1984 as British Telecommunications plc (renamed BT in 1992), an entirely separate organization from the Post Office. The company was regulated by OFTEL (Office of TELEcommunications), appointed by the Government. In 2001, BT was split into two new companies, BT Group plc and mmO2, both listed on the stock market. See [www.lightstraw.co.uk/ate/tmo/tmo.html](http://www.lightstraw.co.uk/ate/tmo/tmo.html) (visited 22 October 2002).

APU staff. I think that this collaboration was largely responsible for the setting up of initially quite strong in-house human factors groups within organizations such as the Post Office, the Coal Board, and the Transport Research Laboratory.<sup>77</sup> Some of those didn't survive privatization too well, but at the time I think they benefited considerably from the input from the APU and I think this was probably one of the main contributions the unit has made to society. It was a 'giving away of psychology' and I was very privileged to be part of that; I liked the unit so much I stayed for 40 years. It wasn't a razor and I couldn't buy the company, but I did stay on.<sup>78</sup> I know I have mentioned a name or two of my colleagues who are here today, who were involved in this wide range of human factors research, and maybe they can give us some, perhaps better remembered comments than my own.

**Gregory:** Oh, admirable. You have beaten Hick's Law for the amount of information in a given time. Bless you, really splendid.

**Brown:** It didn't all come out of my brain; it may have come out of yours.

**Gregory:** It was superb. I remember, by the way, the flippers and flashing lights for car signals. As I remember it, from 40 years ago, you were comparing the flippers for cars at that time, with the use of flashing lights. I think the flipper was quicker, but they used to break off, so you had to abandon them. Isn't that right? The link with engineering was always tricky, wasn't it?

**Brown:** I think it was the flash frequency that we were looking at, because there had been some basic research showing that if you had a flash rate of about three per second, you maximized the conspicuity of the signal.<sup>79</sup> These were the lines that we were working on.<sup>80</sup>

**Gregory:** I think one of the things you didn't mention, which I am sure other people have, is negative transfer situations. There's the famous case about a mine trolley that had a brake pedal and accelerator – so the accelerator was on the right and the brake on the left for one direction of travel – but reversed on the way back – as one sat on a different seat and used the same pedals.<sup>81</sup> Believe it or not, they had an awful lot of crashes. Do you remember that?

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<sup>77</sup> Brown I D, Batts V, McGougan C E. (1970) The Medical Research Council Applied Psychology Unit. *Occupational Psychology* 44: 267–279.

<sup>78</sup> The phrase, 'I liked the Remington shaver so much that I bought the company', refers to an advertisement by Victor Kiam in the 1980s, which has been used since to advertise many other products.

<sup>79</sup> Gerathewohl S J. (1954) Conspicuity of flashing light signals of different frequency and duration. *Journal of Experimental Psychology* 48: 247–251.

<sup>80</sup> Brown I D. (1955) *Signal Conspicuity and Discrimination in Sea Rescue Work*. Medical Research Council, Royal Navy Personnel Research Committee Report no. SS78.

<sup>81</sup> Dr Ivan Brown wrote: 'Negative transfer refers to the inappropriate use of previously learned control movements.' Note on draft transcript, 10 February 2003.

**Brown:** I think it was Bernard Gibbs who was working on that.<sup>82</sup> A similar situation existed in the control rooms in the electricity-generating industry, where the engineers designed the same control panel for use on opposite sides of the room, so the control display relationships were reversed and people had to remember on which side of the room they were sitting and if they were on the wrong side they had to think, 'Well, right means left'.

**Gregory:** But they looked nice, because they were mirror images.

**Brown:** Oh, they were symmetrical. Rather like all the rows of identical switches on the dashboards of cars in the early days, where you had to count along before you got to one that operated the lights.

**Gregory:** Does anybody want to add anything? I mean there's so much in your presentation. Can anybody add to it?

**Professor Pat Rabbitt:** I think Ivan [Brown] is being very modest in suggesting that the unit was more or less an applied unit and emphasizing applications over theoretical contributions. I believe that what made the APU more important than other organizations doing applied work, like Farnborough,<sup>83</sup> was the extent to which the applied work done there fed into theory. This raises a point made earlier, just to embroider on Ivan's account a little. One example is Donald Broadbent's earliest and most famous work on dichotic listening, which led to his model of selective attention, the key to the thesis of his seminal book.<sup>84</sup> Donald's work on dichotic listening was a direct result of his applied work, finding ways to facilitate pilots in distinguishing between communications from ground control and from the other aircraft. This example also illustrates the extent of collaboration and of common interest among members of the APU at that time, because while Christopher Poulton actually was the first to publish the applied work on binaural listening and to point out that in cases of such difficulties humans find it easier to unscramble, to appropriately select between, and to remember two simultaneous messages if they are each presented to a different ear. It was Donald who pointed out the theoretical significance of this discovery and started a line of research on attention that is still current.

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<sup>82</sup> Several people, including Professor Richard Gregory, Professor John Senders and Professor Patrick Foley have confirmed this arrangement, but have been unable to recall where the details were published. E-mails to Mrs Lois Reynolds, 27 February and 10 March 2003.

<sup>83</sup> The psychologists of the RAF Institute of Aviation Medicine and the Defence Research Establishment at Farnborough conducted a wide range of research under the direction of Air Marshal William Stewart, with John Rolfe leading the flight simulation work. Alan Burrows, who was a student of Stewart, later headed a human factors unit at Douglas Aircraft in Long Beach, California. See John Rolfe's history of flight simulators, including the Cambridge cockpit, at [www.raes.org.uk/fl-sim/FSG%20Cambridge%20Cockpit.htm](http://www.raes.org.uk/fl-sim/FSG%20Cambridge%20Cockpit.htm) (visited 9 January 2003).

<sup>84</sup> See note 22.



The interplay between applications and theoretical advances is also apparent in research on car driving, directed by Ivan and carried out by John Duncan. John took paradigms for testing selective attention in driving that Ivan had developed, monitoring drivers' attention to different information sources such as rear-view mirrors, instrument panel and the road ahead. He found that the extent to which drivers succeeded in appropriately distributing their attention between these sources varied directly with their performance on classic pencil and paper intelligence tests, such as the Cattell and Cattell 'Culture Fair' test.<sup>85</sup> This work was seminal in developing the first useful models for individual differences in attentional selectivity. One may argue that it began a thread in John's work that led directly to his current brilliant work on the relationships between attention, intelligence, and the functional neurophysiology of the pre-frontal cortex at the APU's successor unit, the Brain and Cognition Research Unit.<sup>86</sup> The practical aspect of John's work, a potential contribution to selection of more able drivers and an actual contribution to the understanding of road traffic behaviour is, in my view, neither incidental nor a less valuable outcome.<sup>87</sup>

A second illustration of how excellent applied work led directly to important theoretical advances comes from research that Conrad did for the UK Post Office, as it was in the 1960s and early 1970s. For example, it was in response to a request from the Post Office to find telephone dialling codes that created as few user confusions as possible that Conrad explored the errors that people made when trying to remember successive strings of digits. He made the theoretically valuable discovery that many errors were transpositions of digits within a string, or migrations of digits between strings.<sup>88</sup> Conrad's further explorations of the advantages of presenting telephone numbers as groups of digits rather than as continuous strings raised important theoretical issues about the ways in which ordered sequences of

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<sup>85</sup> Cattell R B, Cattell A K S. (1960) *Handbook for the Individual or Group Culture Fair Intelligence Test*. Champaign, IL: Institute for Personality and Ability Testing. See also McKenna F P, Duncan J, Brown I D. (1986) Cognitive abilities and safety on the road: A re-examination of individual differences in dichotic listening and search for embedded figures. *Ergonomics* **29**: 649–663. (APU 1848) Duncan J. (1993) Selection of visual information in the control of behaviour, in Baddeley A D, Weiskrantz L. (eds.) *Attention: Selection, Awareness and Control, A Tribute to Donald Broadbent*. Oxford: Clarendon Press, 53–71. (APU 2743)

<sup>86</sup> Everling S, Tinsley C J, Gaffan D, Duncan J. (2002) Filtering of neural signals by focused attention in the monkey prefrontal cortex. *Nature Neuroscience* **5**: 671–676. Duncan J. (1986) Disorganization of behaviour after frontal lobe damage. *Cognitive Neuropsychology* **3**: 271–290.

<sup>87</sup> Duncan J, Williams P, Brown I D. (1991) Components of driving skill: experience does not mean expertise. *Ergonomics* **34**: 919–937.

<sup>88</sup> Conrad R, Hille B A. (1957) Memory for long telephone numbers. *Post Office Telecommunications Journal* **10**: 37–39. (APU 322) Conrad R. (1958) Accuracy of recall using keyset and telephone dial and the effect of a prefix digit. *Journal of Applied Psychology* **42**: 285–288. *idem* (1960) Experimental psychology in the field of telecommunications. *Ergonomics* **3**: 289–295.

events are encoded in the brain. These intriguing findings have only recently been taken up again by memory theorists. As an extension of this work, Conrad found that mixed strings of letters and digits were much better remembered if the letters and digits were given in separate groups than if they were mixed for presentation in random temporal order. As a further extension of this work he showed that groups of letters plus groups of digits were much better remembered than were equivalent groups of letters alone or of digits alone. Finally, as part of the explanation for this Conrad found that items were better remembered if they were acoustically distinct than if they were acoustically confusable.<sup>89</sup> This demonstration that the mutual acoustic confusability of items strongly affected the success with which they could be recalled made the valuable point that items are, at least initially, stored in the functional memory system as representations of sounds rather than as representations of visual symbols or in some more abstract format. It was not only an important contribution in its own right, but led directly to Alan Baddeley's development of models for phonemic encoding of material in what he termed 'working memory' and to what is now a flourishing industry of theoretical working memory studies.<sup>90</sup>

Other spin-offs from Conrad's work for the Post Office was the research that I directed, showing that high levels of crackle or of random noise on telephone lines not only had the obvious effect that it made some messages harder to understand, but had the more subtle consequence that even messages that were perfectly understood and correctly repeated were less well remembered, particularly if they were followed by further difficult-to-hear material.<sup>91</sup> This demonstration that effort put into making out words that are degraded by noise detracts from the mental resources necessary to remember them is of theoretical significance but, in practical terms, has also led to a better understanding of the problems faced by older people with hearing impairments.

I have quoted just a few examples, but I think if one took almost any project run at the APU between 1950 and 1996, it is possible to show two separate benefits, of which the solution of an applied problem was only one. The other, and the more important benefits as far as the development of psychology in the UK was concerned, were the theoretical advances that came out of the applied work. The key to the success of the APU was the enormous ingenuity and effort that was put in to make the outcomes of projects useful to those who had commissioned them, but also to the academic community at large.

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<sup>89</sup> Conrad R. (1963) Acoustic confusions and memory span for words. *Nature* **197**: 1029–1030. *idem* (1964) Acoustic confusions in immediate memory. *British Journal of Psychology* **55**: 75–84.

<sup>90</sup> Baddeley A D. (1986) *Working Memory*. Oxford: Oxford University Press.

<sup>91</sup> Rabbitt P M A. (1968) Channel-capacity, intelligibility and immediate memory. *Quarterly Journal of Experimental Psychology* **20**: 241–248.

**Dr David Tyrrell:** I worked at the Common Cold Research Unit, Salisbury, a fellow MRC unit with the APU. I went to the unit in 1957 and almost by accident I read reports from the other MRC units, which included some from the APU. I thought their research was very interesting stuff and in the sort of general mental wondering that I supposed I should cultivate (trying to be a lateral thinker), I wondered if performance changes when you get a cold. It looked as though some of their tests might detect it, so I contacted the only psychologist I knew in the area, at Porton Down, but he was much too busy doing important work. So we went off and looked after viruses and clinical problems and things like that.

Years later Andy Smith, who became my colleague, and I think some of you may know him, often mentioned the names of Rabbitt and Baddeley. He had been a PhD student, studying the effects of noise on learning in schools, and the subject that he wanted to work on was whether disease could affect learning and psychological functioning. He was advised by Donald Broadbent to come to the Common Cold Unit and there he set up a programme.<sup>92</sup> It wasn't that easy. At the time he was employed in an MRC laboratory in Brighton, so he had to go back and forwards along the very crowded A27, before they made it a double carriageway. He could work with volunteers infected with common colds under double-blind conditions, who were going to be tested for whether they were really infected or not, and he began to show that when there were symptoms, hand-eye coordination was impaired. One of the reasons he could do this and maintain strict isolation was the computerization of some of the tests. We thought this meant interacting with big computers, but he brought along little Sinclair computers that could be put down in a unit such as ours, with separate volunteer rooms, and therefore we did not break isolation and so contaminate our biological results.

In the course of this sort of work we were encouraged to think more broadly about things that could influence an infection. One example was to do control questionnaires to see whether personality or environmental psychological factors could have an effect, and it looked as though people who were introverted, or who were having personal stress, not actually while they were at the unit, but in their home life and so on, seemed to be more susceptible to infection with viruses. These preliminary results were communicated to a visiting committee. Sir Christopher Booth was there and I remember he said that this was important, interesting work. For example, if people can't attend well and aren't having quite a normal degree of hand-eye coordination when they are firing a gun, it might have important consequences. I thought this was interesting work too, but the committee did not. This sort of work was not for our unit. However, we continue by doing what I call piggy-back experiments. The volunteers can be inoculated in order to test out some new drug

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<sup>92</sup>Smith A P, Tyrrell D A, al-Nakib W, Barrow P G, Higgins P G, Leekam S, Trickett S. (1989) Effects and after-effects of the common cold and influenza on human performance. *Neuropsychobiology* 21: 90–93.

against colds, that may or may not work, but we can use the data we get from their psychological testing to look at questions of the role of brain function on infection, and of infection on brain function. It rather tickles me that the only unit paper that appeared in the front of the *New England Journal of Medicine* was the report of a study of this sort.<sup>93</sup> We had an admirable collaborator, Sheldon Cohen from Pittsburgh, who managed to analyse work on the last 300-odd volunteers studied at the unit before it was closed down in 1990. His analysis shows that personal stress unquestionably makes you more susceptible to colds and that this is not a trivial effect. For instance, from the lower to the upper end of his scale of stress, you get a two-fold difference in your likelihood of being infected, getting symptoms and so on. He has gone on to replicate this and elaborate it by excellent work in the US.

In conclusion, people have said, 'Well, what the APU did was to generate ways of looking at psychological function in general conceptual terms and in testing these functions'. From all this we took just a small fragment, and applied it to what I thought were interesting and important questions in clinical medicine. Some may have practical applications, such as whether it's a good idea to drive fast when you have got a cold. Anyway our group enjoyed doing that research and I am very grateful to the people who made it possible.

**Gregory:** Thank you very much. Well, I think we will move on. Pat [Rabbitt], as everybody knows, has done a lot of work on ageing. I knew Pat when he was a student, and I am a hundred years older than he is. I can tell you what it is like being old, Pat – one becomes a gargoyle.

**Rabbitt:** This is a disclaimer of my ability to talk about clinical psychology, on which I was, somewhat unfortunately, scheduled to speak. I had assumed that the discussion might progress in terms of the historical chronology of theoretical rather than applied topics. This led me to think how it might be possible to illustrate how the work of the unit contributed to, and was driven by, some of the main theoretical frameworks used in human experimental psychology between the 1940s and the 1990s. In this context it is appropriate to remind you of people who were important members of scientific staff during the early days of the APU, and who were also very influential in the scientific community at large. One example would be William Hick, whose seminal paper picked up on the applications of Shannon and Weaver's Information theory to psychology.<sup>94</sup> Others of this generation, though not members of the APU, were Ted Crossman (who worked contemporaneously with Hick in the Nuffield Ageing Unit in Cambridge), Ray Hyman and 'Tex' Garner in the USA. This early interest in the unit provided a methodological framework for excellent further work in the late 1950s by Broadbent, Poulton and, in particular Alfred Leonard.

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<sup>93</sup> Cohen S, Tyrrell D A, Smith A D. (1991) Psychological stress and susceptibility to the common cold. *New England Journal of Medicine* **325**: 606–612.

<sup>94</sup> See note 9.

It is clumsy to let my personal interests obtrude, but, for me, models for choice reaction times (CRTs) provide a natural way of showing how work in the APU of the 1950s shaped thinking during the next decades and, indeed, is still relevant. Hick showed that CRT increases as a direct function of the binary logarithm of the number of signals and responses between which humans have to choose. Hyman showed that CRTs also vary with the relative probability of occurrence of signals and responses during a task.<sup>95</sup> The APU statistician, Mervyn Stone, imaginatively saw that one effect of biasing probability or reducing the number of alternatives in CRT tasks was to increase the frequency with which any individual signal was repeated on immediately successive trials. That is, as signals become more frequent they get repeated more often, and RTs to repeated signals are much faster than RTs to signals that follow other, different, signals. This recognition of a 'repetition effect'<sup>96</sup> provided a simpler explanation of signal probability effects than did the line favoured by Hick and Hyman, which was that frequent signals are rapidly recognized and responded to because they convey less information, while rare signals are more slowly recognized because they convey more information. Stone's early recognition of repetition effects became influential in European cognitive psychology because it was taken up by the distinguished Belgian psychologist, Paul Bertelson, who became aware of the idea while he was a visitor to the APU and, it is fair to say, based his early career on it. Stone showed that there were signal and response repetition effects as well as probability effects, but he never found a way of separating them.<sup>97</sup> There was a repetition effect clearly as well as probability bias effects. Bertelson in Brussels vigorously pursued this issue, as did Sylvan Kornblum, also an American visitor to the APU, who, in 1966, finally showed that both kinds of effect existed.<sup>98</sup>

In the APU itself Henry Shaffer,<sup>99</sup> in a much neglected paper, showed that repetition effects were not found only for easily discriminable signals and simple responses but also for repetitions of the logical structures of patterns of relationships between events and responses

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<sup>95</sup> Hyman R. (1953) Stimulus information as a determinant of reaction time. *Journal of Experimental Psychology* **45**: 188–196.

<sup>96</sup> Stone M. (1960) Models for choice-reaction time. *Psychometrika* **25**: 251–260.

<sup>97</sup> Bertelson P. (1961) Sequential redundancy and speed in a serial two-choice responding task. *Quarterly Journal of Experimental Psychology* **13**: 90–102.

<sup>98</sup> Koster W. G. (ed.) (1969) *Attention and Performance II: Proceedings of the Donders Centenary Symposium on reaction time*. Amsterdam: North-Holland Publishing Co. The symposium was held in Eindhoven, 29 July–2 August 1968 under the auspices of the Institute for Perception Research, Eindhoven, the Netherlands, and reprinted from *Acta Psychologica* **30**, 1969. Kornblum S. (1973) Sequential effects in choice reaction time: A tutorial review, in Kornblum S. (ed.) *Attention and Performance IV*. New York: Academic Press, 259–288.

<sup>99</sup> Shaffer L. H. (1966) Some effects of partial advance information on choice reaction with fixed or variable S-R mapping. *Journal of Experimental Psychology* **72**: 541–545.

to them. That is to say, when people deal with successive problems that have the same logical structure, they respond faster and more accurately than when they have to deal with problems that have different structures.

Another similar issue was whether the repetition effects in simple tasks only occurred when particular signals were repeated, or only when particular responses were repeated and, if there were repetition effects for both signals and responses, which were the more powerful. This is of interest to experimenters who hope to dissect the chain of functional relationships that intervene between the perception of a signal and the selection and execution of an appropriate response to it. One way of doing this is to ask people to make the same response to all members of one set of signals and a different response to all members of another, different set. This allows analysis of the effects of repeating an identical signal and an identical response. Responding successively to different signals, but with the same response, and of responding to a new signal with a new response. This work by Bertelson led to the further question of the relative effects of signal and response information load. That is, people have to make the same response to all members of a large set of signals and a different response to all signals in another set, whether increasing the numbers of signals in each set has the same, or a lesser effect as increasing the numbers of signals and responses. This produced work by Broadbent, and also by Irv Pollack,<sup>100</sup> then a US visitor to the unit, on many-to-one mapping. It also, incidentally, started my own career as a research student, loosely attached to the unit. We all found that the sizes of sets of signals can be increased very greatly, without increasing response time. In other words it is the number of categories into which people have to divide events, rather than the total number of events to which they have to respond, that increases their decision times. This has obvious practical implications for the design of systems that require people to choose between alternatives, but it also has important theoretical implications for the understanding of how people manage such efficient perceptual categorization of their complex and fast-changing worlds.

There were many other theoretical advances that directly came out of applied work, and which form the foundations of our current theories of cognitive performance. One for which the early APU was famous was the efficiency with which people can maintain selective attention during very long and dull tasks. Mackworth's studies of signal detection in tasks simulating that of radar operators have become classic in the field, and the large part of the scientific history of the APU that is concerned with this issue has been very ably reviewed in Donald Broadbent's magisterial book *Decision and Stress*.<sup>101</sup>

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<sup>100</sup> Pollack I. (1952) *The Assimilation of Sequentially-encoded Information*. 3. Serial position analyses. Human Resources Research Laboratories Memo Report no. 25. Air Research and Development Command, US Air Force.

<sup>101</sup> Broadbent D E. (1971) *Decision and Stress*. London: Academic Press.

Another aspect of the problem, in which the APU and Broadbent himself were much involved was the use of reaction time tasks to study this problem. The issue was whether people doing long, boring tasks begin, as the task runs on, to pause increasingly often to produce exceptionally slow reaction times that have been termed 'blocks' or 'gaps'. It is an empirical fact that as tasks are prolonged, more unusually slow responses appear, but what is still not clear is whether these slow responses are rare, atypical events, unlike others, reflecting momentary 'blocking' of attention or, as Lubin and others have claimed 'microsleeps' during which no information can be processed, or whether they are simply the outliers of the very skewed distributions of RTs that are observed in all tasks. Another way of looking at this distinction is whether these slow responses represent points at which a fatigued system switches off for a second or more, or whether people have no choice but to rarely make some exceptionally slow responses because their responses are generated by a functional system that has idiosyncratic statistical properties and cannot help but generate some very slow responses from time to time, whether or not it is fatigued. This is still a very lively issue, and it is interesting to note how explanations and data first offered in the early 1960s by Paul Bertelson, Alfred Leonard, Donald Broadbent, Alan Welford and others anticipate papers in recent issues of the *Psychonomic Bulletin and Review* by Graham Radcliffe and others.<sup>102</sup>

Another example is the problem of whether, and how, people can manage to detect and correct errors that they make during continuous industrial processes. Again, reaction times offer an unglamorous, but very simple and illuminating approach to this. Demonstrations that people could detect and correct most of the errors that they made during continuous CRT tasks, and could do this very quickly indeed, were congenial to ergonomists who have to worry about maintenance of accurate performance in repetitive tasks. The finding that error correction is not only reliable, but also very fast indeed, sometimes as fast as 40 msec, raised theoretical issues as to how a CNS in which component neurons have a minimum operating lag of 1 msec can manage to do this.

The resolution is that the brain processes events in parallel, and that processing continues after the moment when a decision is made, so that an impulsive incorrect response can very rapidly be countermanded by another response that has been simultaneously programmed, and is, as it were, waiting its turn in case it is necessary. To put this another way, the brain is a parallel processing system in which different processes work towards the same outcome so that if some, perhaps terminating prematurely, terminate in incorrect solutions they can

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<sup>102</sup> Dr Susan Marshall of the *Psychonomic Bulletin and Review* wrote: 'We have no record of recently accepted papers by Graham Radcliffe.' E-mail to Mrs Lois Reynolds, 29 January 2003. Copies of this correspondence will be deposited with the records of the meeting in Archives and Manuscripts, Wellcome Library, London.

be countermanded by others that have taken a little longer and so, perhaps, have a higher probability of terminating accurately.

Another topic that is currently very lively in the theory of attention, is the study of spatial attention, currently vigorously pursued by Charles Spence in Oxford and by Jonathan Driver in UCL.<sup>103</sup> This also had antecedents in the early years of the APU. The first work on spatial attention that I know of was done by Alfred Leonard,<sup>104</sup> and by Donald Broadbent and the then Margaret Gregory in the very early 1960s. This, like much of the recent work by Spence and Driver, was concerned with comparing responses to tactile stimulation in different parts of the haptic field with visual stimulation in different parts of the visual field.

**Gregory:** Another thing that Alfred Leonard did was on the blind, didn't he, with canes of different lengths?<sup>105</sup> That's a thing nobody has mentioned yet – help to handicapped people. That's another area going on, is it not?

**Rabbitt:** People who overlapped with Alfred's last years in the unit may add to that. I don't know. Perhaps why they didn't mention it was because most of that work was done after he left, at the Blind Research Unit in Nottingham.<sup>106</sup>

**Professor William Marslen-Wilson:** This is something I was really wanting to say before Pat's remarks, but which I think fits in well with those. I think an interesting continuity is the way in which the theoretical issues have steadily emerged as ends in themselves. As current director I would be very surprised if some group came to me and said, 'I want you to organize a massive project on violent behaviour' or some such. There's been quite a rapid shift in attitudes. For many years the MRC saw it as part of its remit to provide the sort of services the APU was providing to the military and quangos and so on, but there has clearly been a shift away from that. Our remit now is very much basic research in cognitive function in the context of underlying brain systems as tapped into through

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<sup>103</sup> Spence C, Driver J. (1996) Audiovisual links in endogenous covert spatial attention. *Journal of Experimental Psychology, Human Perception and Performance* **22**: 1005–1030. *idem* (1997) Audiovisual links in exogenous covert spatial orienting. *Perception and Psychophysics* **59**: 1–22. *idem* (1997) On measuring selective attention to an expected sensory modality. *ibid.* **59**: 389–403.

<sup>104</sup> Leonard J A. (1959) Tactual choice reactions. 1. *Quarterly Journal of Experimental Psychology* **11**: 76–83.

<sup>105</sup> Leonard J A. (1963) Mobility and the blind. *Medicine, Electronics and Biological Engineering* **1**: 465–481. Leonard J A, Davidson A. (1965) A digital read-out device for blind people. *ibid.* **3**: 441–442. Leonard J A. (1966) Experimental maps for blind travel. *New Beacon* **50**: 32–35. *idem* (1966) Aids to navigation: A discussion of the problem of maps for blind travelling, in *Sensory Devices for the Blind*. London: St Dunstan's. Leonard A J, Newman R C. (1970) Three types of 'maps' for blind travel. *Ergonomics* **13**: 165–179.

<sup>106</sup> The Blind Mobility Research Unit was at the University of Nottingham.



neuro-psychological research, brain imaging, and other kinds of clinical input. I wonder whether anybody here has anything to say on this? Alan in particular would be in a position to say something about how this has developed over the last 15 years.

**Baddeley:** Yes, it might be interesting to talk about how the unit got involved in clinical psychology in that context. Although I think Derek Russell Davis, possibly within the unit, did clinical work during the Second World War, for many years the unit was not, I think, involved at all clinically. One of the features of the unit is that I had to keep the MRC and particularly the Neuroscience Board happy, and in order to do that you have got to have a reasonable number of publications and have some thoughts as to where you are going next. Now that's actually a great boost to thought. It means that you not only can, but also are encouraged to, choose areas that are not currently terribly popular. I think clinical psychology was a case in point. It was chosen because it seemed theoretically important to look at the interface between cognition and emotion. Doing that within the laboratory is not terribly easy. Clearly, clinically based emotional factors are important, and this seemed to be an obvious way forward. The difficulty was finding clinicians who wanted to be employed in this area. We had quite a lot of difficulty finding clinical psychologists who were prepared to swap over, so to speak. Eventually Fraser Watts,<sup>107</sup> who was running a clinical service at King's College, London, agreed to move. Then subsequently, John Teasdale,<sup>108</sup> who was doing clinical research in Oxford, decided that he would move to Cambridge. Mark Williams, who had a lectureship in Newcastle, came down initially for a non-tenured job. They built up a group that became very committed to the idea of combining cognitive psychology with the clinical issues of depression, anxiety and so forth. They produced a book that proved to be extremely influential.<sup>109</sup> They started a new journal in 1987, *Cognition and Emotion*, and I think sowed the seeds of what was to become really quite a major development. That was made possible by the fact that we had a unit that was large enough to be able to take a gamble on branching out in a new direction and the MRC was, in those days at least, forgiving enough to maintain support over a decade. Initially, they weren't very keen on this development, but fortunately by the next progress report they were very enthusiastic. I think it reflects (a) the fact that we were encouraged to strike out, (b) that we were allowed sufficient time to do that, and (c) that sometimes choosing what appears to be a practical problem actually can have considerable theoretical impact.

**Gregory:** Any other comments on this?

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<sup>107</sup> See, for example, Watts F N. (ed.) (1993) *Neuropsychological Perspectives on Emotion*. Hove: L Erlbaum.

<sup>108</sup> See, for example, Teasdale J D, Barnard P J. (1993) *Affect, Cognition, and Change: Re-modelling depressive thought*. Hove: L Erlbaum.

<sup>109</sup> Williams J M G, Watts F N, MacLeod C, Matthews A. (1988) *Cognitive Psychology and Emotional Disorders*. Chichester: John Wiley.

**Barnard:** I would just like to add to what Alan said, and perhaps try to draw a link into what Pat was saying before that about the core theoretical links. To a certain extent I think Alan was underestimating or underplaying his own role in encouraging the theoretical connection between, for example, memory and attention and clinical issues, and then the applications of those in a therapeutic context. In the course of the work on cognition and emotion at the unit,<sup>110</sup> there's been loads of stuff on the emotional Stroop effect<sup>111</sup> and how attention is employed in anxiety, sort of consolidating many of the attentional ideas that were developed in other contexts in the unit. The same is true of Mark Williams' work on memory,<sup>112</sup> demonstrating, for example, that suicide patients and depressed patients experience over-general memory, and then subsequently going onto some of the theoretical ideas to show how particular therapeutic techniques, for example, could reduce over-general memory, or that interventions based on theory, like John Teasdale's development of mindfulness-based cognitive therapy,<sup>113</sup> could actually reduce relapse rates in severe repeated depression by up to 50 per cent. The connection between theory and application, which Alan has played a big role in encouraging,<sup>114</sup> was actually carried through again, having a very real impact in the world of therapy and outside the lab.

**Logie:** I think one of the other areas that follows on from what Pat was saying about the link between practical problems and theory which hasn't really been brought out so far, is the topic Alan Baddeley touched on earlier in terms of thinking of neuropsychology, not in terms of normal people being affected by adverse conditions, but the impact of brain damage on individuals. During the 1980s in particular, there was a major development in investigating the impact of various forms of brain damage, either brain disease, or focal brain damage, and driving those investigations by theory and using the information, the data, from those investigations to develop the theory further. That kind of synergy, I think, developed not only within the APU itself, but also in a number of very successful

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<sup>110</sup> See note 109.

<sup>111</sup> The 'Stroop effect' was named after J Ridley Stroop. The naming of the colour of the ink in which a word is printed is slowed if the actual word refers to a colour that is different from the ink colour itself. See Stroop J R. (1935) Studies of interference in serial verbal reactions. *Journal of Experimental Psychology* 28: 643–662. Words with emotional links can have the same effect of interfering with the naming of ink colour. Watts F N, McKenna F P, Sharrock R, Trezise L. (1986) Colour naming of phobia-related words. *British Journal of Psychology* 77: 97–108.

<sup>112</sup> Williams J M C. (1996) Depression and the specificity of autobiographical memory, in Rubin D C. (ed.) *Remembering Our Past: Studies in autobiographical memory*. Cambridge: Cambridge University Press, 244–267.

<sup>113</sup> Segal Z V, Williams J M G, Teasdale J D. (2002) *Mindfulness-based Cognitive Therapy for Depression: A new approach to preventing relapse*. New York: Guilford Press.

<sup>114</sup> Baddeley A, Logie R, Bressi S, Della Sala S, Spinnler H. (1986) Senile dementia and working memory. *Quarterly Journal of Experimental Psychology* 38: 603–618.

collaborations including very successful international collaborations. There were several very successful international links with Italian neurologists, for example, where people from the APU brought a knowledge of cognitive psychology and the Italians came from a medical background with the neurology expertise. Many of those collaborations are still on-going,<sup>115</sup> still continuing to contribute to our understanding of memory, of attention, and also of understanding the nature of the deficits suffered by these individuals. Of course latterly the work in terms of applying that to rehabilitation in neuropsychology was also a major theme within the APU, and that I think was a major success, one of the other major successes of the APU from about the early 1980s onwards. Not least is Alan's own influence in that area, where his own contribution has been really quite tremendous in terms of increasing our understanding of human memory through the study of not only healthy individuals, but also individuals who suffered from these forms of brain damage.

**Professor Sergio Della Sala:** I am one of the Italian neurologists who capitalized on the relationship with the unit in the early 1980s. I just want to share my autobiographical bit of what I learnt.

I came from an environment, a neurology environment, a medical school, where I was labelling symptoms. I was giving names to diseases. I arrived in the early 1980s at the unit and I learned through questioning, 'What does this mean? What is it about?', to question the symptoms in some more depth. I learned to use the cognitive approach, the theories, to understand the symptoms a bit more, to the point that I thought eventually it was a better approach and I swapped from medical school to psychology. But at the time when I went to the unit, it was quite a shock; I came from medical school with pomposity and I was there being asked about a patient and I was giving names – symptom, like amnesia – as if this was encompassing the concept. Then when I was pressed to answer further questions, 'What did this patient have? Has there been pain?', I didn't have any answer, but I learnt from Alan (Baddeley) and Bob (Logie) and other people there that there were ways of finding out answers about what these people actually had. A typical example was our own work on Alzheimer's disease.<sup>116</sup> We were labelling Alzheimer's disease as a cognitive memory deficit, and I slowly learned to use models that Alan and other people were developing to understand a bit more what these people were really facing, and coming up with a better understanding of the patient's problems. That was the early 1980s, so the story about studying the brain and cognitive brain was started very early on.

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<sup>115</sup> Denis M, Bescin N, Logie R H, Della Sala S. (2002) Visual perception and verbal descriptions as sources for generating mental representations: evidence from representational neglect. *Cognitive Neuropsychology* **19**: 97–112.

<sup>116</sup> Baddeley A D, Bressi S, Della Sala S, Logie R, Spinnler H. (1991) The decline of working memory in Alzheimer's disease. A longitudinal study. *Brain* **114**: 2521–2542. *idem* (1986) Dementia and working memory. *Quarterly Journal of Experimental Psychology A [Human Experimental Psychology]* **38**: 603–618.

**Gregory:** Any other points?

**Booth:** On the question of clinical research, I don't know to what extent the unit had relationships with other areas within Cambridge and within the university. The feature that comes out of human factors research is that it nearly always involves two or three different disciplines. I just wondered to what extent the unit was involved with, say, forensic psychology or clinical analysis, and the use of clinical testing of children and in people with brain damage. I don't know the extent you were involved in that.

**Baddeley:** May I answer the question briefly? In the early days the links with Addenbrooke's were not very strong, with the exception of speech therapy. In general the Neurology Department did not seem very interested in behaviour and did not really seem to believe you could measure anything very much. I am happy to say that that has changed dramatically, once the University Medical School developed and they started to appoint academic professors who were actually interested in research, whereas I think in the early years basically many were principally interested in clinical issues. I have to say we did get support from Walpole Lewin, for example, on head injury,<sup>117</sup> and, as I said from speech therapy, and more recently through a very substantial liaison with psychiatry.<sup>118</sup> I think William can no doubt say more about that.

**Marslen-Wilson:** I think I should say that, thanks in particular to the enormous amount of ground work that Alan put in, we now have a very good relationship with Addenbrooke's [Hospital], so that almost every one of the groupings at the unit works closely with various departments there. One very important relationship we now have, of course, is with the Wolfson Brain Imaging Centre, which got going about three or four years ago.<sup>119</sup> We have been intimately related to the developments there. I think somebody made a point about having to go to London to get patients. Nowadays – and I think the leadership of John Duncan was particularly important here – we now have the Cambridge-wide Cognitive

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<sup>117</sup> See, for example, Lewin W. (1964) Nonsurgical treatment of patients with head injury. *Clinical Neurosurgery* 12: 75–94. *idem* (1966) *The Management of Head Injuries*. London: Baillière Tindall and Cassell. *idem* (1976) Changing attitudes to the management of severe head injuries. *British Medical Journal* ii: 1234–1239. Lewin W, Marshall T F, Roberts A H. (1979) Long-term outcome after severe head injury. *ibid.* ii: 1533–1538.

<sup>118</sup> Teasdale J D, Taylor M J, Cooper Z, Hayhurst H. (1995) Depressive thinking: shifts in construct accessibility or in schematic mental models? *Journal of Abnormal Psychology* 104: 500–507.

<sup>119</sup> Professor William Marslen-Wilson wrote: 'The Wolfson Brain Imaging Centre (WBIC), based at Addenbrooke's Hospital in Cambridge and directed by Professor John Pickard, houses two separate instruments for human brain imaging: a positron emission tomography (PET) camera and a 3 Tesla magnetic resonance imaging (MRI) system. The MRC Cognition and Brain Sciences Unit (the successor to the APU) is one of the principal users of this facility, especially for research in cognitive neuroscience using fMRI.' Note on draft transcript, 11 December 2002.

Neuroscience Research Panel<sup>120</sup> run from the unit, whereby in effect every single patient who comes into Addenbrooke's from the East Anglian catchment area, who has a brain injury is evaluated for possible interest for research. If they are of interest, there are complex processes of permissions and so forth, and currently we have 200 or more neuropsychological patients to access, which is a tremendous resource. That's just one example of the breadth and the depth of our links with Addenbrooke's and with neuropsychological and clinical material in general. I have just one little footnote to something that Geoff Bunn mentioned about the move from the Department to Chaucer Road. I am sure Alan has also read it, but there's a very interesting file of largely hand-written correspondence between Mackworth, Zangwill and Sir Harold Himsworth, the then head of the MRC. With the change from the Bartlett to the Zangwill regime and the relationships with Mackworth and so forth, it wasn't a completely straightforward move. But it's certainly true that Mackworth did go out and pay personally a 10 per cent deposit of £800 on this £8000 and there is a letter from Himsworth to Mackworth, saying, 'I don't really mean this, but none the less duty obliges me to write to you and reprimand you for committing MRC money in advance'.<sup>121</sup> So indeed he did commit the unit to buying the MRC the very nice house.

**Dr Peter Hunter:** I was very interested that Dr Mackworth did research on the design of displays.<sup>122</sup> Did he include research into the relationship between the design of displays and crashes on landing on aircraft carriers?<sup>123</sup> Did the unit do research on the mental function

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<sup>120</sup> Dr John Duncan wrote: 'This panel operates in a very similar way to the volunteer panel. [See note 12.] If they volunteer to join the panel, patients serve in a wide range of neuropsychological studies, not only from the CBU, but from the whole Cambridge research community. In this way we gain the benefits of shared organization in patient identification and access, shared behavioural results, and shared access to brain imaging data and lesion description. Over the past five years or so, around 70 scientists have made use of this panel. As far as I know, nothing quite like this is available elsewhere in the world.' E-mail to Mrs Lois Reynolds, 24 January 2003.

<sup>121</sup> Professor William Marslen-Wilson wrote: 'The letter is dated 31 July 1952.' Note on draft transcript, 11 December 2002.

<sup>122</sup> See note 18.

<sup>123</sup> Dr Peter Hunter wrote: 'I recall being told that after the Second World War, the Fleet Air Arm became concerned at the number of crashes occurring during landings on aircraft carriers. A decision was made to study the direction of gaze of pilots during the landing approach. Two small pieces of a light-reflecting material were attached to the conjunctiva of each eye. A fixed light source was shone on the reflectors. As the pilot's line of gaze changed so did the angle of the reflected light. By measuring the angle continuously it became possible to establish not only whether the pilot was looking at the approaching carrier deck or at his instruments, but also for how long. The technique also made it possible to establish which part of the instrument display was being looked at. It was established that during the last phase of the approach a disproportionate amount of time was spent watching the air-speed indicator. The problem was overcome by feeding air-speed information to the pilot as an auditory signal.' Note on draft transcript, 27 November 2002.

of nuclear submarine crews on prolonged underwater patrols, in particular diurnal variation in cognitive function?

**Baddeley:** Yes, there was a huge amount of work done over the years on diurnal variation of cognitive function,<sup>124</sup> which was I am sure at least partly aimed at crews of nuclear submarines. Exactly what was done and when, I am not sure; if Bob Wilkinson were here, he might be able to help, but certainly there was a lot of work done on diurnal function, fairly regularly generated by the Navy,<sup>125</sup> not always having quite the effect that one might hope.

There was one occasion when I was visiting Portsmouth as chairman of an MRC Navy subcommittee. The chief Navy psychologist was Edward Elliott, who had a rather dry sense of humour. The rear admiral at the time was keen on having more work done on watch-keeping schedules. After talking to him we were taken to see the latest destroyer and Edward nudged me and said, 'Ask the captain how he decides on what the watch-keeping schedules are.' We must have done 20 years' research on this. The captain said, 'Oh, I just get the chaps together and ask them what they feel like, and we sort it out; if everybody's happy with them, then we do that.' So I asked the admiral if he knew how the Navy chose its watch-keeping schedules. He didn't, but he still decided that he would commission the work, and I am sure it has had some impact. But, I think, like a lot of applied work, it's often not necessarily the people who commission it who get the benefits, simply because research takes time and it takes time to filter through to the user.

**Marslen-Wilson:** I might have to pass you over to Alan Baddeley to fill in some of the darker secrets of the APU's efforts in speech and language. This should not be regarded as the result of an in-depth historical analysis of the work of the unit over those years. I just want to mention a few contributions that the unit has made to research in speech and language that I was aware of, mainly from the perspective of the development of theory about how speech and language are processed as complex mental activities. Alan has just reminded me that there's an early paper by Broadbent and Ladefoged on the effects of a speaker's voice and the discrimination of 'bet' and 'bit'.<sup>126</sup> I think that's an important and influential article demonstrating the relativity of the perceptual account that you assign to the same piece of speech, depending on who you think it is said by and at what rate it is being said. John Morton, who should be here speaking about this, was extremely influential

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<sup>124</sup> Colquhoun W P. (1971) Circadian variations in mental efficiency, in Colquhoun W P. (ed.) *Biological Rhythms and Human Performance*. London: Academic Press, 39–107.

<sup>125</sup> Wilkinson R T. (1961) Comparison of paced, unpaced irregular and continuous display in watch keeping. *Ergonomics* 4: 259–267.

<sup>126</sup> Ladefoged P, Broadbent D E. (1957) The information conveyed by vowels. *Journal of the Acoustical Society of America* 29: 98–104.

in the development of modern theories of what's called the mental lexicon, which is how words are stored in the mind. John Morton,<sup>127</sup> apart from his skills as a guitarist and an actor, will always be remembered as the author of the logogen theory. There was a well-known article published in *Psychological Review* in 1969, putting forward the logogen theory, which is really a very early multiple parallel theory about how words are represented in the mental system and how the input, spoken or text, is mapped on to these representations in order to solve the problem of recognition.<sup>128</sup>

There is also a very influential line of work associated with Karalyn Patterson, based in neuropsychological concepts; this is work that she and others at the unit did with a particular class of patients. These were patients who following stroke have selective disorders of language function. One particular kind of syndrome that she's associated with is 'deep dyslexia'. This is a very interesting acquired disorder of reading, where in particular the person will tend to make mistakes, where they will replace the word that they see with another word that may be completely different in form, but has a similar meaning, so reading 'orchestra' as 'symphony' and so forth.<sup>129</sup> This is not something that has really been brought out so far, but the unit played an important role in the development of the field of cognitive neuropsychology, which is really a development of the information-processing approach to cognition, and marrying it with evidence from brain lesions and neuropsychology. The doctrine is strictly functional in its perspective, looking at a pattern of deficits, for example, deep dyslexia or other kinds of dyslexia, and making inferences about the functional structure of the cognitive system that supports them.

Another important group that is still pretty active in the unit is represented by Ann Cutler and Dennis Norris. Ann Cutler has since moved to be a director of the Max Planck Institute in Nijmegen. Ann Cutler and Dennis Norris are very well-known exponents of their own

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<sup>127</sup> See biographical notes on page 85. For speed reading on page 56, see, Morton J. (1966) A two-hour reading course. *Nature* **211**: 323–324.

<sup>128</sup> Morton J. (1969) The interaction of information in word recognition. *Psychological Review* **76**: 165–178. *idem* (1970) A functional model of memory, in Norman D A. (ed.) *Models of Human Memory*. New York: Academic Press, 203–254. *idem* (1979) Facilitation in word-recognition experiments causing changes in the logogen model, in Kolers P A, Wrolstad M E, Bouma H. (eds) *Processing of Visible Language*, vol. 1. New York: Plenum Press, 259–268.

<sup>129</sup> Morton J, Patterson K E. (1980) A new attempt at an interpretation, or, an attempt at a new interpretation, in Coltheart M, Patterson K E, Marshall J C. (eds) *Deep Dyslexia*. London: Routledge, 91–118. Patterson K E. (1979) What is right with 'deep' dyslexic patients? *Brain and Language* **8**: 111–129. Patterson K. (1981) Neuropsychological approaches to the study of reading. *British Journal of Psychology* **72**: 151–174. Patterson K, Marshall J C, Coltheart M. (eds) (1985) *Surface Dyslexia: Neuropsychological and Cognitive Studies of Phonological Reading*. London: Erlbaum.

theories of the mental lexicon, of spoken word recognition.<sup>130</sup> There's also a very strong theme that I am probably less able to say something about, which I might pass on to Phil Barnard. This is the human factors work on language, on text – for example, Pat Wright's work on design of forms and so forth.<sup>131</sup> There was also the work of people such as Conrad, working on speech and language in the deaf;<sup>132</sup> again, there may be people here who are better equipped than I am to comment on it. But even with this selective, off-the-cuff summary, it's clear that language was and has been an important part of the unit's contribution. I have emphasized its contribution to what we have been calling theory rather than practice, and I am sure in many cases these were bits of research that did indeed grow out of applied problems. Perhaps Alan Baddeley would like to augment some of that.

**Baddeley:** Just one brief story that William thought I ought to tell. Many years ago now there was an MP who was rather deaf, and who frequently asked questions in the House as to how much work the research councils were doing on speech and language. So every year, I would be rung by I think a retired research worker and asked how much were we doing on speech and language. I always said, 'Well, it's difficult to define it really, for example, I am doing these experiments on people hearing and remembering words and repeating them back'. 'Oh, that's speech and language', he would say, and it ended up, I think, that the unit was doing three-quarters speech and language. I suspect monitoring is, perhaps, a bit more eagle-eyed these days.

**Gregory:** Any other points on speech and language? There must be a lot more on this topic, because it's pretty central to those of us in the jabbering classes.

**Professor John Groeger:** I am sure Alan is too modest to mention it, but, of course, developments of the working memory model have been very influential in terms of characterizing the acquisition of first and second languages.<sup>133</sup> This is an example of a way

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<sup>130</sup> Cutler A, Norris D G. (1988) The role of strong syllables in segmentation for lexical access. *Journal of Experimental Psychology: Human Perception and Performance* **14**: 113–121. Norris D. (1994) Shortlist: a connectionist model of continuous speech recognition. *Cognition* **52**: 189–234.

<sup>131</sup> For example, Wright P, Lickorish A. (1989) The influence of discourse structure on display and navigation in hypertexts, in Williams N, Holt P. (eds) *Computers and Writing: Models and tools*. Oxford: Intellect/BSP, 90–124. *idem* (1988) Colour cues as location aids in lengthy texts on screen and paper. *Behaviour and Information Technology* **7**: 11–30.

<sup>132</sup> Conrad R. (1970) Short-term memory processes in the deaf. *British Journal of Psychology* **61**: 179–195. *idem* (1977) The reading ability of deaf school-leavers. *British Journal of Educational Psychology* **47**: 138–148. *idem* (1979) *The Deaf Schoolchild*. London: Harper and Row.

<sup>133</sup> First and second languages might be English and Italian. See Baddeley A D, Gathercole S E, Papagno C. (1998) The phonological loop as a language learning device. *Psychological Review* **105**: 158–173.



in which two perhaps relatively separate parts of the unit's activity have combined to produce workable accounts of processes that might normally be regarded as distinct.

**Gregory:** Any other points? This is perhaps not very relevant: there was Mackworth's work, wasn't there, on whether you could read upper or lower case letters more easily and he discovered that lower case were better than upper case.<sup>134</sup> Does that count in this topic or not?

**Brown:** I don't know very much about it, but I did look some of this up, earlier this year I think it was, so that I could say something about Christopher Poulton at his memorial service, because unfortunately he died towards the end of last year.<sup>135</sup> The only story I know is that he was doing work on speed reading and recognition of lower case versus capitals and as a result of his research, they completely changed the front page of *The Times* newspaper, which you recall was incredibly boring and almost illegible in the early days. So I think we have to thank Christopher's research for that change.<sup>136</sup> That really is all I know about his work in this field. All that I can remember anyway.

**Baddeley:** John Morton, among other things, used to do some research on speed reading, at least he used to do practicals on speed reading, and on one occasion I went along and helped him. This would involve all the students being encouraged to bring a paperback book and to read it for x minutes, followed by a period when John would urge them to go faster, and faster, and faster, and demonstrate then that they *could* actually read a lot faster, and that there was nothing very magical about it. It was just that we tend to read slowly – it's a habit. I have no idea whether he ever wrote it up, or whether he ever set up courses in this, but it was one of the things that John was very interested in.

**Gregory:** Was there ever work done on getting information through listening, as in a lecture, or reading, and comparing listening and reading? That seems a very interesting topic. Was that ever worked on?

**Baddeley:** I have a feeling Pat Wright's done work on that. My memory is that there are differences, that they are not as large as you would expect, disappointingly so; there certainly has been work done, but not very exciting work.

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<sup>134</sup> Mackworth N H. (1944) *Legibility of Raid Block Letters and Numbers*. April 1944. FPRC423 (s). Unpublished reports to the Medical Research Council circulated in the Flying Personnel Research Committee series.

<sup>135</sup> See biographical note on page 81. See also Oakeshott P. (2001) Eustace Christopher Poulton. *British Medical Journal* **322**: 999.

<sup>136</sup> See, for example, Poulton E C. (1959) *Effects of Printing Types and Formats on the Comprehension of Scientific Journals*. Cambridge: Cambridge University Press.

**McLeod:** If I can just add one more anecdote about Christopher Poulton's work on reading. In a fairly classic Christopher experiment, he compared how quickly people would read and subsequently remember text, if it was right justified, or if it had a ragged right. He showed, not surprisingly I suppose, that right justification actually impeded reading speed and memorability, presumably because of the uneven spaces between the words and uneven spacing of the letters. This article was published in *Applied Ergonomics*, and, to their credit, they changed from having right justification of their text to a ragged right.<sup>137</sup> I think they are the only journal I know that actually does this.

**Gregory:** Actually I can cap that, because I run a journal called *Perception* and we started off with it unjustified on the right, and a lot of people refused to publish in the journal, because it didn't look posh enough. That's an absolutely true story. I remember that Mackworth was working on upper and lower case, very early on, just after the war, for legibility for runways and that sort of thing. OK. Well, we move on to memory and believe it or not, Alan Baddeley is going to talk on this.

**Baddeley:** Well, the unit has been associated with memory both theoretically and in an applied way from the start. The unit's second director, Bartlett, was particularly famous for his work on memory for prose, long-term memory and the impact of knowledge on memory.<sup>138</sup> I think probably by the time the unit was set up, he was more interested in thinking and applied issues. Probably the unit's subsequent contribution has been more obviously to aspects of short-term memory, where Donald Broadbent's 1958 book<sup>139</sup> elaborated probably the first and most influential information-processing model in the area. It was influenced among other things by his work on attention, using dual-task methodology in order to look at the capacity to share attention. As we heard earlier, this was something that was stimulated initially by problems of air traffic control.<sup>140</sup> That led to

<sup>137</sup> The editors of *Applied Ergonomics* wrote in the first issue: 'There has been a considerable amount of research in recent years, primarily by Dr Christopher Poulton, on various aspects of format, layout and typography for easier reading and comprehension. The journal has made use of this research and expert advice also, and it is planned to summarize the relevant data and their implementation in a case-study in an early issue.' See Anonymous. (1969) *Applied Ergonomics – aims and philosophy. Applied Ergonomics* 1: 2–3. Quote on page 3. For details of the design and layout of the new journal, see Poulton E C, Warren T R, Bond J. (1970) Ergonomics in journal design. *ibid.* 1: 207–209. See also Poulton E C. (1959) A note on printing to make comprehension easier. *Ergonomics* 3: 245–248. Burt C. (1959) *A Psychological Study of Typography*. Cambridge: Cambridge University Press.

<sup>138</sup> Bartlett F C. (1932) *Remembering: A study in experimental and social psychology*. Cambridge: Cambridge University Press.

<sup>139</sup> See note 22.

<sup>140</sup> See a special issue of *Applied Cognitive Psychology* devoted to Donald Broadbent and applied cognitive psychology (guest editor Dianne Berry, vol. 9, 1995, S1–S216).

other models of memory, including the Atkinson and Shiffrin model<sup>141</sup> in the late 1960s (which explicitly acknowledges its debt to Broadbent), as did other models such as my own working memory model, which is very much an elaboration of Donald's initial model. A second, and very important, source of influence in short-term memory is Conrad's work. Again it was driven initially by the practical issues of telephony and people remembering telephone numbers or postal codes. He was the first person to demonstrate the importance of acoustic confusions in immediate memory, and to demonstrate the acoustic similarity effect, which has continued to play a very important role in studies of short-term memory. My own work was stimulated by Conrad's research.<sup>142</sup> I initially actually worked on long-term memory, because I was employed to work on the design of postal codes. I became interested in language structure and postal codes and what made nonsensical material easier or harder to remember. But when Conrad went on sabbatical, he had just discovered the phonological similarity effect<sup>143</sup> and I had the job (that Pat Rabbitt subsequently took over, I am happy to say) of trying to measure the effectiveness of telephone communication by using other measures. I thought, 'Well, I will try looking at the effects of using short-term memory and I will look at Conrad's acoustic similarity effect'. I was captivated by how big and easily obtainable an effect it was. You could test people in a group of 20 and they would all show the effect.<sup>144</sup> I, together with a friend, Harold Dale [see Figure 17, page 62], became intrigued with using this, and exploring it more widely. We found that there was a difference between the effects of acoustic similarity and semantic similarity; similarity of meaning seemed to be important for long-term and acoustic for short-term memory,<sup>145</sup> and I suppose in a way I have been doing work on that sort of thing ever since. In terms of long-term memory the unit has had, I think, quite a lot of involvement, some of the time with very practical issues. So we were asked to help the military improve face recognition and it became clear after a while, as this was during the early Northern Ireland troubles, that the purpose was to improve the military's chance of catching terrorists, and I can remember having a real crisis of conscience. 'Could this be used to suppress civilization as we know it? and so forth.' In fact it turned out that we discovered that it was rather hard to teach people to improve

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<sup>141</sup> Atkinson R C, Shiffrin R M. (1971) The control of short-term memory. *Scientific American* **225**: 82–90. *idem* (1968) Human memory: A proposed system and its control processes, in Spence K W, Spence J T. (eds) *The Psychology of Learning and Motivation: Advances in research and theory*, vol. 2. New York: Academic Press, 89–195.

<sup>142</sup> Conrad R. (1964) Acoustic confusions in immediate memory. *British Journal of Psychology* **55**: 75–84.

<sup>143</sup> Conrad R. (1963) Acoustic confusions and memory span for words. *Nature* **197**: 1029–1030.

<sup>144</sup> For example, Baddeley A D. (1966) Short-term memory for word sequences as a function of acoustic, semantic, and formal similarity. *Quarterly Journal of Experimental Psychology* **18**: 362–365.

<sup>145</sup> Baddeley A D, Dale H C A. (1966) The effect of semantic similarity on retro-active interference in long- and short-term memory. *Journal of Verbal Learning and Verbal Behavior* **5**: 417–420.

their memory for faces, and it was very easy to make it very hard to recognize people by using disguise. We had a number of people around the unit wearing false beards, and wigs, and moustaches, serving as targets for recognition experiments, and one of the things that emerged was that if the photograph was in three-quarter rather than full face, performance was rather easier.<sup>146</sup> And so we thought ‘Aahhah!, this is something that we should definitely develop’.

I happened to notice in the National Portrait Gallery that nearly all portraits were in three-quarter view, and I thought, ‘Ah those portrait painters knew a thing or two’. I tried to develop it; I thought maybe all passports should be three-quarter, so let’s just check it out. A number of us, with help from the *Cambridge Evening News*, set up a study whereby in each of a series of small towns around Cambridge; on a Friday evening, there would be a photograph of targets in different views, profile, frontal or three-quarter, and then our targets would zoom around Cambridgeshire, walking around a series of different towns, each of them having seen a photo in a different view. The newspaper assured us that, ‘All you need to do is tell people to ring in if they see the target, you will get huge numbers of responses.’ I sat by the telephone with my son to help me from nine in the morning until five-thirty that evening, when I got my first and only response; it wasn’t three-quarter. Eventually, three studies later, we settled for using our Cambridge subject panel, who we *knew* would try their best. We had our targets going round and round a circuit in the centre of Cambridge, and the people who were spotting them doing the circuit in the opposite direction, and we finally got some data. It was a very, very weak effect. We also had another interesting task, that was to evaluate a course that claimed to train people to be better at recognizing faces. It used a procedure, a very ancient procedure, developed initially by Leonardo da Vinci, of ‘reading’ a face by breaking it into features. We discovered that after this three-day course, people were somewhat *worse* at recognizing faces, so at least we saved somebody some money.

More recently, I think the research on memory has been more concerned with memory breakdown in closed-head injury, in amnesia, and in rehabilitating memory.<sup>147</sup> That, I think, has been quite a major feature over the last few years. Once again, I think it has been possible to take a practical area, like closed-head injury, which didn’t look very promising theoretically but was important, and actually put some resources into looking at it and finding that there are interesting things that could be gleaned from it. So in general, I think the unit is an institution where, at least in the old days, and I hope still, you could afford

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<sup>146</sup> Patterson K E, Baddeley A D. (1977) When face recognition fails. *Journal of Experimental Psychology: Human Learning and Memory* 3: 406–417.

<sup>147</sup> Wilson B A, Patterson K E. (1990) Theory and practice in rehabilitation. *Journal of Applied Cognitive Psychology* 14: 247–260.

to take a relatively long look at things and ask questions that were not necessarily going to pay off in the short term. They might be focused on what appeared to be rather mundane practical questions, but could in the long term prove to be theoretically enormously fruitful. I think that's one of the major advantages of an MRC unit.

**Gregory:** Thank you very much. I must say I think your comments ought to go into a chalice, it's so important to have a team that know each other, and joke with each other, and challenge each other, over many years, and things pop up. On your point about the three-quarters face, what do you think about silhouettes? I mean in the eighteenth century you got silhouettes always from the side, but very characteristic surely?

**Baddeley:** Yes, in fact profile is probably the hardest to recognize. I later discovered why a three-quarter view was used by talking to a portrait painter, Professor Lawrence Gowing at the Slade. He told me that the answer to my question of why three-quarter views are so common was very simple. If you look at modern portraits, they are often full-face; the reason is that it's very hard to do a full-face unless you take a photograph. Three-quarters is easier still, and that's on the whole why artists used to do that.

**Logie:** I wanted to add to the comments about the way in which the environment doesn't always collaborate or cooperate with our attempts to understand it, in relation to the study Alan mentioned of looking at three-quarter view versus frontal profiles with the *Cambridge Evening News*. A major debate arose during the mid- to late-1980s in cognitive psychology, and particularly in memory, as to whether studying memory in the laboratory was the best way to discover generalizable principles about how memory operates and functions as opposed to studying memory in the real world, memory for real-life events, and so on. Part of the debate focused on the extent to which the real world is really rather too complex, quite often, for us to study it in its naturalistic setting, if we are to take generalizable principles from our studies. One of the other important points that came from that sort of work was the object lesson that only when we were in a position to control the external environment sufficiently to exclude confounding factors, did we begin to get the effects that had been obtained in the laboratory, suggesting that, yes, we can look at applied problems, we can look at practical problems. Often we can gain a lot more in terms of insight in both those problems and in terms of developing theory by studying those practical problems in controlled settings, rather than naturalistic settings. That debate in itself is continuing. The APU did make a major contribution to it, particularly in the area of memory.<sup>148</sup>

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<sup>148</sup> Professor Robert Logie wrote: 'A review of the contribution from the APU and from Alan Baddeley in particular is given in Baddeley A D. (1990) *Human Memory: Theory and Practice*. Hove: Lawrence Erlbaum Associates.' Note on draft transcript, 28 November 2002.

**Della Sala:** I just want to add to what Alan [Baddeley] said about the benefit of following up from the practical studies, and these applied studies so that they eventually become interesting theoretically. To say that also the other way round implies it's a two-way interaction. A theory which doesn't appear to lead anywhere might prove useful and interesting questions later on, even if they are not immediately perceived as applied, might eventually become applied. So a question itself, if you think it is interesting, is worth pursuing.

The collaboration between Alan and myself and colleagues in the medical school proved that this was the case when we applied his ideas, and tried to find out the displaying deficits presented by patients; when these ideas were presented theoretically we didn't have any clues about their potential use with the patient. So it really is a two-way interaction from practical to theory, and maybe from theory to practical issues.

**Hitch:** I would just like to echo what Alan was saying about the continuity of work on short-term memory at the unit. I think Donald Broadbent's book in 1958 really was monumental. The impact of that book can't be underestimated. The APU was certainly a centre of expertise in research on short-term memory. Conrad and Alan were there in those early days, as well as Donald. Although as a topic it has waxed and waned a little bit in popularity, it's important to emphasize that it still continues to this day, with the likes of Dennis Norris, Mike Page and Rick Henson. A couple of those have recently left, but they have all been doing very sophisticated work, modelling the very phenomena that Conrad, Alan and Donald Broadbent were investigating 50 years ago. I think that really is quite remarkable.<sup>149</sup> I can't think of many places in the world where you would have found that continuity of expertise.

The other point I would like to make is that this work has always been strongly linked with other areas. We have heard about John Morton's logogen model of word recognition, which is justifiably very famous, but this also included memory. Although the part of his model he termed 'pre-categorical acoustic storage' suffered strong challenges a few years ago, the data he collected were very, very important, and I don't think his theoretical ideas have yet been superseded.<sup>150</sup> Again we heard earlier about the link between short-term memory and learning languages. Those sorts of links between memory, language, and cognition, which Alan himself has done so much to investigate, have flourished throughout the last 50 years and continue to do so.

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<sup>149</sup> See Page M P A, Norris D. (1998) The primacy model: a new model of serial recall. *Psychological Review* **105**: 761–781. Henson R N A. (1998) Short-term memory for serial order: the Start-End Model. *Cognitive Psychology* **36**: 73–137.

<sup>150</sup> See note 128. Crowder R G, Morton J. (1969) Precategorical acoustic storage (PAS). *Perception and Psychophysics* **5**: 365–373. Morton J, Marcus S, Ottley P. (1981) The acoustic correlates of 'speech-like': a use of the suffix effect. *Journal of Experimental Psychology. General* **110**: 568–593.

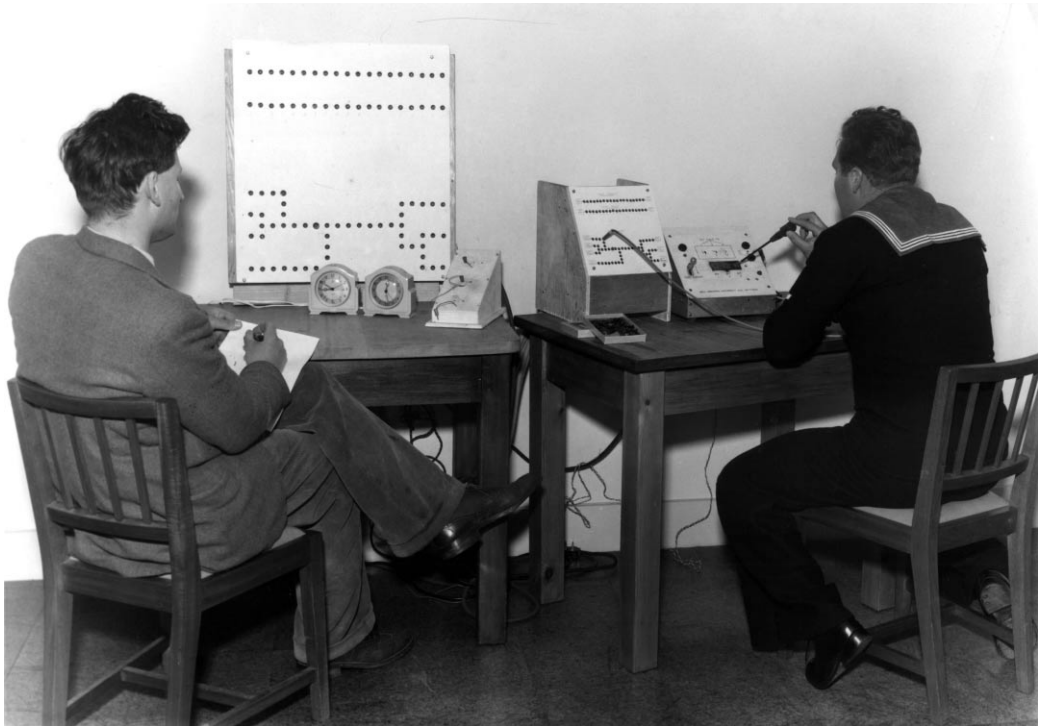


Figure 17: Dr Harold Dale running an experiment on fault-finding in electrical equipment, c. 1957.

**Duncan:** I would like to contribute a bit. This is personal experience of the limits of memory storage. When I became a selection consultant I used to interview up to say 25 people a week and for the first four years of operation, I could remember the details of everyone that I interviewed, but after four years I suddenly discovered that it was rather like a pot that is full and won't take any more, and I had to change my style completely to record things on paper, and deliberately forget everything that I didn't have to remember and I continued that way. The only problem is, I think, with ageing; this system sometimes gets out of control. In my experience it helps to maintain the short-term memory and not put too much strain on the long-term memory, but this is anecdotal information. I hope it makes sense to the rest of the people here.

**Groeger:** A couple of other name-checks are appropriate, in terms of other important areas of memory research at the unit that haven't been mentioned. One is autobiographical memory work by Conway and Bekerian<sup>151</sup> amongst others, looking at how people

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<sup>151</sup> Conway M A, Bekerian D A. (1987) Organization in autobiographical memory. *Memory and Cognition* 15: 119-132.

remember their own lives, an area that the APU has, also under Alan's direction, championed to some extent. Another piece of work worthy of mention is Alan's own research on remembering information acquired in different circumstances. This work on context effects, using divers in the west coast of Scotland still stands as one of the strongest, most reliable demonstrations of changing the retrieval context for information learned in a different context.<sup>152</sup>

**Marslen-Wilson:** I can't help adding that autobiographical memory continues to be a very lively topic to this day, in particular in the hands of Kim Graham and John Hodges, studying another group of patients. These are patients with selective atrophy of the temporal lobes, often bilateral, who show what has been a very interesting reverse autobiographical memory effect. The typical effect is that as memory disorder sets in, you start to have problems with recent memories, and you are still able to recall early memories. There is a group of patients who show the opposite effect – they recall autobiographical memories in the last two years better than early ones and that's really triggered a very interesting set of theoretical and clinical questions. Kim Graham has now gone on to study these questions in a scanner and you can indeed see that when people are recalling different kinds of autobiographical memory; you see differential involvement of different parts of the brain. This can be related on the one hand to progressive lesion data and on the other to the cognitive theory.<sup>153</sup> So, another example of continuity.

**Gregory:** I think we should perhaps move on to the next topic, which is perception, and Peter McLeod is going to begin.

**McLeod:** Well, I think a lot of the individual discoveries in perception made at the APU have been described by Ivan and Pat and other people earlier, so rather than run through a list of those again, I think what I will do is try to describe a bit about what it felt like to be working at the APU, which I think is one of the things that our organizers hoped we would do. I worked there during the 1970s and I think one thing that was clear to me is that we collectively thought that we were the best. We thought that the unit was a power unit for interesting experiments and for theoretical novelties and so on, and I think it is certainly true that a lot of the most influential people, in at least what you might call information-processing psychology, did go through the unit at some time during their careers. Now I

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<sup>152</sup> Godden D R, Baddeley A D. (1975) Context-dependent memory in two natural environments: on land and underwater. *British Journal of Psychology* **66**: 325–331. The divers were from the Clydebank, East Kilbride and Stirling Universities' subaqua clubs.

<sup>153</sup> Graham K S, Pratt K H, Hodges J R. (1998) A reverse temporal gradient for public events in a single case of semantic dementia. *Neurocase* **4**: 430–443. (CBU 3749) Simons J S, Graham K S, Owen A M, Patterson K, Hodges J R. (2001) Perceptual and semantic components of memory for objects and faces: a PET study. *Journal of Cognitive Neuroscience* **13**: 430–443.





Figure 18: Dr Ivan Brown testing a subject in research on the use of subsidiary tasks to measure the effects of small amounts of alcohol on drivers' 'spare' mental capacity.

think you saw there were a lot of very aggressive and competitive people, and the time when you really saw them in action was at the weekly seminar, the Chaucer Club,<sup>154</sup> on Thursday afternoons, and these were usually very competitive affairs. The idea often seemed to me to be to try to, as it were, tear the visiting speaker to pieces, and if there was anything left at the end of this experience, then that must be some pretty good stuff, and you could well take this home. I certainly found when I went to America, that the attitude seemed to be,

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<sup>154</sup> Professor Peter McLeod wrote: 'So called because the APU was in Chaucer Road.' Note on draft transcript, 25 November 2002.

the idea was, to try to find remarks to support the position being put forward by the speaker. I found this a bit of a contrast to the attitude at the APU, where everyone seemed to be trying to prove that they were cleverer than the speaker and could disprove whatever it was he was trying to say. I think one of the reasons, and undoubtedly the overwhelming presence at the unit at the time that I was there, was Donald Broadbent, and this was in part because he was probably the world's leading theorist in the information-processing approach to psychology. And he was undoubtedly extremely knowledgeable and was very clever. He wasn't very keen on people disagreeing with his theories, and if you had the temerity to suggest that one of his favourite ideas was wrong, he would turn his prodigious knowledge of the literature on you, to prove how wrong you were. This could be a fairly harrowing experience for a young scientist, as I discovered early on. I think I might have tried it twice, but not more than that. [Rabbitt: More often than most of us.] It was certainly true in the part of the unit that I worked in, which was interested in perceptual and motor issues, that a lot of the drive seemed to come from either the Royal Naval Personnel Research Committee (RNPRC) or the Army Personnel Research Committee, which to some extent we worked for. This was an interface between the lab and people in the military, and a lot of the human factors problems that we worked on seemed to come from there. So the RNPRC for example was particularly interested in simple decision making – how people did target detection on radar screens and so on, and the effect of stresses such as noise, boredom, sleep loss – the sort of effects that those stresses had on simple decisions. Hence the book that Donald published at the beginning of the 1970s, called *Decision and Stress*,<sup>155</sup> and it was those sorts of experiments that led them to be seen as crucial issues in understanding information processing. Probably from the outside, most people would not think that stress was a particularly major topic, but what Donald did was to show how, whatever starting point you came from, you could spin global information-processing theories based on these often very simple experiments. The other major input, at least as far as my own work was concerned was with the Army Personnel Research Committee. Their main interest at that time was on the design of simple control systems, particularly manual devices for doing things like hand-guided anti-tank missiles, and so on, and what was the best way to design the man–control interface. And of course the main person at the unit who worked on this was Christopher Poulton and his work in this area was summed up in his book, *Tracking Skill and Manual Control*.<sup>156</sup> This turned out to be an area in which it was very easy to demonstrate effects of experimental design on the results that you got, and he developed a great interest, indeed some might almost consider it an

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<sup>155</sup> See note 101. See also Broadbent D E. (1953) Noise, paced performance, and vigilance tasks. *British Journal of Psychology* 44: 295–303.

<sup>156</sup> Poulton E C. (1974) *Tracking Skill and Manual Control*. New York and London: Academic Press.

obsession, in the way that experimental design influenced experimental results – in particular asymmetrical transfer between conditions and range effects. Unfortunately I think his rather idiosyncratic personal style meant that this work didn't have as much influence on the psychological world outside the APU as I think it should have, but it certainly had a very major effect on anyone working with Christopher in the APU, and so we always used separate group designs, having had the bad effects you got if you used so-called balanced designs drummed into us by Christopher. The fact that we worked for the RNPRC led to one of the most distinctive features of the unit, which has in fact been mentioned earlier, that is the presence of the sailors. So once a fortnight you got a new group of six sailors and you could test them every day for two weeks, so basically you were given ten sessions, or nine if you were worried about the sort of demob happy effect on the final Friday. What you got was these endless experiments, at least all my experiments anyway, involving people tested every day for nine days. The people tested, always naval ratings, were most unlike any other psychological group used anywhere else, but because you had to use separate group designs, you could only have six people and it took you a fortnight to run one condition, provided you could get an effect that was significant when you had six subjects in each group. So you needed to work on very big effects if you were going to get your experiments over in any reasonable length of time. If you needed 18 people in each group, in order to show a significant effect, you would take six weeks to run each condition, so this encouraged us all to find very large effects to work on, so we only needed to have six subjects in each group.

**Della Sala:** I just want to add to what Peter [McLeod] has said, because I had a different experience. I never worked there; I was visiting the unit. And although I did find it a bit intimidating giving talks, I found a very friendly environment, and I found it an enormous bonus in having people around; I could knock at their door and ask questions. I was young at the time and I found it extremely helpful to have loads of people around with different expertise. It was not only the experience of intimidation during a presentation; there was also the positive atmosphere of having loads of people to ask questions of, which in the place where I came from was missing completely. Just to add that it was a positive atmosphere as well.

**McLeod:** One of the things I meant to say was that this weekly meeting, the Chaucer Club, everybody, the entire unit, always came to it, and that was when everyone, as Sergio was saying, got together and we all discussed the same issues, because we were all interested in the same things. In the lab I work in now, groups are much more separate, and I think it's become to some extent a bit more like that in the current unit, separate groups working on separate things. I found it very stimulating to have an entire unit in which everybody was interested in the same theoretical agenda.

**Tyrrell:** There are certainly similarities with other units such as the one I was privileged to work in, getting together and talking about the things that enthused and interested us all. But you haven't mentioned at all the relationship between individual scientists or groups of

scientists, and the people we would call your volunteers – your groups of individual subjects. We always called them volunteers, because they weren't sent from anywhere to us – they chose to come. We thought of them as partners, because they contributed a lot by their willingness and continuing to come and cooperate in the projects in one way or another. I just wondered whether that ethos was important and whether it has continued.

**Rabbitt:** I was once bitten by a sailor!

**Gregory:** Can I add a bit on to what you have said? What happened when you didn't want the subject to know what you were doing? What was the ethics on that? Could you hide what it was really all about, or what? I mean sometimes you gave the game away if they knew too much. How did you control that?

**Tyrrell:** We started by telling them before they arrived, in the initial documentation, that this was to be a scientific experiment involving simple clinical observations (and various harmless lab tests) and that it would be double-blind throughout. There would be no secrets in the end, they would all be told what the object was and how it was going to be evaluated and what they had done themselves, but they understood completely that at the time of the experiment they would not know and neither would the people who were administering the tests, or whatever. They weren't supposed to know what was happening, whether they were affected or not, whether they were treated or not.

**Gregory:** Well, I know when Chris Poulton and I were working on blinking and tracking, we had a book of EEG lying negligently on the table. We were recording the blink rate electrically, and we pretended we were measuring their brainwaves. Fibbing, of course, because we didn't want them to think about their blinking. Now is that generally done or not?

**Tyrrell:** With the psychological testing that Andy Smith did,<sup>157</sup> they weren't told exactly the purpose of the individual experiment and he did try to control things like the time of day and the amount of motivation that they had (and that's difficult to do). But in principle, they learned afterwards. Another thing was that for some of the tests that involved personality and so on, they had questionnaires to answer. These were littered with dud questions, of course, but they were only given a general feeling of the sort of thing that the questionnaire was about: mood and attitudes and these sorts of things.

**Brown:** If we are into true confessions, I have one. And you have already mentioned the experiment, I think. We were looking at the effects of small amounts of alcohol on driving

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<sup>157</sup> See note 92. See also Totman R, Kiff J, Reed S E, Craig J W. (1980) Predicting experimental colds in volunteers from different measures of recent life stress. *Journal of Psychosomatic Research* **24**: 155–163. Broadbent D E, Broadbent M H, Phillpotts R J, Wallace J. (1984) Some further studies on the prediction of experimental colds in volunteers by psychological factors. *ibid.* **28**: 511–523.

performance, this was before the breathalyzer<sup>158</sup> came in, and I set up an experiment that I ran around the streets of Cambridge. I was using the dual-task method for measuring people's performance, giving them a subsidiary task to do as they drove around,<sup>159</sup> and I set this up with a local car club. All car clubs probably met at a pub somewhere in Cambridge and we would go along and the people who volunteered would be there and I would say, 'While I am telling you what I would like you to do, because we are developing this method of measuring drivers' performance, would you like a glass of sherry?' Christopher Poulton, by the way, paid for the sherry, not the Medical Research Council, and I was able to do a proper experiment by treating the subjects, some of them to one glass of sherry, and some to two glasses of sherry. Naturally I had to join them, so it didn't look phoney, and then we went out and ran the experiment. [See Figure 18, page 64.] They would show that even after one glass of sherry there are ways of measuring small changes in drivers' reserve capacity, as we called it at the time, but it was a bit naughty.<sup>160</sup>

**Logie:** I just wondered if I could comment a little bit on the subject panel at the APU, which in fact I think was one of its other major contributions, particularly to experimental psychology, because much of experimental psychology elsewhere in the world was the psychology of the undergraduate psychology student, and a major contribution of the APU was developing a panel of volunteers from the general public, primarily housewives, but not exclusively so. This allowed some significant generalizability of findings that had been reported elsewhere, which exclusively used undergraduate psychology students, and I think that in itself was a major contribution that the APU made. The model of developing panels of volunteers from the general public was adopted in several other university departments in the UK several years later.<sup>161</sup> It was clearly a way to go forward, looking at generalizability of these results. In terms of the relationship between the experimenter and the subject, having tested many, many, many subjects at the APU during the 1980s, one of the initial problems, particularly for panel members that were new to the whole experience of taking part in psychology experiments, was to try to dissuade them from their view that this was some sort of therapy session. Much of the initial discussion with a subject panel member tended to be to listen very patiently to their medical history and the various problems that they had in their life, before I curtailed this and started to get them to focus on the

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<sup>158</sup> The breathalyzer was invented by Dr Martin Wright and its use was introduced by the Road Safety Act in 1967. The Government had asked the MRC to look into the subject of alcohol and driving in 1960. See Figure 18.

<sup>159</sup> Brown I D. (1962) Measurement of the 'spare mental capacity' of car drivers by a subsidiary auditory task. *Ergonomics* 5: 247–250.

<sup>160</sup> Brown I D. (1970) The effect of small amounts of alcohol on car driving. Paper presented to the British Psychological Society, London, in a symposium on the work of the Applied Psychology Unit.

<sup>161</sup> For example, the Department of Psychology at the University of Aberdeen established a panel in 1970.

experiment in hand. Certainly several of the people felt that the experience of taking part in the experiment was worthwhile, even if their initial expectations of providing a solution to their problems were not entirely met.

**Barnard:** I wouldn't say they were random samples – Cambridge being Cambridge. I remember when we were doing very early research on HCI, we set up a decoding task to look at command languages where we presented sentences with scrambled words, and the problem for the participant (as we now have to call them) was to unscramble the message and turn it back into an English sentence. At the end we debriefed the subjects to tell them what the experiment was really about, and why we had done it that way. One lady came into the lab, and did the task very effectively, and when we debriefed her she said, 'Well, I think you have done jolly well. You know I did work at Bletchley Park during the war!'

**Richards:** A more general point that seems to me to be emerging is this curious split between what psychology has really been and the public image of psychology that's still trotted out quite readily by politicians, lawyers, and so on in court cases. I begin to find this rather puzzling, because quite clearly comparable work to the APU's done in the USA and so on, shows that psychology has absolutely penetrated into everything from the development of military psychology to the design of traffic layouts. Absolutely all areas of modern technological society have had psychological input somewhere along the line, and yet if you ask the public what their image of the psychologist is, they still think of Freud or something like that. Or counselling. Now I am kind of curious as to any reflections that people may have as to why the public image has remained so inaccurate in the sense of how people perceive the discipline, given its penetration in this way. I am sorry to take this away from these entertaining reminiscences about the subject.

**Gregory:** That would have to be an important part; it even goes into pop art. Take Bridget Riley – that all comes from perceptual experiments on repeated lines, like MacKay Rays and so on, and it started in the lab, not in the art studios, of course.

**Duncan:** I was one of the people who, with John Morton, established the BPS Standing Press Committee [in 1970]. We were greatly disturbed by the way in which when one issued releases of conference papers; the ones that were published tended not to be the ones that the society would have liked, and this started in 1970. I notice that John Morton is still involved in the contacts with the press. The one thing that he developed was a lexicon of key words where we would have a psychologist's response for the questions that the press would ask. Our first discovery was that their lexicon was quite different from ours – that the questions they would ask tended not to be in our dictionaries, such as, 'What is the psychological significance of hair' (sic!). But since 1970 we have managed to develop a much better system of being proactive, of getting there first with the comment and the information, and so the press coverage is very much better than it used to be and it's much more comprehensive than it used to be, much more responsible. But then there are still



Figure 19: Dr Peter Colquhoun performing a vigilance task simulating industrial inspection.

papers released by the press themselves. I think the prize example was when Harry Kay's presidential address on accident proneness got put in with a paper on why pretty girls laugh at dirty jokes, and the two went round the world together, to the great embarrassment of Harry Kay.<sup>162</sup> We managed to prevent that from happening again, but it is something which needs permanent attention, and I don't know whether Stephen White [of the British Psychological Society] would like to speak on this one, because he is nowadays in charge of quite a significant operation of keeping in touch with the press.

**Bunn:** I wonder if I could just go back to Graham's point on that issue. One of the problems that the NIIP had was a prohibition against publicizing their results, because they had worked for individual companies, and the work was regarded as private property. Did the APU have similar frustrations in publicizing their work, given that it was done for private clients?

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<sup>162</sup> Kay H. (1972) Psychology today and tomorrow. *Bulletin of the British Psychological Society* 25: 177–178. *idem* (1978) Accidents: some facts and theories, in Warr P B. (ed.) *Psychology at Work*. Harmondsworth: Penguin Books, 97–117. Mr David Duncan wrote: 'At this time Harry Kay was the foremost British authority on accidents and their causes.' Note on draft transcript, 12 December 2002.

**Brown:** Well, I can't remember the details, but I remember some of the research that was carried out for British Telecom was run by a PhD student, I think, who was visiting from Australia. He was actually doing a Cambridge PhD so of course his thesis had to be put in the university library and for a long time British Telecom would not allow him to publish certain parts of his research, because it was commercially sensitive. I think eventually, maybe Alan knows some of the details, he probably altered some of the details and made it satisfactory. What made it worse was that when he left the unit, he then went to work for Bell Labs in the States. But I think he had got his thesis in the university library by then. Have I got that right?

**Baddeley:** Yes, you have. Fortunately, that was pretty much the exception. I think while I was director there were only two cases I can remember. One of them was some work done for the Army, I think, before I arrived, on drunken driving in army bases in Germany, and they refused to have that published for political reasons. But otherwise, we simply made the point that it is a requirement of the Medical Research Council that if we do work, it is published, and on the whole I think that wasn't problematic. Maybe, Phil, you did a lot more work for private industry than I did, was that a problem?

**Barnard:** It was never really a major problem. There were one or two instances where they didn't want us to talk about or label their products, if we had done some particularly damaging review of some of the effects that we were getting. To my recollection we were never ever stopped from publishing the basic stuff. It was always in the research contract or the agreement, or the funding arrangement, that everything that we did had to be publishable, and for the most part, I think for commercial reasons a lot of the research wasn't paid for. So in human-computer interactions, for example, I think the companies genuinely wanted approaches and ways of thinking things, they didn't want an answer about a particular product, so they were essentially paying up to provide them with ammunition – persuading their own companies that they should be doing more research of that kind. They weren't actually buying a result – they were trying to make a difference to the way in which their companies, the context, functioned at that time, which of course was an interesting, but very different, channel.

**Richards:** Just on a point of information – surely this does not apply to the post-war work for the military? Presumably a large amount of military-funded work up to say 1960 or so would have been classified, and certainly in the archives of offprints that the unit received, of which I have managed to salvage some on the aviation topics.<sup>163</sup> There are lots of things

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<sup>163</sup> For example, see Hick W E. (1945) *Friction in Manual Controls*. Report no. 18. Cambridge: UK Medical Research Council, Applied Psychology Unit. Bartlett F C. (1943) *Instrument Controls and Display-efficient Human Manipulation* (Report no. 565). London: Medical Research Council, Flying Personnel Research Committee.



emanating from Farnborough and so on, which were originally restricted or classified. I was told informally, you know, periodically ‘the men in black’ came and sorted through the files and took some of the stuff away, and there are even gaps in those folders, so presumably there was some control in that, as opposed to commercial companies.<sup>164</sup>

**Baddeley:** I can’t really speak for the very early days after the war, but in general what was required was that one published it in a form that did not give away useful information. So you might be working on missile X, you could describe it as a tracking system with these specific characteristics, together with other conditions, so on the whole that wouldn’t be problematic, but Peter might be able to comment on that.

**McLeod:** Certainly all the work that I did was published, just in the way that everyone else has described, or perhaps I should say submitted for publication. I think the questions were always framed in such a way that they became a rather more abstract general question rather than one specifically to do with a specific military system. Although, in fact, I do recall now doing some work on the influence of ship motion on various sorts of control, and the tracking task that we used did indeed include the control law that was then used by the current naval anti-air missile. We simply just stated what the control law used was, as if it were an entirely arbitrary one that had just been picked at random and didn’t mention why we had chosen this particular one. I don’t know how typical that was.

**Baddeley:** I should add a note to that. I think some of the tracking involved building a small railway that ran from one side of the garden to the other. It was called the toy train, and the controller would sit in the carcass of an ancient baby Austin at the bottom of the garden, and would either control it through vision, or through some form of indirect feedback. We were forbidden to say that this was anything to do with missile control. I can’t remember what cover story we told, but it was all ultimately publishable with suitable circumlocutions.<sup>165</sup>

**Brown:** Could I add to that? It was sometimes difficult to keep this out of the press, because it was all very well within the unit to call this the toy train project, but unfortunately we had to set up the testing of subjects a week ahead. Max Hammerton, who was running this project, I think, put a notice on our noticeboard in the foyer, which said the toy train project will start next week. One of the *Cambridge Evening News* reporters wandered into the unit – in the habit they had of collecting data on what we were doing – and he said

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<sup>164</sup> Mr Kevin Symonds, Librarian of the MRC Cognition and Brain Sciences Unit, wrote: ‘With confidential material it seems that every so often people from the Ministry of Defence came to the unit, took some things from locked cabinets and either give them to us as declassified or took them away to be destroyed.’ E-mail to Mrs Lois Reynolds, 3 December 2002.

<sup>165</sup> Hammerton M, Tickner A H. (1967) Visual factors affecting transfer of training from a simulated to a real control situation. *Journal of Applied Psychology* 51: 46–49.

nothing. But I think within the next day or two the *Cambridge Evening News* had a headline, 'What on earth is the MRC doing spending its money on toy trains?' I think a rocket came down from the Ministry of Defence the next day. Quite how we got out of that I don't know; I think the game was nearly up, but not quite.

**Duncan:** My understanding of the situation in relation to the National Institute of Industrial Psychology (NIIP), is that it was quite a bit of ancient history, and that between the wars they had a symbiotic relationship with the Industrial Health Research Board (IHRB), in which they lent investigators to the IHRB and this helped the NIIP support their staff. The understanding was that anything that was IHRB was publishable, but that there was something of a problem if an individual firm would pay for this, and that would then be their property. This really carried on right to the end of NIIP. But as an investigator, I was able, at will, to refer to a tremendous library of reports of companies, which went right back to the 1920s, about NIIP procedures and techniques. This was the archive that was eventually shredded on the grounds that it was the property of the firms that had paid for it. If it was MRC-funded, the understanding was that anything that was publishable would be published, and that's what we held to. But the commercial pressures, I think, still remain that there are things that you cannot publish.

**Gregory:** Well, there are constraints from patents. If you have an idea, you can't patent it after you have published it. The MRC used to pay for patents. I actually took out about 30 patents, paid for by the MRC through the Department of Scientific and Industrial Research (DSIR) at that time. None of them made a great deal of money I should say, but the inventor was allowed to keep some of the royalties.<sup>166</sup> It was a kind of a mixed ownership thing. I don't know if you got mixed up in that one. It was a very interesting situation, but terribly difficult to get firms to take the thing up, if it wasn't invented by them, of course.

**Duncan:** We had a problem once on the BPS [British Psychological Society] Press Committee, when someone discovered a PhD study that had not yet been published, and there was a great temptation that this was something really new and vigorous, and we promptly had to shut the member up, to prevent it from getting into the press.

**Groeger:** A couple of reflections on unit life that might otherwise perhaps not be mentioned. One of the things that struck me when I came there from Ireland in 1985, as a new PhD, was how similar the unit was to a library. Wandering up and down the corridors, the names on the doors were very much like the carrels of a library in terms of the number of important psychologists who were at the unit. Even though Peter mentions how hostile seminars were,

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<sup>166</sup> See also Tansey E M, Catterall P P. (eds) (1997) *Technology Transfer in Britain: The case of monoclonal antibodies*, in Tansey E M, Catterall P P, Christie D A, Willhoft S V, Reynolds L A. (eds) *Wellcome Witnesses to Twentieth Century Medicine*, vol. 1. London: The Wellcome Trust, 1–34.

the feeling one had, I think, of unit life in general was that there was remarkably less conflict than one would imagine, with so many outstanding researchers present. A number of things allowed a level of communication between people working on different projects. The religiously attended coffee and tea times, and indeed the recording and publishing of external contacts that colleagues had on a weekly basis. These were part of the way that the people with the intellectual attitude, mentioned by Phil earlier on, were jelled together.

**Rabbitt:** Perhaps Bob [Logie] will finish off as I drivel into silence. This is the memory of the course on sleep that I used to teach in Oxford, having left the unit, based largely on what went on in the unit. My recollection about the work on sleep was that it was largely done by Bob Wilkinson<sup>167</sup> and Peter Colquhoun [see Figure 19, page 70], who are not with us today. It was supervised by Donald [Broadbent], because they were both his research students, I believe.<sup>168</sup> And the sailors were invaluable, because they could be kept awake all night, being used to that in their profession, and they could be kept awake for more than one day at a time. So the sleep work arose out of, I suppose, a need for maintaining vigilance, both in the experimental and laboratory sense, and in the practical and naval sense at awkward hours of the day and night. The theoretical thread of the sleep work was strongly related to the effects of stress and arousal on people's performance, so that the resulting theories were quite complicated, and set the scene for much current work. For example the idea that resistance to stressors might be markedly affected by individual differences in the ability to achieve and to maintain arousal, and differences in basal levels of arousal came out of this early work on individual differences in resistance to fatigue and in the ability to sustain attention by Corcoran, Colquhoun and Wilkinson, and work on tolerance of noise stress by Donald Broadbent and his associates. All these profitable lines of research still continue and, in an historical sense, can be seen as the remote outcomes of the way in which the problems of vigilance and sustained attention were defined and approached in the APU during the 1950s and 1960s.

The same is true of sleep research, pioneered at the APU by Bob Wilkinson. Donald Broadbent took an interest in the effects of loss of sleep<sup>169</sup> because sleep-loss imposes stresses that can be countered by other stresses. For example the effects of sleeplessness are countered by noise, and loud noise as a stressor has paradoxical effects, increasing arousal and improving performance in the short term, but leading to marked performance loss if long

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<sup>167</sup> Wilkinson R T. (1957) The effects of lack of sleep. Doctoral thesis, University of Cambridge. See also APU Report no. 323. See also *idem* (1969) Sleep deprivation: performance tests for partial and selective sleep deprivation, in Abt L A, Riess B F. (eds) *Progress in Clinical Psychology*, vol. 7. New York: Grune, 28–43.

<sup>168</sup> Dr Alan Baddeley wrote: 'Peter did his PhD under George Drew, who was then at Bristol.' Comment on draft transcript, 12 March 2002.

<sup>169</sup> See note 101.

continued. The work on sleep extended naturally into other areas such as work on circadian rhythms. The most notable findings here were by Peter Colquhoun, and his very gifted then research student Michael Blake, who showed that clear diurnal fluctuations in efficiency of performance coincided precisely with concomitant fluctuations in body temperature, and that both body temperature and performance cycles differed characteristically between introverts and extraverts.<sup>170</sup> Another aspect of the sleep research, most energetically pursued by Bob Wilkinson, was the effects of sudden awakening – of obvious interest to the military and in both the Royal and Merchant Navy, where competence at responding to sudden emergencies may be a life and death matter

**Logie:** I can't comment. I might be better on clinical psychology.

**Gregory:** Who wants to add to that? Well, somebody might want to subtract from it. If not, would people agree that we open the discussion for ten minutes and have it on anything about the APU: what was done, what wasn't done, and indeed perhaps what should be done in the future, now that we have got all these wonderful techniques, like imaging. It's no longer the APU, but it's continuing in a very exciting way, with modern techniques.

**Richards:** I wonder if somebody could clarify something for me. Like many people I am interested in the history of psychology, in institutional history as such, about the relationships between the APU and the Department of Experimental Psychology in Cambridge – how it had developed – because obviously they come from a single root and my impression now is that there is a somewhat limited channel between the two units.

**Gregory:** I must say I was in both and I found that the relationships were incredibly good, and there really wasn't a problem, I thought. Was that other people's experience or not? Not quite, I gather.

**Baddeley:** Well, I think there is some interesting history buried away in the APU files of correspondence. During the early years, Mackworth and Zangwill were both starting up, each with half of Bartlett's empire.<sup>171</sup> I think in the early days both were a little worried that the tail might wag the dog, but were not clear which was the dog and which the tail. Now in fact I think things improved later, and when I first went there in 1958, there were good

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<sup>170</sup> Blake M J. (1967) Relationship between circadian rhythm of body temperature and introversion –extraversion. *Nature* **215**: 896–897.

<sup>171</sup> Sir Frederick Bartlett retired in 1951, succeeded as Director by Dr Norman Mackworth. Oliver Zangwill was Professor of Experimental Psychology and Head of the Department of Experimental Psychology at the University of Cambridge from 1952 until 1981, and also held a position at the National Hospital for Neurological Diseases and Blindness, Queen Square, London (later the Institute of Neurology). See Zangwill O L. (1967) The experimental psychology group, 1946–1958. *Quarterly Journal of Experimental Psychology* **19**: 368–370.

links in the sense that people at the APU were allowed to register for PhDs, although one had to wait around and convince Oliver [Zangwill] that one was a good chap, which is fair enough. My feeling is that, in general, it just gradually improved, and that where there were common interests in particular, then there was a lot of interchange, such as in the area of psychoacoustics, for instance, and in some of the clinical areas. Although Oliver tended to say, 'Well, we don't want to appoint cognitive psychologists, because there are lots of those at the APU'. Because there weren't too many cognitive people in the Department in the early days, there wasn't so much contact in that area. But I think over the years it has certainly continuously improved and, I don't know, I assume it is still very good. I think historically there was a slight problem of the two halves of the empire, but I am sure that gradually changed over the years.

**Gregory:** Of course in psychology at that time, one was very much under the thumb of the physiologists. We felt rather inferior to the physiologists, who were incredibly distinguished, with Lord Adrian and so on. That has improved enormously with the development of the Craik lab.<sup>172</sup>

**Marslen-Wilson:** Can I just add something about the relationship with the University Department? In my experience over the last three or four years there has been no hint of tension at all, and in fact I think we have strengthened our links, institutional and otherwise, especially where graduate training is concerned. All the graduate students at the APU, now the Cognition and Brain Sciences Unit (CBU),<sup>173</sup> are automatically enrolled as Cambridge students, indeed their entrance is handled through the Cambridge Board of Graduate Studies, so there's never an issue of whether they would be accepted by the Department. Also, as Alan has said, in the areas of common interest there is a great deal of interaction with psychology, as well as a lot of interaction and cooperative work with people in psychiatry. We also now run a joint graduate programme in behavioural and cognitive sciences. Obviously, being in Chaucer Road, we are a mile outside the centre, so we are not physically in the same set of buildings as Psychology or indeed any other department, but I don't think that distance is really seen as an obstacle. We do have one very great advantage and that is that people can park, whereas if you go to Addenbrooke's Hospital, or to the Downing site, you should either cycle or make parking reservations days in advance. I suspect that there were periods when relations between the APU and the Department were less close so that there was probably not that much interaction. I certainly remember, when

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<sup>172</sup> The Kenneth Craik Laboratory was the name given in 1976 to a refurbished laboratory, then known as 'New Agriculture', in the middle of the Downing site that was to house the visual scientists from the two Departments of Physiology and Experimental Psychology. The first Craik Club talk was given in May 1976. See Mollon J, Pelah A. (1998), caption to Figure 7 on page 12.

<sup>173</sup> See note 7.

I was a lecturer in the Department myself, wondering why there weren't more cognitive psychologists in the department. The feeling was that, 'They are all at the APU – we don't need to hire any'. I don't think people feel this any more and I don't believe that we feel we are under the thumb of physiology or whatever. We feel that we have our own strength. There's a good interaction between peer groups.

**Gregory:** Do the students get the benefit from both? I mean do the Cambridge students get supervision now, or not?

**Marslen-Wilson:** Yes, we offer supervisions on a voluntary basis and one quite popular thing is that people offer to supervise third-year projects for the students – we have quite a few of those. There's a lot of interaction at the graduate student level as well.

**Gregory:** That's great. Well, we have got about five minutes, just under. Does anybody want to develop those topics, or raise another one?

**Tyrrell:** I would just like to mention, perhaps as rather an outsider, some thoughts in response to what you asked about what should happen in the future? Because I think it has been immensely beneficial that what frankly many outsiders felt about experimental psychology (that it was rather esoteric, rather narrow looking), has been shown by the APU to be something which is a complex of a battery of techniques and ideas that can interact very creatively with other branches of medicine and science. And I think that this will have to go on. One of the things we were trying to do was to look at the mechanisms by which micro-organisms produce infections, and then produce changes in cerebral function and performance, and try to see and dissect those mechanisms. I now feel that one of these days, and we are probably already entering that stage, we will know enough about the neuropharmacology of normal brain function to be able to say what happens when an infectious process begins, and new cytokines emerge in the circulation, interfere with receptors and so on. I hope that in the future people will not say, as people said to us, 'Oh, that's an entirely different area of science – you mustn't have anything to do with it'. We will need to be prepared to let them interact. In that way I think we will go forward. It's probably very obvious to all of you, but for an outsider like me, it needs to be said.

**Lush:** Can I say something very briefly about the role of the MRC Head Office in all this? The council's policy always was to pick the brightest men and women and give them facilities to give effect to their own ideas, without being burdened by having responsibility to teach and examine. When trade unions, the armed services, and so on, approached Head Office, wanting research to be undertaken, we always insisted that there should be freedom to publish, provided one had highly satisfactory work. Then, as Dr Baddeley has said, there were ways and means of publishing without infringing the security classification and it wasn't really until after I left the Head Office in 1972 that the Government introduced the Rothschild machinery with a heavy hand and tried to tell researchers what they wanted to

be done and what results they needed.<sup>174</sup> Up until the Rothschild report the Council was free to do its own work, and publish as it wished.

**Gregory:** I think what you are saying is very important and I just hope it continues. I myself was supported for 20 years in Bristol at the medical school, doing anything I wanted to do. I had to say what I had done every five years – that was it – and it went on for 20 years. One was simply supported in whatever one wanted to do, and I think that's a marvelous thing to have, in a country that wanted to get on in science. The MRC is a really wonderful organization.

**Marslen-Wilson:** Well, as I am sure Alan will testify, they are still very generous, but they do like to see more than a progress report every five years. But it seems to me that one of the things coming out, that we are being reminded of today, is how essential a role the APU played over 50 years in the development of a scientific, mentalistic psychology, a psychology which took seriously the mind as an information-processing system, and so forth, in many surprising and unexpected ways. All sorts of extraordinarily important, influential bits of research emerged and developed within the unit. It would be nice to know what the recipe was that allowed this to continue for such a remarkably long period of time. It may be something as simple as funding for relatively large interdisciplinary groups to get on with areas of science where they play a large role in defining what's actually done, or it may indeed be because people were allowed to be rude to each other at seminars once a week, or it may be something about the interaction between having to do applied work and being forced to deal with real problems of cognitive activity, and reflecting on those and so forth, which is not of course what we do directly in the same way any more. But I think it's a very interesting question – just what made the APU such a success? But certainly we should be grateful to the MRC for providing the opportunity for it to be such a success.

**Gregory:** We should end by also thanking the Wellcome Trust for holding this meeting; it will be preserved in immortal prose for ever and ever. It has been a wonderful opportunity for all of us to meet each other again, to get a bit nostalgic, and to appreciate the fact that the APU was set up with the genius Kenneth Craik, who initiated it, Frederic Bartlett and all these wonderful people. I think our lives have been enormously enriched, and hopefully those of our students, and the world at large, from what the APU has actually achieved – which is an enormous amount. So thanks to everybody.

**Tansey:** May I add our thanks to those of the Chairman, to all of those of you who have contributed this afternoon. I would also like to thank Geoff [Bunn] and Daphne [Christie]

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<sup>174</sup> The key principle of Lord Rothschild's 1971 report on *The Organisation and Management of Government R & D* (Cmd 4814. London: HMSO), was 'that applied R & D, that is R & D with a practical application as its objective must be done on a customer-contractor basis. The customer says what he wants; the contractor does it (if he can); and the customer pays.'

for organizing this meeting. Their difficult job now starts with trying to edit the proceedings and to turn this into a readable typescript. They will be in touch with you over the next few months, and I certainly would like to thank Professor Richard Gregory for his excellent chairing and bringing us in right on time – just in time to enjoy a glass of, I am afraid it is not one glass of sherry, or even two glasses of sherry, but there will be a glass of wine next door. Thank you very much for coming.



## Biographical notes

### Professor Alan Baddeley

CBE FRS (b. 1934) was a member of the scientific staff of the MRC Applied Psychology Unit, Cambridge, for nine years from 1958, obtaining his PhD during that time. He left in 1967 to join the University of Sussex, later moving to a chair at Stirling University in 1972. He returned to the APU as Director in 1974, succeeding Donald Broadbent, who had moved to Oxford. After 21 years, he resigned the directorship and moved to his current post as Professor of Psychology at the University of Bristol in 1995. His interests are in human memory and in applying cognitive psychology.

### Professor Sir Joseph Barcroft

FRS (1872–1947) was reader (1919) and Professor of Physiology (1926–1937) in Cambridge, and was appointed director of the Agricultural Research Council's Unit of Animal Physiology in 1941. His research interests included studies of the properties of blood, especially blood gases and the oxygen-carrying function of haemoglobin, and studies on the physiology of the foetus. See, for example, *The Respiratory Function of the Blood* (1914) and *Researches on Prenatal Life* (1946). See also Roughton F J W. (1948–1949) Joseph Barcroft. *Biographical Memoirs of Fellows of the Royal Society* 6: 315–345.

### Dr Philip Barnard

(b. 1948) gained his PhD in experimental psychology from University College London and joined the research staff of the Applied Psychology Unit in 1972. He now works in its successor unit, the MRC Cognition and Brain Sciences Unit. Throughout his research career he has sought to develop integrated theories of human cognition and apply them to both technology design and issues in clinical psychology.

### Professor Sir Frederic Bartlett

Kt CBE FRS (1886–1969) was reader in Experimental Psychology, Director of Psychological Laboratory in 1922, first Professor of Experimental Psychology at Cambridge University from 1931 until his retirement in 1952 and editor of the *British Journal of Psychology* from 1924 to 1948. He is best known for his studies of memory and social psychology. His most significant and influential work is *Remembering* (1932), which examines the influence of social factors on memory in an experimental setting, and marked a break with the German tradition in psychology and the advent of methods to study higher thought processes without the use of introspection. The notion of schema or conceptual model originated with Bartlett. See also Broadbent D E. (1970) Frederic Charles Bartlett, *Biographical Memoirs of Fellows of the Royal Society* 16: 1–13.

### Sir Christopher Booth

Kt FRCP (b. 1924) trained as a gastroenterologist and was Professor of Medicine at the Royal Postgraduate Medical School, Hammersmith Hospital, London, from 1966 to 1977 and Director of the Medical Research Council's Clinical Research Centre, Northwick Park Hospital, Harrow, from 1978 to 1988, and Harveian Librarian at the Royal College of Physicians from 1989 to 1997. He was the first Convenor of the Wellcome Trust's History of Twentieth Century Medicine Group from 1990 to 1996.

### Dr Donald Broadbent

CBE FRS (1926–1993) joined the scientific staff of the Applied Psychology Research Unit, University of Cambridge, in 1949, was appointed director of the unit from 1958 to 1974 and a member of the external scientific staff of the Medical Research Council from 1974 to 1991.

He was a member of the Social Science Research Council from 1973 to 1975; Chairman of the ESRC/MRC/SERC Initiative on Cognitive Science from 1987 to 1991; and President of the British Psychological Society in 1965 and of Section J of the British Association for the Advancement of Science in 1967. See Weiskrantz L. (1994) Donald Eric Broadbent. *Biographical Memoirs of Fellows of the Royal Society* 40: 32–42.

#### **Dr Ivan Brown**

OBE (b. 1927) joined the unit under the direction of Dr Norman Mackworth in 1953, after National Service in the Royal Engineers and training as a scientific instrument maker. He obtained his BSc and PhD in Psychology externally while working at the unit. He specialized in the development of tests for the measurement of impairment from fatigue and cognitive overload among vehicle operators in transport systems. He managed consultancy arrangements between the APU and outside contractors such as the Post Office, British Telecom and the Transport Research Laboratory, and he was Extramural Professor of Traffic Science at the University of Groningen in the Netherlands from 1988 to 1991. He was Assistant Director to Alan Baddeley from 1974 until his retirement from the MRC in 1993.

#### **Dr Geoff Bunn**

(b. 1968) was British Psychological Society Research Fellow at the Science Museum from 1998 to 2001. Dr Bunn curated the British Psychological Society's Centenary Exhibition at the Science Museum, 'Mind Your Head', in 2001.

#### **Dr R Conrad**

CBE (b. 1916) was a student of Sir Frederic Bartlett and joined the staff of the APU in 1948. He was Assistant Director of the Applied Psychology Unit under Dr Donald Broadbent from 1958. He was noted for his applied research,

particularly for the Post Office and subsequently on the cognitive effects of deafness.

#### **Dr Kenneth Craik**

(1914–1945) was appointed the first director of the Applied Psychology Research Unit by the Medical Research Council in 1944. He died the following year after being knocked off his bicycle, aged 31. The Craik Laboratory at Cambridge is named after him. For a collection of his work, see Sherwood S L. (ed.) (1966) *The Nature of Psychology: A selection of papers, essays and other writings by the late Kenneth J W Craik*. Cambridge: Cambridge University Press, including Sir Frederic Bartlett's obituary notice. See also Zangwill O L. (1980) Kenneth Craik: The man and his work. *British Journal of Psychology* 71: 1–16. Gregory R L. (1983) Forty years on: Kenneth Craik's *The Nature of Explanation* (1943). *Perception* 12: 233–237. Cordeschi R. (1986) Kenneth Craik and the 'mechanistic tendency of modern psychology'. *Rivista di Storia della Scienza* 3: 237–256. A short history of the Kenneth Craik Club and some appreciations appear in Mollon J D and Pelah A. (1998), caption to Figure 7.

#### **Professor Sergio Della Sala**

FBP's FRSA FRSE (b. 1955) has been Professor of Neuropsychology and Honorary Consultant in Neurology at the University of Aberdeen since 1994 and the editor of *Cortex* since 2001.

#### **Mr David Conochie Duncan**

FSS AFBP's CPsychol (b. 1926) took his first step towards becoming a Chartered Occupational Psychologist, as a researcher in the National Institute of Industrial Psychology (NIIP), working on a Medical Research Council contract in 1953. Since then he has spent over 40 years as a chief executive in industry, applying psychology to business problems. He joined the British Psychological Society in 1956, and has been active

in developing occupational psychology as a profession, through a Standing Press Committee, Test Standards Committee, Division of Occupational Psychology, and finally as an Assessor of the Board of Examiners in Chartering. His biographical and research papers are held in the Duncan Archive at the Centre for the History of Psychology at Staffordshire University, and his library is held in the Duncan archive in the Department of Psychology at Bristol University.

#### **Dr Joan Faulkner**

(Lady Doll) (1914–2001) qualified in 1937, took the Diploma in Public Health in 1942, and joined the MRC staff in 1943 as research assistant to Dr Philip D'Arcy Hart. She joined headquarters staff in 1945 and retired as Senior Principal Medical Officer in 1979.

#### **Professor Lawrence Gowing**

Kt CBE ARA (1918–1991), a painter and writer on painting, was Keeper of the British Collection and Deputy Director of the Tate Gallery, London, from 1965 to 1967, Professor of Fine Art at the University of Leeds from 1967 to 1975 and Slade Professor of Fine Art, UCL, from 1975 to 1985.

#### **Professor Richard Gregory**

CBE FRSE FRS (b. 1923) Professor of Neuropsychology and Director of the Brain and Perception Laboratory at the University of Bristol from 1970 until his retirement in 1988, later Emeritus. He joined the MRC Applied Psychology Unit at the Department of Psychology, University of Cambridge, in 1953 as a research worker, receiving tenure in 1955. He then moved to the Department as a demonstrator and later lecturer, until his appointment as Professor of Bionics at the Department of Machine Intelligence and Perception at the University of Edinburgh in 1967. He was chairman of the department at Edinburgh from 1968 to 1970; the founder editor

of *Perception* in 1972; and has been President of the Exploratory Hands-on Science Centre since 1991 and of Section J of the British Association for the Advancement of Science in 1975, Section X in 1986 and Section Q in 1989 and 1990.

#### **Professor John Groeger**

(b. 1959) worked at the Applied Psychology Unit from 1985 to 1994, mainly on cognitive aspects of driving. In 1994 he moved to the University of Leeds, and was appointed Professor of Cognitive Psychology at the University of Surrey in 1995. His research continues to focus on cognitive aspects of real-world skills.

#### **Professor Graham Hitch**

(b. 1946) studied for his PhD in experimental psychology at the MRC Applied Psychology Unit from 1968 to 1971. He worked as a post-doctoral scientist with Alan Baddeley at the universities of Sussex and Stirling before returning to the unit in 1974. He was appointed to a lectureship at the University of Manchester in 1979 and moved to a chair at the University of Lancaster in 1991. Since 2000 he has been Professor of Psychology at the University of York. His research interests are in human memory and cognition.

#### **Dr Peter Hunter**

MRCP (b. 1938) qualified from Middlesex Hospital, London, in 1963 and was Consultant Physician at the Royal Shrewsbury Hospital, from 1974 to 1993. From 1994 to 1997 he read pharmacology at King's College London, as preparation for full-time research on the history of discovery of drugs and medicines in the modern era.

#### **Professor Harry Kay**

CBE PhD DSc (b. 1919) was a member of the Nuffield Research into Problems of Aging at the University of Cambridge from 1948 to 1951, acted as the psychologist of the 1949 Naval Arctic

Expedition, and was lecturer in experimental psychology at Oxford University from 1951 to 1959, and Professor of Psychology at the University of Sheffield from 1960 to 1973. He was President of the British Psychological Society from 1971 to 1972 and Chairman of the MRC Environmental Medicine Research Policy Committee from 1975 to 1977.

#### **Mr Walpole Lewin**

CBE FRCS (1915–1980) was Consultant Neurological Surgeon to Addenbrooke's Hospital, Cambridge from 1955 until his death. He was also consultant neurological surgeon to the Army. He was a member of the Central Health Services Council from 1966 to 1976; of the General Medical Council from 1971 to 1980; of the Council of the BMA from 1968 and its Chairman from 1971 to 1976 and Vice President from 1979 to 1980.

#### **Professor Robert Logie**

FBP&S FRSA FICS AcSS (b. 1954) focused on the area of human memory and mental imagery in his training. He then worked at the MRC Applied Psychology Unit as a research officer, then a senior research officer from 1980 to 1986, and subsequently was appointed lecturer in Psychology at the University of Aberdeen. He has been Anderson Professor and Head of the Department of Psychology at the University of Aberdeen since 1997 and Editor of the *Quarterly Journal of Experimental Psychology. Human Experimental Psychology* since 2001.

#### **Dr Brandon Lush**

FRCP (b. 1920) was a member of the MRC staff from 1951 and Principal Medical Officer at the MRC from 1961 to 1972. He then became a Consultant Physician at the Frenchay and Manor Park Hospitals, Bristol, from 1973 to 1985. He was Chairman of the Mason Medical Research Foundation from 1972 to 1982 and President of the Cossham Medical Society from 1992 to 1993.

#### **Dr Norman Mackworth**

was Director of the Applied Psychology Unit from 1952 to 1958 when he moved to North America. He worked on stress and performance and was responsible for moving the unit to its Chaucer Road site. See Figure 2.

#### **Professor Peter McLeod**

(b. 1946) worked at the APU from 1969 to 1983, mainly on attention and motor control. In 1983 he moved to the Oxford University Department of Experimental Psychology where he has worked on visual-motor coordination, particularly in high-speed sports, implicit memory of motor skills and connectionist modelling of the acquisition of motor skills.

#### **Professor William Marslen-Wilson**

FBA FAE (b. 1945) gained his PhD from MIT in 1973 and, after a stint as assistant professor at the University of Chicago, moved to the newly founded Max Planck Institute for Psycholinguistics in Nijmegen, The Netherlands, in 1977. He subsequently held posts as lecturer in the Department of Experimental Psychology in Cambridge; Director of the MPI in Nijmegen; a senior scientist at the APU, and Professor of Psychology at Birkbeck College, London, before returning to Cambridge as the director of the APU in July 1997, where he has been a prime mover in the changes in scientific organization and remit reflected in the unit's change of name in 1998 to the MRC Cognition and Brain Sciences Unit (CBU). His interests are in understanding the cognitive and neural systems that underlie the human language function, especially in the domain of spoken language comprehension.

#### **Professor John Morton**

OBE Acad Euro (b. 1933) got his PhD from Reading University and then worked at the APU from 1960 to 1982. He was Director of the MRC Cognitive Development Unit at University College

London from 1982 until his retirement in 1998. He is currently at the Institute of Cognitive Neuroscience at UCL.

#### **Dr Charles Myers**

CBE FRS (1873–1946) was Professor of Psychology at King's College, London, from 1906 to 1909, Principal of the National Institute of Industrial Psychology from 1921, a member of the Advisory Committee, War Office, on Personnel Selection and former Director of the Psychological Laboratory and Reader in Experimental Psychology at the University of Cambridge. He acted as consulting psychologist to the British Armies in France from 1914 to 1919; editor of the *British Journal of Psychology* from 1911 to 1924 and was the first President of the British Psychological Society in 1920.

#### **Irv Pollack**

was a sabbatical visitor from the University of Michigan. He worked on psychoacoustics and attention. Information from Professor Alan Baddeley, note on draft transcript, 3 December 2002.

#### **Dr Christopher Poulton**

(1918–2000) qualified at Cambridge and Guy's and was an experimental psychologist at the MRC Applied Psychology Unit, Cambridge. His work included blinking and tracking; the effect of lack of sleep on junior hospital doctors, ergonomic research on efficient reading methods by means of typeface and the effects of bias. See Oakeshott P. (2001) Eustace Christopher Poulton. *British Medical Journal* 322: 999.

#### **Professor Pat Rabbitt**

(b. 1934) was a research student at Cambridge, supervised by Dr Donald Broadbent. He joined the APU in 1961, working on GPO contracts with Dr R Conrad on problems of keyboard entry for letter-sorting machines, crackle on telephone lines, and problems of error-correction and signal

response compatibility. He was a lecturer in psychology at the University of Oxford from 1968 to 1982, moving to the University of Durham as Professor of Psychology and head of department in 1982 and from 1983 has held the Research Chair in Gerontology and Cognitive Psychology and Director of the Age and Cognitive Performance Research Centre at the University of Manchester.

#### **Professor Graham Richards**

(b. 1941) has been Professor of History of Psychology at Staffordshire University since 1998, where he established a Centre for the History of Psychology. He is also Director of the British Psychological Society's History of Psychology Centre in London, and a past Chair of that society's History and Philosophy of Psychology Section.

#### **Dr Tilli Tansey**

HonMRCP (b. 1953) is Convenor of the History of Twentieth Century Medicine Group and Reader in the History of Modern Medical Sciences at the Wellcome Trust Centre for the History of Medicine at University College London.

#### **Dr David Tyrrell**

CBE FRS FRCP FRCPath (b. 1925), physician and medical virologist, was trained at Sheffield and worked on the staff of the Medical Research Council, mainly at the MRC Common Cold Unit, Salisbury, from 1957, as its Director from 1982 until his retirement in 1990 and Deputy Director of the Clinical Research Centre, Northwick Park Hospital, Harrow.

#### **Dr Fraser Watts**

(b. 1946) was a clinical psychologist trained at Oxford University and the Institute of Psychiatry. He moved from a clinical post to start the APU Cognition and Emotion Group in 1981. He has been at the University of Cambridge Faculty of Divinity since 1994 as Starbridge Lecturer in Theology and Natural Science.

### **Professor John West**

FRCP (b. 1928) qualified in medicine in Adelaide, Australia, in 1951, moving to London where he was at the Royal Postgraduate Medical School, Hammersmith Hospital, off and on between 1954 and 1969, when he joined the new Medical School at the University of California San Diego. In 1960 he was a physiologist on Sir Edmund Hillary's Himalayan Scientific and Mountaineering Expedition, and in 1981 he led the American Medical Research Expedition to Everest. He was President of the American Physiological Society from 1984 to 1985, received the Edward Livingston Trudeau Medal of the American Thoracic Society in 2002 and is a Fellow of the American Academy of Arts and Sciences and a Foreign Member of the Russian Academy of Sciences.

### **Professor Oliver Zangwill**

FRS (1913–1987) was Professor of Experimental Psychology at the University of Cambridge from 1952 to 1981, later Emeritus. He joined the Cambridge Psychological Laboratory as a research student in 1935, acting as psychologist to the Brain Injuries Unit in Edinburgh from 1940 to 1945 and assistant director to the Institute of Experimental Psychology at the University of Oxford from 1945 until he succeeded Sir Frederic Bartlett as head of the Psychological Laboratory in 1952. He was editor of the *Quarterly Journal of Experimental Psychology* from 1958 to 1966 and President of Section J of the British Association for the Advancement of Science in 1963, the Experimental Psychology Society from 1962 to 1963 and the British Psychological Society from 1974 to 1975. See Gregory R. L. (2001) Oliver Louis Zangwill. *Biographical Memoirs of Fellows of the Royal Society* 47: 516–524.

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