Anne Treisman

Born:

Wakefield, Yorkshire
February 27, 1935

Education:

Cambridge University, Modern and Medieval Languages, BA (1956)
Cambridge, University, Natural Sciences, Psychology, BA (1957)
Oxford University, Psychology, PhD (1962)

Appointments:

Medical Research Unit in Psycholinguistics, Oxford (1963)
Visiting Research Scientist, Behavioral Sciences Department, Bell Labs, Murray Hill, NJ (1996)
Fellow of St. Anne's College, Oxford (1967)
University Lecturer in Psychology, Oxford University (1968)
Fellow at the Center for Advanced Study in the Behavioral Sciences, Stanford, CA (1977–1978)
Professor of Psychology, University of British Columbia (1978)
Fellow of the Canadian Institute for Advanced Research (1984)
Professor of Psychology, University of California, Berkeley (1986)
Visiting Scholar, Russell Sage Foundation, New York (1991–1992)
James S. McDonnell Distinguished University Professor of Psychology, Princeton University (1993–2010)

Honors And Awards (Selected):

Society of Experimental Psychologists (1979)
Killam Senior Fellowship (1982)
James McKeen Cattell Sabbatical Award (1982)
Fellow of the Royal Society, London (1989)
Howard Crosby Warren Medal of the Society of Experimental Psychologists (1990)
Distinguished Scientific Contribution Award of the American Psychological Association (1990)
William James Fellow of the American Psychological Society (1992)
National Academy of Sciences, Foreign Associate (1994); Member (1999)
D.Sc., *Honoris Causa*, University of British Columbia (2004)
Elected to American Philosophical Society (2005)
D.Sc., *Honoris Causa*, University College, London (2006)
George Miller Prize from Cognitive Neuroscience Society (2008)
Grawemeyer Award in Psychology (2009)
Elected to British Academy (2009)
Governing Council of National Academy of Sciences (2011)
National Medal of Science, USA (2013)
Wiley Prize for Psychology, British Academy (2013)

Anne Treisman explored the mechanisms of attention, first in audition and later in visual perception. She proposed the "filter attenuation" theory of selective attention to speech. Later, she put forward the "feature-integration" theory, suggesting that selective attention to locations binds features to form integrated representations of visual objects. The theory led to the discovery of conjunction errors in the binding process and explained diverse phenomena in search, in visual organization, and in the peculiar symptoms of patients suffering from Balint's syndrome. Finally, Treisman investigated the capabilities of distributed attention in extracting information in scene perception.

Anne Treisman

Looking back, I feel that I have been extraordinarily lucky in my career. I have seemed in some ways to lead a charmed life, perhaps because I chose goals that came easily to me. I began with essentially no real training in science or psychology, and yet I got pretty much all the grants I applied for (this was in the good old days before the present tightening of scientific funding). I ended up with far more recognition than I expected and probably than I deserved, and I retired in 2010, before working in my field became really tough. I managed to have four children, who graciously allowed me to work without exhibiting too many signs of neglect. I had students who were fun to work with and full of ideas; and my career spanned a period of real development and little competition, an era in which my amateurish approach could still yield interesting results.

Family and Early Years

My mother, Suzanne Touren, was French, and I spoke French before English, which I learned when I was four or five. I remained bilingual, although English became my dominant language. My mother's father, her brother, and her nephew were all physics teachers in Paris. They were descended from Protestants, although they were no longer religious. My grandfather (as well as my uncle, cousin, and mother) wrote a memoir, following the example of his own great-grandfather who, according to the memoir, was a soldier in Napoleon's army and had many adventures, including being shipwrecked in Africa on an expedition to put down Toussaint L'Ouverture's rebellion in Haiti and being saved by an African woman who took a fancy to him.

My French grandmother came from Pithiviers, a village in north-central France, where her father owned the pharmacy. My grandmother and her sister were known in the village as the *demoiselles* Kuss. My grandmother and her family were also Protestants, which is why my grandfather met my grandmother—with a view to an arranged marriage—because, apparently, Protestant girls were few and far between in his circle in Paris. I do not think my grandparents were actually religious. I never knew of them going to church, but it would have been a Protestant one if they had. My grandmother was a wonderful woman. She had left school at 16 but was always reading books—history, current affairs, and so on. She had a great sense of humor and would infect my mother and me with *le fou rire*—a fit of giggling—which amazed and irritated my grandfather. He loved opera and the theater, but I think he did not understand his wife very well.

On my father's side, I know much less. My paternal grandfather died young, in his early fifties, as did my father, Percy Taylor, and his brother. My English grandmother, Bessie, was quite enterprising. She was one of seven sisters from Lidgate, a village in East Anglia, but she decided to explore the world and went to Italy to earn her living as a nanny to an aristocratic family. The head of the family, an Italian count and novelist, Alberto Denti di Piraino, dedicated one of his novels to her. When she came back to London, she married and had three children, of whom my father was the youngest. After her husband died, to make ends meet, she opened a boardinghouse for young women from abroad, which was how my mother met my father. My mother came to England to improve her English and to get over a previous unhappy love affair. Achieving both goals, she married my father.

This binational background made me feel somewhat different from the other children I knew. I was happy but did not always feel that I belonged. I have had a similar experience as an adult in the United States. I have always felt at ease but have never fully absorbed the culture around me. To this day, I do not know the rules of baseball or American football.

Because of the Great Depression, jobs were hard to get in England in the early 1930s, and my father, after being turned down for many school teaching jobs (perhaps because his letter of recommendation said that he was "no great athlete"), was lucky to be offered a position in education administration in the Yorkshire mining town of Wakefield. That is where my parents started their married life and where I was born, in 1935. Wakefield was something of a shock to my mother, who had to adapt to the smoke and the black coal dust that settled on their clothes, in a place that had neither bookstores nor theaters. It was quite a contrast to the cosmopolitan culture she had been used to in Paris. My father's next job was in Lancashire, in another mining town, where my sister, Janet, was born. When I was five, my father got a job as chief education officer for the Medway towns—Rochester, Chatham, and Gillingham—in Kent.

Soon after we had moved to Kent, World War II broke out. We had been going to Paris every year to see my French relatives, but the war ended those visits, and we were cut off from them until 1945. The separation was hard on my mother, but my parents protected me and my sister from the terrors of the war. My sister and I would be carried down to shelter in the cellar whenever the air raid sirens went off. We took our cat with us, and his gentle purring masked the sound of planes flying overhead. Our house was right in the path of the V-1 flying bombs heading for London—doodlebugs, as they were called. My sister and I would always include a doodlebug in the sky in the pictures we drew.

At the beginning of the war, my mother, my sister, and I were evacuated to Scotland, leaving my father in Kent, where his work was deemed essential. In Scotland, we stayed in a large country house near Oban, owned by a friend of my father's, together with several other mothers and their children, one of whom (I was told) was my first (unrequited) love. Meanwhile, the Battle of Britain was fought in the skies over Kent, where my father was. Later, we were evacuated to another village where the house we were staying in was later bombed, while ours in Kent survived unscathed.

My parents got tired of being separated. After a year in Scotland, where we went for walks over the hills and through the frequent, if not continuous, "nice drop of rain," my mother decided to go back to Kent, and we spent the rest of the war there. Our everyday life was filled with reminders of the war—blackouts, air raids, taking refuge in the cellar or under the Morrison shelter, gas masks at school, barrage balloons in the sky just beyond our garden floated there in an attempt to bring down the doodlebugs before they reached London, ration books and limited food, dried eggs, "bananas" made from parsnips and banana essence, and whale meat for protein. We picked rose hips and blackberries for vitamin C and mushrooms and ripening wheat in the local fields. I remember studying the flags with which my father marked the advances or retreats of the Germans on a large map. I went to the village school, which was run by a somewhat sadistic headmaster with seven canes of different thicknesses, with which he kept order in the school. I managed not to be a recipient of the caning. The class teachers were kind, and I was timid enough that the worst punishment I received was to write out a hundred times "I will not be late."

After the Germans overran France, we had no word from my French family, except toward the end of the war, through Red Cross letters that were capped at 26 words. One contained a coded message about the escape of a Jewish friend, who had been helped by my uncle, who was in the French Resistance. It said "La poupée et St. Paterne sont les meilleurs amis du monde"—"The doll and St. Paterne are the best friends in the world." The Jewish friend had a niece called Poupée, and St. Paterne was the name of my uncle's manor house.

I was 10 when the war ended. I remember the excitement of traveling to France by boat, of course, and being greeted at the train station in Paris by 20 or so relatives, all of whom hugged me while announcing their names and none of whom I remembered, apart from my grandparents.

After the war, we went to France twice every year, once at Easter, to stay with my grandparents in their house near Paris, and once to a holiday place, either in the mountains—the Alps—or by the sea in Brittany. When I was 12, I spent a whole term with my grandparents, going to a French school—le Lycee Marie Curie in Bourg la Reine. My grandparents often came to stay with us until they got too old to travel. In his eighties, my grandfather succumbed to Alzheimer's disease, but he was always so cheerful and charming that it did not seem tragic. One conversation he had with my grandmother and my mother went as follows (in French): "Tell me, Zette"—my mother's pet name—"What exactly are you to me?" My mother replied, "But I'm your daughter, of course." He looked pleased and said, "Ah!

I'm delighted to hear it." Then, after a short silence, "And do you know who your mother is?" My mother replied, "Yes, of course I do. She is sitting right beside you in the arm chair." "Ah," he said, turning toward her. "Did you know that, Jeanne?"

My father's last promotion was to run the state education system in Reading, Berkshire, and we moved there when I was 11. He was very good at the job and was much loved in the town. When he died of a heart attack at age 53, my mother received hundreds of letters expressing that affection. When he took the job, in 1945, he had set himself the task of reorganizing education in Reading, which had been sadly neglected for years by an inefficient chief officer. My father transformed secondary education first, developing a sort of comprehensive school system before the term had been coined. The introduction of continuing technical education and the building of the College of Technology were other innovations. The motto he chose for the technical college was borrowed from Michelangelo in his old age: *Ancora Imparo*—"I am still learning." He took a special interest in deaf children and started classes for the partially deaf, with the novel idea of integrating them into the life of a normal school, an idea that was copied later by many authorities but that I believe was unique at the time. He knew he had succeeded when he heard that normal children were coming home with bits of plastic stuck in their ears "to be like the others." People sometimes wondered why there were so many deaf children in Reading. The answer was simple: Their parents had chosen to live there. A new form of youth club was one of his last accomplishments.

Education

I went to the girls' grammar school in Reading—Kendrick School—wearing the school uniform of black tunic, white blouse, black blazer edged in red, a black-and-red tie, and a hat or beret in black and red. We had to wear the uniform all day, including the hat when we were out of doors, and we were not supposed to eat in the streets, but I remember feasting on fish and chips. The school day started with prayers and a hymn, with all the students assembled in the school hall. The Jewish pupils were allowed to sit quietly in a classroom and filed in at the end of the religious service for any general school announcements. Each class had a class teacher, but we were taught certain subjects by other teachers, some much better than others. I enjoyed biology and physics the most but did not get very involved. I do not think any of the teachers were really inspiring. I had to wait until I got to a university for intellectual thrills. I do remember being given a microscope one Christmas and being amazed to see, and to draw, all the normally invisible creatures that swam in a tiny drop of pond water.

Education in England was very specialized. We had to narrow down to three subjects from the age of 15. Initially I chose the sciences, but after a few weeks my father panicked, thinking I would grow up without any culture, and persuaded me to switch to French, Latin, and history, which I did for my last three years at high school, dropping math and science completely, which was a sad loss for me—although giving up the arts would have been equally bad. It seems like a terrible thing to force such an early choice on children. The idea was that undergraduates would then become experts, studying their chosen subject in depth in their three years at a university. To my mind, the cost greatly outweighs that benefit.

My school had not sent any students to Oxford or Cambridge for many years; the teachers feared that the Oxbridge standards were higher than they could reach. But my father wanted me to try for those universities because he had been a student at Oxford and had happy memories of his time there. So the school agreed to let me try the entrance exam and two of my friends did the same. As it happened, we were all offered places, and I think that from then on the school did encourage students to try.

I chose Cambridge, perhaps to be different from my father, and I became an undergraduate at Newnham College, where I read modern languages. That was something of a revelation to me. My tutors were eccentric and interesting. As must be common, I suppose, I remember irrelevant details better than anything I was taught. My English supervisor had a strange verbal tic; she said "wuther" at least once in every sentence. Eventually we learned to tune that out, but at first it was the only thing we heard. Newnham's French tutor, Claude-Edmonde Magny, is best known for her *Lettre sur le pouvoir d'écrire*—"Letter on the Power of Writing"—which she wrote, in 1943, to a young man who read it only after being released from Buchenwald. It is quite a moving discussion of what it means and what one needs to write literature. In 1954, when I had supervisions (the Cambridge term for tutorials) with her, she appeared more interested in shocking her students than in teaching. I got more from the university lectures that I went to than from my sessions with her. Reading French literature was a very different enterprise from the boring translations in which I had engaged at high school. I did well in the modern languages tripos, getting a starred first, although I sometimes felt a little as though I were bluffing, rather than deeply understanding the poets and novelists that I read. I am very grateful for the glimpse of another intellectual world that I had during those years.

Newnham then was a college for women only. If we went out after dark, we had to wear our gowns, and one of our amusements was to run away when we saw a proctor coming toward us, enticing him to chase us. The gowns were also worn at dinner in Hall, where, as a scholar, I had to say grace before the meal. I was at Cambridge in the early 1950s, and we still had ration cards for food and for coal. I remember some very cold evenings. Friends used to share their coal and spend evenings together, talking or gossiping in one another's rooms. I made some good friends during those three years, and I joined a number of clubs, including the Folk Song Society, the Newnham Orchestra (where I played the violin), a choir (the highlight was singing in the Verdi "Requiem"), an acting society, and many others. That was certainly the time in which I sampled the widest variety of experiences.

Thinking that I would need to earn my living, I applied for a job teaching French at Oxford High School. Luckily, I was turned down, perhaps because I did not know what the letters GPDST stood for (Girls Public Day School Trust—a select association of girls' high schools). If I had known that acronym I might have had a very different life.

In the meantime, Cambridge offered me a research fellowship to work toward a doctoral degree in French literature. Although I had very much enjoyed my undergraduate degree, I panicked at the thought of focusing on a single 16th-century poet for the next three years. So I asked if I could use the fellowship money to do a second undergraduate degree, in psychology, instead. My French supervisors were horrified. I remember one of them saying, "But that's all about rats in mazes!" I said I thought that rats could be interesting, and my request was granted.

Psychology, Bachelor's of Arts, and Doctor of Philosophy

I switched to psychology in 1956, at a time of rapid change in the discipline: behaviorism and learning theory's hold on psychology was fading, and the "cognitive revolution" had started. Many psychologists were attracted to a new view of the mind as an information-processing, symbolic system, and the computer was beginning to replace the telephone switchboard as the dominant metaphor for the brain. I was very lucky to be assigned Richard Gregory as my supervisor. He told me not to bother too much with learning theory and to read instead about information theory and vision. Most importantly, he showed me the experiments that he was doing.

In one experiment, he had subjects look at a face mask from the inside. If the subject was stationary, the concave mask was usually seen as if it were a normal convex face. Richard's explanation was that the strong visual bias favoring seeing a normal convex face rather than a hollow mask reflects the effect of top-down knowledge on perception. As soon as one moves one's own head and eyes, however, the mask becomes concave again and seems to follow one around. We tried out several of the other illusions and perceptual effects that are described in Richard's first book, *Eye and Brain* (1956). I remember that he had built a little railway in his lab, which allowed him to study how perception is affected by motion. It was an exciting year. Richard was a wonderful teacher but not a conscientious one. The tutorials I had with him were not serious discussions of standard topics. If I produced an essay that did not involve perception, Richard would soon say, "That sounds fine. Would you like to try out an experiment I've just thought up?" This was an inspiring way to get a student to want to discover more, and it was also always great fun. For Richard, so far as I could tell, psychology was always about having fun.

There were just 12 undergraduates reading psychology at Cambridge that year (including John Morton, who proposed the logogen theory of reading, and Pat Rabbitt, who did great work on skills—and was also very funny). There were only about six journals to read, so it seemed a little less absurd than it would be now to get a degree in one year with no science background whatsoever. Some of the lectures, those by Alan Watson for example, were still about behaviorism and rats, and I did find them interesting, although less inspiring than those by Richard or by Oliver Zangwill, one of the early founders of neuropsychology. There was also an exciting series, by Alan Welford, on what were the beginnings of cognitive psychology. C. G. Grindley, who was, by then I believe, an advanced alcoholic, lectured on perception. He demonstrated the after-effects of motion by making us watch a rotating spiral and then look at his nose, which was already quite red and, in the after-effect of rotation, seemed to be expanding alarmingly. Another lecturer was W. E. Hick, of Hick's Law (which says that reaction time increases logarithmically with the number of alternatives being discriminated). He did not like lecturing and got out of it easily by beginning his courses with the sentence, "This is rather like casting pearls before swine." He also avoided teaching a class in clinical psychology by telling us, in his first lecture, about a patient who had a fetish about prams, which he sprayed compulsively with cans of oil. When we laughed at the story, Hick professed himself so shocked that he refused to teach us anything more.

Once I had my bachelor of arts degree, I thought I might become a clinical psychologist, but Richard Gregory persuaded me to stay in research. His argument, as I remember it, was that I could not help people unless I understood the mind, and the mind depended on the brain, and the eye was a really convenient bit of the brain to study because it has been pushed out to the periphery. So my best strategy would be to study vision. I was again offered a scholarship to do research, this time at Oxford, so I moved there, though not initially to work on vision.

The requirements for a doctor of philosophy degree at Oxford were minimal at that time. There were no lectures for graduate students, no exams, and no duties, except to produce a thesis at the end of three or four years. We did go to department seminars, and I listened to Tony Deutsch and Stuart Sutherland, two brilliant psychologists, argue vehemently about their models of rat learning and octopus perception and about what psychology ought to be. These discussions had a big influence on me, by illustrating the power of simple theoretical models to explain perception and behavior in animals. Tony and Stuart debated with no holds barred and plenty of wit. Stuart was an impressive character, quite ruthless in conversation, never suffering fools gladly, but funny and very likable. I remember the day that he came in to coffee having absent mindedly put on two ties. When everyone started laughing, he said, "That just shows the difference between manners in the psychology department and in Magdalen College. When I was there for lunch today, nobody said anything, much less laughed at me."

New graduate students, on arrival in Oxford, were left to pick a topic for their research. I wondered what to choose. Still thinking of doing something clinical, with the idea of helping patients, I asked my advisor, Carolus Oldfield, for suggestions; he proposed that I look at aphasic patients, perhaps using a framework put forward by Colin Cherry, in his book, *On Human Communication* (1957). After that, I was pretty much on my own. Oldfield was quite shy, and so was I. For several years, we would pass each other in the corridors, and he would ask how things were going, and I would say, "Fine, thank you." He did find my thesis quite interesting when I handed it in.

I decided to use a paradigm that Cherry had developed, in which simultaneous passages of prose are presented on different channels (in my case, the right and left ears through headphones). Participants are asked to attend to and repeat back one of the two messages, staying a couple of words behind the stimulus. This task, which Cherry called "shadowing," is a very effective way to keep attention on the selected message. It also allows the experimenter to monitor how well the participant is doing because any lapse of attention results in missed words in the shadowing. Cherry had shown that participants in the shadowing task know very little about the message that is presented on the rejected channel. For example, they even fail to notice if the unattended message is in another language. However, they immediately notice a switch to a new voice, or other physical changes, such as tones inserted into the speech.

I started going on hospital rounds to learn about aphasic patients and even tested a few on their ability to attend to a selected speech message when there was interference from another. Because I knew almost nothing about brain-damaged patients, it was not surprising that my project quickly failed. I was very reluctant to press the patients to try a task that they were obviously incapable of doing, so I decided to start with some normal controls—and did not go back to investigating clinical patients (not aphasics) for at least 30 years.

At about the same time that Cherry's book came out, Donald Broadbent was working in Cambridge on applied problems of attention in air traffic control operators and developing his general filter theory of attention, which he published in 1958, in his seminal book, *Perception and Communication*. Neville Moray and I were both beginning graduate students at the time. We read his book and more or less learned some chapters by heart. We both decided to do our theses on selective listening, testing Broadbent's theory. This was a functional model of attention, a flow chart of events in the brain when participants focus attention on one of two simultaneous messages. Broadbent's idea was that we take in different sensory stimuli simultaneously on separate "channels"—for example, the two ears, when each is receiving a different speech passage. The stimuli reach a central bottleneck when the amount of information exceeds our capacity to understand both. To protect us from interference between the two, attention selects one and "filters" out the other, blocking it completely from further processing. Hence, in the case of two simultaneous speech messages, we are aware that there are, for example, two speakers, one on each ear, but we know nothing about what the unattended one is saying.

This kind of model was a new departure in psychology. B. F. Skinner's approach, which dominated American behaviorism at the time, rejected any speculation about what might intervene in our brains between the arrival of sensory stimuli and the selection of behavioral responses to them. Broadbent, however, was trying to understand those processes in functional terms.

Moray and I were each given a two-channel tape recorder, headphones, and tapes, and left to get on with whatever experiments we wanted. Apart from the papers by Cherry and by Broadbent, so far as we knew, nobody had done or was doing any research in the field of selective listening, so we had it more or less to ourselves. This would be hard to imagine for graduate students in this day and age. We were competing to have the best ideas for experiments and to carry them out, and then to find the most convincing interpretations, within the framework that Broadbent had set out in his book. We both challenged the claim of an all-or-nothing filter by showing that some information did appear to get through. We used Cherry's paradigm in which participants repeat back (or "shadow") the passage of prose coming in on one ear and ignore the prose on the other unattended ear. Normally, they identify none of the words on the unattended ear. However, Moray showed that if the participant's own name was included in the unattended message, participants would hear it about 30 percent of the time (Moray, 1958). I showed that if the attended and the unattended messages switched places, participants occasionally repeated one word from the previously attended message, now on the unattended ear. I also showed that bilingual participants, presented on the unattended ear with a translation of the attended message, lagging by two or three seconds, occasionally realized this.

Moray and I each proposed a modification to Broadbent's filter theory. Moray proposed that a very important word, such as one's own name, might have a special "analyzer" dedicated to detecting it at some pre-filter stage. This could account for his result but not for my subsequent findings because any word could be made highly probable in the context of the attended message.

My suggestion was that two forms of selection might be combined to achieve a more flexible system involving two separate mechanisms. I suggested that the general filter that Broadbent had proposed might attenuate, rather than block, the unattended messages (See Figure 1). In addition, a top-down lowering of thresholds might ensure that important stimuli, such as one's own name or currently relevant stimuli in the context of the attended message, would get through to awareness even in their "attenuated" state. This would allow attention to focus on the most relevant set of stimuli, while also monitoring for any highly relevant stimuli that might appear on unattended channels. This was an early hypothesis about priming, which is now a very central topic in cognitive and social research.

This modification of Broadbent's theory became known as the filter attenuation theory. Its main rival at the time was a late selection theory proposed by Deutsch and Deutsch (1963), claiming that the attention bottleneck arose beyond the level of perceptual processing, at the stage of memory and response selection. The two hypotheses were very difficult to distinguish empirically using behavioral data. Riley and I (1969) ran an experiment that I thought might be decisive, in which we eliminated response competition and memory load by asking participants to stop shadowing immediately and press a button if they heard a digit in either the attended or the unattended message. We compared cases in which the digit was in the same voice as the shadowed message or in a different voice and found that participants detected almost all the digits in the different voice (which would be analyzed before the filter) and very few of those in the same voice, which would require the post-filter speech analyzers. Response and memory load were the same in the two cases, and only the complexity of the perceptual processing differed.

However, the early selection camp did not retain its preferred status for very long. A finding by Corteen (1972) challenged the basic assumption that we had been making. He showed that a conditioned fear response, induced by associating mild electric shocks with city names, was quite often elicited even when a city name was presented on the unattended channel, and even though the participant was unaware that she had heard a city name. In the 1960s, we had assumed that anything that was perceptually detected would also reach awareness. If perceptual processing could result in "unconscious perception," the story became more complicated. In the 1980s and 1990s, further examples of unconscious processing (e.g., Marcel, 1983), as well as challenges from skeptics, began to appear. By now, it is clear and generally accepted that some stimuli can be registered without reaching awareness, even when they require quite complex analysis.

The early versus late selection controversy was essentially resolved by Nilli Lavie (1995) in her load theory of attention. Her suggestion was that the perceptual load presented by the tasks that were competing for attention would determine the level of selection. If we assume that the perceptual system uses all the capacity available at any given time, we can predict early selection when the current load exceeds the capacity and late selection when it does not. I had anticipated this in 1969, when I wrote, "It may be

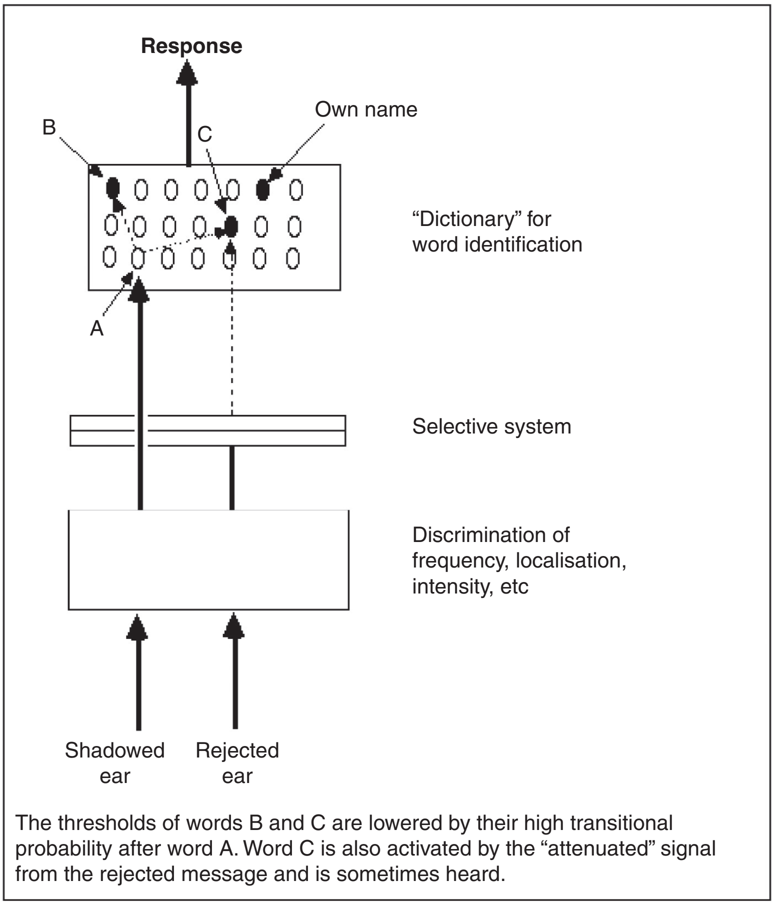

Fig. 1. The model I proposed for selective listening, combining attenuation of unattended messages with top-down prediction of incoming words (Treisman, 1960): The selective filter reduces all the sensory information coming from the rejected ear, making it less likely to activate the corresponding words in the mental "dictionary." Words are shown as nodes in the dictionary network, which are heard when they reach a high enough threshold of activation. Within the dictionary, the thresholds are permanently lower for identifying important signals such as your own name and are also temporarily lowered for stimuli (e.g., words B and C) that are likely in the context of the preceding stimuli (such as word A) that was heard on the attended channel. The combination of the attenuated signal from the rejected ear (word C) and its lowered threshold in the context of the previously attended word (A) increases the chance that word C will reach its activation threshold and that you will therefore hear the unattended word, C. So, for example, if you have just heard the attended words "sitting at the mahogany," the word "table" is a likely continuation of the sentence and may be heard even though it arrives in an attenuated form from the rejected ear.

that the nervous system is forced to use whatever discriminative systems it has available, *unless* these are already fully occupied with other tests or inputs, so that we tend to use our perceptual capacity to the full on whatever sense data reach the receptors" (Treisman, 1969). But Lavie developed the idea into a convincing theory and published an extensive body of research supporting this claim and extending it further.

In parallel with this behavioral story, developments in cognitive neuroscience led to similar conclusions. Back in 1973, Steven Hillyard and his colleagues had shown very early effects of attention on evoked potentials to auditory stimuli, beginning around 80 to 110 ms from onset, confirming that at least some selection can occur early. However, Stanislas Dehaene and colleagues (1988) used another electroencephalogram (EEG) component (the lateralized readiness potential), as well as functional magnetic resonance imaging (fMRI), to show that high-level semantic priming of words could affect response-selection processes without any awareness by the observer. Many other examples, of early selection-reducing (though not eliminating) responses to unattended stimuli and of high-level discrimination without awareness, have been shown using fMRI and other indices of neural processing. It seems that both sides were partially right in the early/late selection debate; meanwhile, our understanding of the more complex and interesting story has become much more refined.

During these early years in the Oxford department, I also came across a simple handheld stereoscope, which allowed one to present different stimuli superimposed to the two eyes. Nobody was using it, so I appropriated it and decided to look for any analogues to dichotic listening using binocular vision. When I presented two different prose texts in different colors, one to each eye, I found that participants were able to attend to and read either one, although they sometimes had difficulty because the combined binocular image, unlike the binaural combination, superimposed the two images spatially and gave rise to rivalry in which the two alternated in conscious experience.

I became interested in the more standard use of the stereoscope, which was designed to evoke the sensation of depth from slight disparities in the locations of stimuli in the two eyes, corresponding to the two different views received by the two eyes because of their horizontal separation. When two such two-dimensional images are seen by the two eyes with the appropriate positional disparities, the brain combines them into a single image experienced in three-dimensional depth. I was still a graduate student at the time and did not find all the previous work that had been done in this area when I searched, so I just enjoyed myself trying to discover how the brain dealt with binocular stimuli. This research typifies my research style, so I will describe it in some detail.

One question I explored was the relation between two findings—stereoscopic depth and binocular rivalry. When two different incompatible images are presented to the two eyes, we experience either one of the two (with the images often alternating) or a patchwork combining parts of both. This alternation or dominance of one of the two images is known as binocular rivalry. I wondered whether it would be possible to combine stereopsis and rivalry and get the sensation of depth from a combined image at the same time as rivalry between the images in the two eyes. I found that this was indeed the result when I combined two stereoscopic images, one in red and one in green. I used the same simple stimulus in all these experiments: two circles were presented to each eye with the inner one displaced toward the nasal retinae in each eye. When fused, this stimulus is seen in depth, with a concentric inner circle closer to the observer than the outer circle. In order to induce rivalry, I made the circles green in one eye and red in the other. I found that participants experienced the depth difference, despite suppression of the color in one eye. One property of a stimulus (its color in one eye) could be suppressed while information about others (its relative location and shape) was preserved to give rise to depth. This was an early precursor of my interest in the idea that separate systems or analyzers independently process different aspects of the same stimuli. The dissociation cast some doubt on the idea that rivalry is always between the eyes and occurs at a relatively low level. My results argued that it can also be at a higher level, involving selection of particular aspects of the stimuli.

In the next experiment, I explored what has to differ between the eyes in order to give rise to rivalry. I presented circles in the same gray but with positional disparities to the two eyes, using a white background in one eye and a black background in the other. Instead of fusing, the identical gray-colored shapes went into strong rivalry between a perceived light-gray shape in one eye and a dark-gray shape in the other, and no depth was perceived. The backgrounds of the stimuli, meanwhile, would fuse to give a gray or silvery background, against which the contours of the shapes went into clear rivalry, competing or alternating. On the other hand, if the monocular contrast was in the same direction in the two eyes, a large difference between the brightness of the monocular circles posed no problem for fusion. A dark-gray figure on a black background in one eye and a white figure on a light-gray background in the other fused well and gave good stereoscopic depth, despite the large difference in brightness of the figures. Problems arose only when the contrast between figure and ground was in opposite directions in each eye. Rivalry depended on the direction of contrast in the monocular stimuli; when it was the same, depth could be seen, but when it differed, it was extremely difficult to achieve a fused image of the circles and to see them in depth. The backgrounds, however, were relatively easy to fuse, and the monocular contrasts were seen relative to the binocular fused background. I related these results to the recently published findings by Hubel and Wiesel (1959) on the "on" and "off" receptors in the cat cortex. A paper reporting my findings was published in 1962 (Treisman, 1962).

After completing these studies of binocular vision, I returned to the research for my thesis, which was mainly on selective listening but also included some studies of the role of expectancy in speech perception. I looked at the effects of predictability in language processing, using a way of controlling the transition probabilities between words. To generate passages differing in predictability, I asked each person to guess the next word in a prose passage when shown a varying number of preceding words. A "second order approximation" to English would result from showing each person only one preceding word and an eighth order approximation would result from showing the preceding seven words, and so on. Various tasks, such as shadowing, translating, and remembering were strongly influenced by this manipulation, suggesting that people in listening to speech are predicting what the likely next words will be and are influenced by the context extending some way back.

I ran about 14 experiments on the effects of selective attention and top-down expectancies on speech processing and produced a 240-page thesis reporting them all, thus earning my D.Phil. in 1962.

Early Career; Oxford

In 1960, I married Michel Treisman who was also a graduate student, in psychophysics, and then a junior lecturer at Oxford. With my D.Phil completed, I was offered a research position in the Medical Research Council (MRC) psycholinguistics research unit at Oxford. I had the first of my four children (Jessica) in 1963 and 16 months later the second (Daniel). I had thought I would take time off work but found that I missed it too much. So I started taking Jessica to work with me. I had a soundproof cubicle in my office because of my work on auditory attention, and it was occasionally of help with a fractious baby (although I would never leave her there for long!). I continued with research on selective listening, extending and writing up the experiments from my thesis, although my own attention was somewhat divided between work and the babies. St. Anne's College (then a women's college), where I had a teaching position, was enlightened enough to open a nursery when several of its Fellows had babies. I could take mine there each day and see them whenever I wanted to. I was and am very grateful for how easy the college made it for me to continue working—a real contrast with what is happening now to many academic women in the United States.

Perhaps the fact that I had no brothers, was educated with girls only until the age of 17, and then went to a women's college at Cambridge made me less aware of any obstacles due to gender than I might otherwise have been. It was not a very salient dimension for me. I did not pay much attention to occasional sexist comments—just took them for granted as a regrettable part of social life. I assumed that I could do whatever I was capable of and wanted to do, and that assumption in my case proved to be true. But, of course, I was lucky. Other women had a more difficult time. I was never disadvantaged, although I was the only woman on the psychology faculty at Oxford for my first few years in the job.

From 1966 to 1967, Michel and I spent a wonderful year as visiting scientists at Bell Labs, in New Jersey. When we asked what research they would like us to do, we were told that they might question the relevance to Bell Labs of research on the sex life of the octopus, but otherwise we were free to work on any topic that we wanted. The psychology department was excellent, with several brilliant members, including Saul Sternberg, George Sperling, Bela Julesz, and Charlie Harris, among others. I did some work with Saul Sternberg and learned a lot. He was developing his ideas about using additive effects in reaction time to infer independent processes (Sternberg, 1969). He was a perfectionist in all the experiments he ran, which was a salutary counterweight to my more slapdash style.

We lived in a beautiful rented house in Morristown. My mother came with us to help look after our two children. (My father had died a few years earlier.) She was a little shocked when she went for a walk close to our house to be told by a policeman that she could not do that (i.e., walk alone on the local roads). Everyone drove everywhere. We were invited to our first Thanksgiving dinner and were impressed by the custom of inviting strangers to share the celebration with the family. We also had the shock of driving through the racial riots in Newark in 1967. We were invited to give talks at a number of universities in the East (MIT.and Harvard) and in California, where we visited the campuses of the University of California at Berkeley, San Diego, and Irvine (the latter still a brand new campus, which reminded me vaguely of the pyramids, with its large new buildings seemingly isolated in the desert). I was still giving talks about my thesis research; work on selective attention seemed to generate a great deal of interest at the time.

The cognitive revolution was definitely underway in the United States by then. Jerome Bruner's *A Study of Thinking* (1956), George Miller's *Magical Number Seven Plus or Minus Two* (1956), and Miller, Eugene Galanter, and Karl Pribram's *Plans and the Structure of Behavior* (1960), had all come out in the previous decade. Ulric Neisser's book *Cognitive Psychology* was about to be published, in 1967, definitively marking the end of behaviorism and its taboo on concepts such as imagery, mental representations, and cognitive models. Contrary to the behaviorist idea that stimuli activate responses to produce behavior, the cognitive revolution saw stimuli as conveying information—reducing the uncertainty about possible states of the world by modifying mental representations—a major conceptual change. Attention was central to cognitive psychology from the beginning, in part because it involved a purely mental event that changed what people perceived.

When we came back to Oxford in 1967, I returned to my position at the Oxford MRC psycholinguistics research unit and assumed that I would go on doing research. But then I was put on a search committee to replace an animal psychologist who was leaving the Oxford faculty. While I was on this committee, another faculty member (Marcel Kinsbourne) resigned his lectureship to move elsewhere. The search committee discussed the possibility of choosing someone from the current list of candidates to fill his position, rather than going through the advertising process again. At that point, I realized that this might be my last chance of finding a position in roughly my area of psychology for quite a while. So I told the others on the committee that I would like to apply for it. As I remember it, although this seems a bit farfetched to me now, they asked me to leave the room, and then after a short time they came out and said, "You can have it."

Taking for granted the idea that I could combine being a mother with an academic career, I had two more babies, Stephen, in 1968, and Deborah, in 1970. Stephen turned out to be a Down syndrome child. Initially, of course, this was a huge shock. But, as it happened, my doctor at the time also had a Down's son and he told me that, although it might seem impossible to me with the news fresh in my mind, it would turn out that this baby would be a joy. And so, in fact, it did. I believe this often happens with Down's children. They are generally happy, affectionate, lovable, funny, and very likable human beings. Stephen became a focus in the family for our other three children, who were and are still as attached to him as we are.

The research I was doing for my remaining years at Oxford was mainly on selective listening, writing up and extending the results of my D.Phil. thesis. But my work with Saul Sternberg at Bell Labs had made me wonder about visual attention and the perception of objects. There was in the air at the time a notion that perception involves the analysis of a number of different features, including shape, color, orientation, and motion, which could be separately attended to. I had been exposed to this notion of analyzers by Sutherland in my early years at Oxford, and I had used it to interpret my results on binocular rivalry and stereoscopic depth perception. In 1969, I tried to write an overview of attention that could encompass more of the findings that were available at that time. I distinguished different forms that attentional selection might take: selection of inputs to analyze (defined by their sources or locations), selection of analyzers (which properties to attend to), selection of items (or objects, defined by the sets of features that characterize them), and selection of which responses to make. In Cherry's and Broadbent's selective-listening task, participants attend to one of two or more competing inputs (in this case auditory messages). In the Stroop task (1935), in which participants named the colors in which other color names are printed, they attended to the analyzer for color and suppressed the outputs of the word analyzer. This is a difficult task because of the interference from the word, but quite possible. I showed that these different selective processes function differently in various tasks. I pointed out that it is much easier to select between competing inputs than between competing analyzers perhaps because the analyzers are independent systems that can be used concurrently, whereas two similar inputs compete for the same brain mechanisms (cf. my earlier discussion of Lavie's load theory of attention).

The idea of separate analyzers raised a further problem, which I also mentioned in the same paper and which would occupy me for the following two or three decades. This is the question of how we recombine the outputs of the separate analyzers correctly when we identify objects. In the 1969 paper, I raised the possibility that mismatches might sometimes occur in selective attention, so that, for example, "a particular word may be heard or remembered in the wrong voice or position." I came back to this question in the 1970s, taking another example: If the color system registers a green object and a red object and the shape system independently registers an X and an O, how would we know that the X is red and the O is green and not the reverse? This was later termed the "binding problem." One solution might be to use their shared spatial locations. I had the idea of a spotlight of attention traveling over the visual field, joining together the features that it finds in each location as it passes through it. Perhaps we could identify the correct conjunctions simply by focusing on each location in turn. If attention was directed to one location at a time, any features that were present in that location should normally belong to the same object.

I remember trying a paper-and-pencil experiment on my children, sitting on our lawn in Oxford one day. I asked them to find the red X on a page of red Ts and green Xs. The result was quite surprising. It took them much longer to find the red X than to find either a blue letter or an O in the same background of red Ts and green Xs. Note that two features define the search targets in each task: red and X versus blue or O, but they seemed to pose a very different level of difficulty in search. It seemed that binding features together (the red color and the X shape) to locate the red X target was much harder than finding either of two unique features without having to bind them. So I ran the experiment with proper reaction-time measures on proper participants (Oxford undergraduates). The results matched the predictions quite closely: search times increased linearly with the number of items in the display when the target was defined only by how its features were combined, but when the target was defined by a disjunction of features—either blue or O—search times were almost independent of the number of nontargets. When binding was required to identify the target, the ratio of the slopes on target absent to target present trials was close to two to one, suggesting that search was not only serial but self-terminating, ending when participants found the target (which would be on average halfway through the display) or continuing to the end of the display when it was not present. The finding was consistent with the idea that the brain might analyze separate features in specialized areas and then integrate each pair in turn to form the correct conjunctions. I suggested that this might be done by focusing attention on different locations one at a time and integrating whatever features were currently in the window of attention (Treisman and Gelade, 1980).

Move to Canada: Feature Integration Theory

Meanwhile, my first marriage had broken up and Daniel Kahneman and I were invited to spend the year 1976–77 at the Center for Advanced Study in the Behavioral Sciences in Stanford, California. We had an exciting year there, during which I began to write up my ideas and the data I had collected so far on the role of visual attention in binding features together to form perceived objects. We also applied for possible university jobs to which we could move together when the year at the center was over. We were lucky enough to get offers from Michigan, Illinois, and the University of British Columbia (UBC) in Vancouver. We fell in love with the beauty of Vancouver when we visited there, and, the following year, we moved to UBC. We spent the next eight years very happily in Vancouver. The department was a good one, with Peter Suedfeld as chair, Richard Tees and Tony Philips in neuroscience, Michael Chandler and Janet Werker in developmental psychology, and many others. We felt at home with various aspects of Canadian politics and culture, which were more akin to the welfare states we were used to in Britain and Israel. In fact, we came very close to applying for Canadian citizenship.

My children went to schools in Vancouver, all except Stephen, who was barred from immigrating either to Canada or to the United States. We had a difficult time with the immigration officials, who wanted also to bar me as the mother of a Down syndrome child. It was never very clear why, but perhaps the fear was that we would later apply to bring him in as well and that he would become a drain on the government finances. We were fortunate enough to find a wonderful village for mentally handicapped Jewish people in England (Ravenswood Village, in Berkshire). He has lived there ever since, visiting us each year for two or three weeks. It is a happy place, and Stephen has thrived there, delivering the mail, acting as disc jockey for the village dance parties, and joining in many other community activities. He has been on fundraising bicycle rides all over the world (on tandems with a non-handicapped partner).

Eventually the Canadian immigration official who had given us a hard time, keeping us in doubt for most of the year, moved to Hong Kong and was replaced by a much more reasonable one, who acknowledged that there was no problem with my application and gave us our visas without further ado.

In Vancouver, I continued to test my feature integration theory in a number of different tasks, hoping to get converging evidence for the ideas. I ran about nine experiments testing various types of feature conjunctions. I wondered whether difficulties in combining features would also appear with other kinds of features (e.g., parts of letters). For example, an "R" can be made by combining a P with the diagonal from a Q. A "T" can be made by combining the top of a Z with an I. Sure enough, search for a target R in a background of Ps and Qs gave search times that increased linearly with the number of items in the display, as did search for a T among Is and Zs. The pattern was different with the control conditions I used: search for an R among Ps and Bs and search for a T among Is and Ys. The similarity between the nontargets and the targets was, if anything, greater in those control conditions, but the slopes were less steep and nonlinear. The pattern was consistent with serial search when the target required binding of letter parts but was not when there was no binding problem.

The theory also makes predictions about perceptual grouping. In order to organize all the items into homogeneous areas separated by clear boundaries, we need to be simultaneously aware of multiple items within and across the groups. This should not be possible for objects defined only by conjunctions if these require focused attention to each item in turn. Observers who were asked to decide whether the boundary between two groups was vertical or horizontal were much slower when the two sides of the display shared the same features and differed only in how they were combined than when they differed in one feature and matched in the other. For example, blue Os and red Ts on one side of the boundary and red Os and blue Ts on the other did not produce a clear impression of a boundary between the two areas. I did these experiments in a variety of versions, varying the stimuli and other parameters, which I wrote up in a paper called "A Feature Integration Theory of Attention" (Treisman and Gelade, 1980).

The theory made another prediction—that if attention is overloaded or diverted elsewhere, the observer might get the binding wrong and see illusory conjunctions. With some trepidation, I tested the prediction. My research assistant, Hilary Schmidt, prepared some tachistoscope cards with three colored letters and flashed them briefly, while I tried to report any combinations of color and shape that I saw. After a few trials I said, "This isn't working. I can see them quite clearly. We need another theory." Hilary laughed and said, "You actually made a number of switching errors, mismatching colors and letters." We quickly ran the experiment properly with participants who did not know what to expect. In order to load attention and prevent them from focusing attention on any single letter, we gave them a primary task of reporting two black digits (which we presented one at each end of a central row of three colored letters) and then identifying any colored letters that they were able to see. Just as I had, on about a third of the trials, they made an illusory conjunction error in which they reported a letter that was presented in the color of another presented letter. We subtracted a baseline of feature identification errors in which they reported a shape or color that was not presented. On a quarter of the trials they were binding features from two different objects and seeing a wrongly integrated combination. What convinced me that these were genuine illusions was the fact that about half the observers at some point reported that they saw colored digits, despite the fact that they had been told, correctly, that the digits would always be black (Treisman and Schmidt, 1982).

Around this time, neuroscientists, including Zeki (1978, 1993) and Van Essen et al. (1992), independently discovered evidence suggesting that different features are analyzed separately in different areas of the brain. Visual perception seemed to register different features in a whole array of specialized maps. It seemed that our data from psychological experiments might be converging with data from the brain. So the theory aroused a fair amount of interest and got me invited to conferences in neuroscience and computer science as well as in psychology. One morning, I even received a phone call from a man who identified himself as Francis Crick. Not knowing at the time about Crick's interest in neuroscience, I wondered who was playing a joke on me, but he soon made it clear that he was telling the truth, and he invited me to visit him in La Jolla to talk about my findings on vision. With some excitement, I went there and presented my theory and findings to a group of vision researchers and discussed them with Francis. I believe I went several times as Francis struggled to make sense of some of the disputes with others in the field and reached the somewhat disappointing conclusion (for me) that perhaps psychology was not a clear and simple route to the truth. I was enormously impressed by his sharp and incisive analyses of my claims, as well as by his friendly approach.

My theory had indeed run into some problematic results. One finding (Nakayama et al., 1986, and then Wolfe et al., 1989) was that search for conjunctions of features could sometimes look parallel rather than serial (in other words, the latency of detecting the conjunction target could be unaffected by the number of nontargets in the display), casting doubt on the idea that binding was always a serial process requiring focused attention to each item in turn. Ken Nakayama's and Jeremy Wolfe's computer displays used highly discriminable features, relative to my tachistoscope cards, which were made with colored ink pens. Wolfe and I each suggested a modification to the theory. I went back to an earlier result we had found, suggesting that adjacent items that are identical can be grouped and treated as a single item because illusory conjunctions could not occur within the groups (Treisman, 1982; Treisman and Sato, 1990). If highly discriminable features can also be grouped and rejected before attention is directed to them, the display would be essentially segregated into two separate feature search tasks. Wolfe proposed a similar account based on selective activation of items with the attended features. Both could be correct if attention could both activate items with one target feature and suppress items with one or both nontarget features. On the tachistoscope cards that I used in those pre-computer days, the features were less discriminable than they are with bright computer displays, and we had never observed the parallel processing pattern of latencies.

In the information flow model that I proposed to account for the findings (Figure 2), stimuli are registered in a map of filled locations and also at the

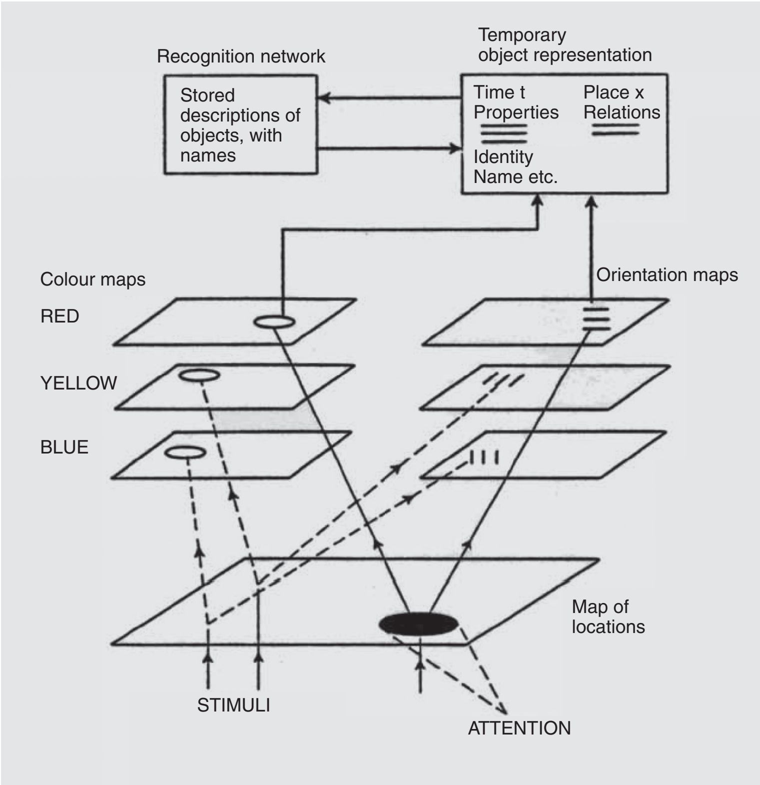

Fig. 2. The model I proposed for feature integration theory (Treisman, 1988): At the earliest stage, visual stimuli are represented in a map of locations specifying where they are and also in separate stacks of feature maps representing which colors, directions of motion, orientations, and so on are present. In order to bind together the features that are in the same location, attention moves around the map of locations selecting one location at a time thus specifying which features it contains. The selected features in a particular location are combined in what we called an "object file" that specifies which features belong to that object and the relationships between them. So in the figure, attention is temporarily focused on the object in the lower right corner and retrieves its features— in this case, red and horizontal. Of course, there would be many more feature maps for other properties such as direction of motion, and simple elements of shape, size, and so on. The representation of the currently attended object is compared to a store of known objects to be identified or, if it is unfamiliar, recognition fails but the object is still perceived as a particular conjunction of properties at a particular time and place. Unattended objects remain unbound as "free-floating" features, but we also have access to the general layout of filled locations. In this case, we would know that there are two objects, in the lower left and upper left locations, but their features may combine wrongly so that, for example, an illusory blue vertical conjunction may be seen.

same time in different specialized feature maps. Attention can be focused on particular locations in the master map and, through those, can retrieve the features that they contain. The locations selected can include groups of identical objects. The features in the attended location are collected together to form a temporary representation of the currently attended object or group in what we later called an "object file" (see following). These can be bound together because they all coincide in the attended location. Attention can then move on to another location where the process is repeated. Simple features are available in parallel before the binding, but to get the correct conjunctions, the binding process must be applied to each object in turn.

This theory attracted a fair amount of attention. Why? I think because it proposed an underlying mechanism for how our conscious perception could result from testable brain functions of which we remain unaware. Its claims were surprising and counterintuitive. We have no sense that stimuli are in any way decomposed and recomposed. The implication of the theory was that in some ways we create our experience rather than its being determined directly by a camera-like process. Perception is more like a controlled hallucination than like an automatic registration of stimuli. The theory made some testable predictions about, for instance, the occurrence of illusory conjunctions in normal people and the failures of perception in brain injuries (see following). In addition, it integrated a range of different findings with a single general account. Finally, it was relatively easy to test. I had stuck my neck out in an inviting way. As often happens in controversies like this, the theory was condensed into a simple summary—search is parallel when the target is a simple feature and serial when the targets are conjunctions. People tend to ignore the other findings that prompted the theory, as well as later modifications of the theory, and dismiss it if it fails any of the original predictions.

The main alternative theory put forward more recently is the biased competition model of Desimone and Duncan (1995). The claim here is that the neural representations of objects compete in the brain and that attention can bias the competition in favor of one alternative. The goals of the two theories and the data they were designed to explain were rather different. Biased competition theory offered no clear explanation for the occurrence of illusory conjunctions or for the contrast between serial search for conjunctions and parallel search for simple features. This is not the place to review the theory in detail, but it is an interesting hypothesis. Future research will help to determine which theory accounts best for all the data or whether the two can be integrated into a more comprehensive account.

Moving Objects

In the 1980s, while we were still in Vancouver, I began to do some collaborative research with my husband, Daniel Kahneman, and his student, Brian

Gibbs. We were interested in how representations of objects are formed and updated over time as the objects move or change.

In one of our many experiments, we used a succession of two displays each consisting of two letters. In the examples shown in Figure 3, the initial display consists of the letters B and M and two pluses. The second display also consists of two letters, which are displaced horizontally and vertically from the original letters. In all the cases shown in the figure, the displacement is up and to the right but on randomly mixed trials, it was up and to the left, or down and to the right, or down and to the left, so that the observer could not predict from the first display in which direction they would move. The second display was shown immediately after the first was removed, creating a compelling impression of a single pair of letters moving coherently from the initial to the final locations, along the trajectory shown by the arrows in the figure. The observer's task was to name the letter that was closest to the plus, B, K, and T, respectively, in the three conditions. We found that the letter was named about 30 milliseconds faster in the first condition than in the other two, which differed by only a few milliseconds.

What makes this finding interesting is that it is not a conventional priming effect. We normally think of priming as the lingering effect of an earlier stimulus on the perception of a subsequent one. In the three

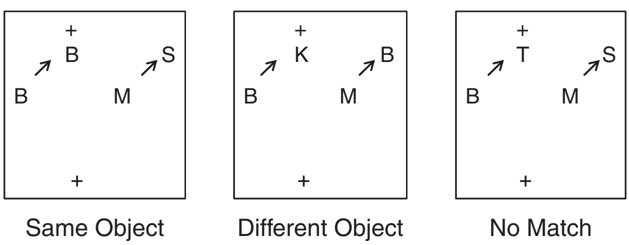

Fig. 3. Examples of stimuli used in our experiments with moving objects (Kahneman et al. 1992): Each figure represents two successive intervals of time, chosen to give an impression of apparent motion between the two displays (shown by the arrows). (a) Same object means that the cued object to be named is seen to remain the same as it moves; (b) different object means that the cued object matches the other object from the first display rather than the one that seemed to move to the cued location; and (c) no match means that neither of the objects in the second display was shown in the first. Priming (speeded response) is shown only in the same object condition, although the information in the first display is neutral with respect to which will be the relevant letter. Both are equidistant from the cued location. The location of the irrelevant (uncued) letter in the second display is what determines the direction of the apparent motion of both letters. If the S in the same object example had appeared to the left of the cued location, the motion would have been seen as between the M and the cued B, which would have made it an example of the different object condition.

panels of Figure 3, however, the original displays are identical. The differences between conditions can be caused only by a "reviewing process" that occurs after the presentation of the second display. When a new display is presented, the visual system establishes a correspondence between perceived objects, by "looking for" the previous state of the same object and integrating the two in a single "object file." The selection of the previous state of the same object is determined by its apparent spatiotemporal continuity. We ran five other experiments testing this general hypothesis, using real motion as well as apparent motion or stationary objects with continuity determined by a shared spatial position, and all gave similar results—a previewing benefit of around 30 ms. We called this object-specific benefit the "reviewing effect."

It seems that perception of a later state of the same perceptual object is faster than perception of a new or different object. The effect could not be due to priming from a previously seen letter because when it was seen as a different object, the naming latency was if anything slightly slower than when the initial letters were replaced by asterisks. Nothing in the initial display indicated which of the two previewed letters would be seen to move into the target location and thus to "become" the target letter. The target letter was equidistant from the two initial letters. Lingering activation in particular spatial locations could generate only nonspecific benefits.

We proposed that seeing an object is a separate process from identifying it. Identification depends on matching a current perceptual representation to a stored description of features and their relationships. Before this is done, we must collect and bind the features that belong together in a particular "object file," and this is done on the basis of shared location at a particular time or of continuity of perceived motion over time. An object file is defined as a temporary "episodic" representation that mediates conscious awareness of a present object in its current location, from the particular viewpoint and in the particular illumination in which it is seen. If the object moves or changes, the object file is updated to maintain the continuity and identity of the representation. (One analogy is to the file that the police might open to record a particular incident. It would be labeled by the time at which it occurred and the location, and any further details would be entered as they accrued.) We distinguished these temporary representations from the permanent mental dictionary of object identities used to identify instances as members of a category (Kahneman et al., 1992). These ideas have proved useful in explaining the perception of dynamic events and also in the development of object perception in infants.

This experience of collaboration also taught us a lesson about research. We initially had different ideas about one aspect of the underlying model. Danny thought that perceptual objects were present preattentively and I thought that they were created through an act of attention. We had planned to present our findings in two successive papers at a meeting of the

Psychonomic Society. The fact that we had this theoretical disagreement was of some concern, but we thought that by the time we gave the talks, we would have gathered enough evidence to settle the argument one way or the other. We then embarked on a series of experiments that we each expected to decisively convince the other, only to find that that task was harder than it seemed. If an experiment appeared to prove Danny correct, I would quickly see why it was not in fact a good test, and he did the same to my results. In the end, we had to leave the contested point open. We finally managed to write a paper that we were both reasonably happy with. Recent research on our paradigm has continued to explore the ideas, some aspects of which remain controversial, but the distinction between "seeing" and "identifying" and the question of how object representations are established and maintained across time and space are recognized as significant issues.

University of California, Berkeley

Although we were happy in Vancouver, we became restless, and in 1985, we considered moving to the United States. We were offered positions by a number of universities and ultimately decided to move to the University of California, Berkeley. Berkeley is a special place—a great state university with a history of intellectual debate and left-wing politics. The beauty and wonderful climate of the Bay Area were additional assets that made our time there precious. There were many exceptional people working in the psychology department, some of whom became our close friends. They included Al Riley who was the chair, Ervin Hafter in psychoacoustics and auditory attention, Steve Palmer in perception, Russ and Karen DeValois in vision, Alison Gopnik in developmental psychology, and Dan Slobin in psycholinguistics. Cognitive science was also strong at Berkeley with John Searle and Bert Dreyfus, among others, on the faculty. We stayed an extra year in Vancouver in order for my youngest daughter, Deborah, to finish high school there and then settled for six years in Berkeley where Danny and I both had faculty positions in psychology and Deborah was accepted as an undergraduate. Jessica and Daniel had both done undergraduate degrees at Oxford, where their father was on the faculty.

In Berkeley, Danny and I continued to share a lab as we had done in Vancouver. We attracted considerably more graduate students and, at one point, we had 12 or more working with us, mostly women as it happened. Men in our field were more likely at that time to apply to Stanford. For some years, my research continued to deal with feature binding and object perception, but I also became interested in extensions of the theory to memory for novel objects and to practice effects and automaticity. To outline these different lines of research, I will start with the ones that followed most directly from feature integration theory.

Balint's Patient: The Role of the Parietal Lobes in Feature Binding

While we were in Berkeley, I was lucky enough to get results from a very different situation that confirmed a possible link between my feature integration theory and the brain. Lynn Robertson, a neuropsychologist, came across a patient with Balint's syndrome and asked if I would like to study him with her and her student Stacia Friedman-Hill. Balint's patients have bilateral lesions in the parietal lobes, which result in several striking symptoms. First, they exhibit a severe loss in the ability to localize objects; our patient could not tell if a spot was at the top or the bottom, or the left or the right of the screen. Second, they suffer from simultanagnosia; our patient could see only one object at a time even when many were present in the field. So his visual world was reduced to a single object with little sense of where that object was. I wondered if the Balint's symptoms were due to the loss of a representation of space and a consequent loss of the ability to focus attention within space and therefore to bind features. If a patient with damaged parietal lobes could not make divisions in space, then he should not be able to bind different features into separate individuated objects. (PET and fMRI testing has shown that the parietal lobes are active when normal people do search tasks involving conjunctions of features.)

We tested our patient with just two colored letters in a display, leaving them present for several seconds at a time. Sure enough, he had huge difficulty in telling us which letter was in which color, and he reported illusory conjunctions on a large proportion of trials—far more than my normal subjects had when we flashed displays much more briefly. The size of the letters did not seem to matter and neither did the distance between the letters. The search difficulty in Balint's syndrome seems to be specific to binding. Our patient could report a single target color in a display of items in another color—a red O, for example in a display of green Os and Xs, no matter how many there were—because he did not need to scan them serially. However, he could typically only see that one red item. But he was quite incapable of finding a red O among green Os and red Xs.

These deficits specific to binding in search are consistent with the predictions of feature integration theory and they illustrate the dramatic effect that a loss of the ability to bind features can have on perception. As we wrote in the paper reporting our findings (Robertson et al., 1997):

> One morning RM mentioned that he had found a good way to improve his vision. With the help of his granddaughter, he had made a tube through which he looked at whatever he wanted to see more clearly. This is a striking observation. One of the defining characteristics of Balint's patients is that they can see only one object at a time, so it seems initially surprising that RM should need external aids to restrict his vision to one object at

> a time. But it follows nicely from the feature integration theory (FIT) account. If the damage to his ability to represent space prevents the normal binding of features to locations without preventing their detection at early levels of visual processing, the features of different objects should tend to coalesce into a single object, producing illusory conjunctions or confusing mixtures of features in the one object that is seen. RM did complain of such illusions. For example, the following are comments he made at various times in our binding experiments: ''When I first look at it, it looks green and it changes real quick to red. I see both colors coming together. . . . I get a double identity. It kind of coincides.'' His descriptions sound very much as though he has no perceptual space in which to separate and individuate the letters and bind the colors to the shapes. The tube he invented may have helped by restricting the early detection of features to those of a single object. Essentially, he constructed an external window of attention to supplement his defective internal window.

Search for Absence

Feature integration theory suggests that focused attention is needed to bind the features of objects in the correct combinations. A related prediction for normal people is that without focused attention we should not be able to search for the *absence* of a feature that was present in all the other items in the display. That would require binding the critical feature to the items in which it was present in order to locate the one item that did not have it. We used search for an O among Qs. It turned out that search again appeared serial here (linear increases in search time with added nontarget items in the display), whereas the converse—search for a Q among Os—was easy and independent of the display size. To detect a single Q among Os, you need only to detect the presence of a slash anywhere in the display, whereas in a search for the absence of the slash, you have to bind each slash to each O to make sure that they are all Qs.

We could now use this striking search asymmetry as a diagnostic for what counts as a feature for the visual system. Some people had suggested that line ends might be automatically detected. So we tried search for a C among Os, which does turn out to be easy—whereas locating an O among Cs seems to require serial search. The line ends (or terminators) created by the gaps in the Cs are detected automatically in parallel whereas the closed circle is not. Using the logic drawn from the Os and Qs experiment, we could argue that the gap (or the pair of line ends) functions like the slash, whereas there is no unique feature detectable in the closed circles. Closure could have been detected by this diagnostic but seemed not to have been. We tested multiple potential features using search asymmetry as a diagnostic and confirmed that, by this criterion, "tilted" (as opposed to vertical) and curved as opposed to straight are features. Each of these extra features pops automatically out of a display, just like the slash on the Q in a field of Os. On the other hand, when we search for straight or vertical lines, we have to use focused attention to check that each straight or vertical line is *not* combined with the tilts or curvatures that are present everywhere else in the display. This research was begun with my student Janet Souther in Vancouver (Treisman and Souther, 1985) and developed with my research assistant Stephen Gormican in Berkeley (Treisman and Gormican, 1988).

These experiments were also useful for shoring up a potential weakness in my feature integration theory. Critics had suggested that the theory was circular. If search looked parallel (i.e., no increase in latency as the number of items in the display increased), it had to be mediated by a simple feature; if it looked serial (a linear increase in search time as the number of items increased), it depended on binding features. Clearly we needed an independent way of determining what counted as a feature for the visual system.

The alphabet of basic features for the visual system was not known in advance nor did it necessarily correspond to the set of properties that we traditionally identify as different physical dimensions. So, for example, properties such as looming, closure, and terminators or line ends could potentially all be features. Whether or not they were was an empirical question. My theory suggested several independent tests of "featurehood" beyond the parallel search that I had used in the early work. One was that features could recombine to give rise to illusory conjunctions; another was that they could mediate rapid and easy texture segregation; another was that they could create an impression of apparent motion when their location changed in a background of stimuli with other properties; finally, detection of a feature was not helped much by an advance cue to its spatial location, unlike the detection of a conjunction which benefited greatly. Ideally, evidence from neuroscience would also help us determine what properties were coded independently of others. Search asymmetries provided a new criterion. If a curved line popped out of a display of straight lines, but not vice-versa, then "curved" was a feature and "straight" was coded as the absence of that feature.

Memory for Novel Objects

Comparing Explicit and Implicit Memory A natural question, once we have set up new object representations, is how and for how long they survive in memory. I decided to investigate this question in collaboration with my student, Gail Musen (Musen and Treisman, 1990). We used novel shapes created by joining a subset of five or six dots in a three by three matrix (see examples in Figure 4). We found long-lived priming on a perceptual test after a single exposure, measured by the difference in accuracy in drawing

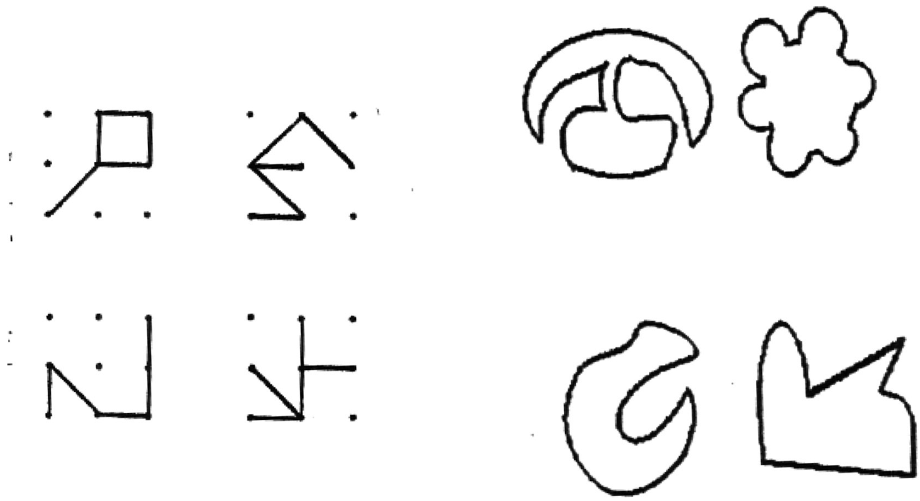

Fig. 4. On the left, I show examples of novel shapes used to compare explicit and implicit memory (Musen and Treisman, 1990). On the right, I show examples of novel shapes used to test implicit memory in negative priming (DeSchepper and Treisman, 1996).

a briefly flashed picture that had been previously seen and drawing a novel shape. The priming lasted at least eight days with little or no loss. The priming results contrasted with the results in recognition memory ("Have you ever seen this object?"), which was subject to substantial loss over time.

A different form of recognition memory can also be tested for specific exposures to a previously experienced item. We do not ask, "Have you ever seen this item before?" but "When did you see this item before—was it on the previous trial or on some earlier trial?" Tulving's concept of episodic memory requires a record of a specific experience or event rather than a distinction between familiar and unfamiliar (Tulving, 1983). We found evidence that the traces formed of novel items can mediate all three types of memory retrieval—perceptual priming or facilitation by pre-exposure, conscious recognition (this shape is familiar or not), and specific event memory (I saw this at a particular time and place)—but that the three depend on different, at least partially independent, representations. There was no correlation between these different tests of accuracy, and the memory survival times were clearly different. Thus novel objects can lay down separate traces for each occasion on which they are experienced as well as a general representation shared by all experiences of the object. The type-token distinction that we need to explain our results with moving objects also applies here.

Negative Priming with Novel Objects I ran another experiment on memory for novel objects with my student, Brett DeSchepper (DeSchepper and Treisman, 1986). We adapted an interesting paradigm known as negative priming, introduced by Stephen Tipper (1985). When observers are asked to attend to one of two overlapping outline pictures of objects and to name (for example) the red one, ignoring the green one, they do this very well. However, if a previously ignored picture is presented again on the next trial as the attended picture, the naming response is slightly slower than the response to a control picture that was not previously ignored. It is as if ignoring a picture makes its representation harder to attend to on the next trial. We wondered if this negative priming would work only for previously familiar objects that had preexisting representations that could be inhibited by attention, so we used as stimuli some novel nonsense shapes that had never been seen before. We made a collection of about 250 outline nonsense shapes (hand-drawn, see examples in Figure 4) and repeated the Tipper experiment using these. We argued that if negative priming is also shown with novel shapes never seen before, they must have created and left memory traces that remained available on the next trial and slowed the response when they were reactivated. We found that negative priming was just as strong with these novel objects as with familiar pictures. A single presentation of a novel nonsense shape evidently leaves a memory trace even when it is ignored and not consciously perceived.

The next question was—how long would the memory trace survive? We looked for negative priming not on the immediately succeeding trial but on the following one, then on one 10 trials later and even 100 trials later, and to our surprise, negative priming was in effect across all these delays. We also used a recognition task in which we asked people to pick out the shapes that had been presented among an equal number of unseen shapes, to confirm that there was no conscious memory of the shapes. It seems that a single presentation of a novel nonsense shape can leave an implicit memory that survives across many intervening presentations and even across a delay of up to four weeks. I believe that this persistence depends on the shape being presented once only. New exposure to the shape during the interval would presumably clear the inhibition that was attached to it when it was suppressed on the initial presentation. The findings imply a surprising degree of plasticity in the visual system, although the memory traces are not explicitly available to conscious memory (Treisman and DeSchepper, 1996)

Memory for Novel Objects in Skill Automatization Finally, yet another kind of memory effect is involved when we acquire a skill through extended practice. What changes in the brain when we carry out a task repeatedly—for example, searching for the same target shape in different displays over hundreds of trials? Does it eventually become an integrated object in our perceptual system and begin to pop out of the displays as though it were a simple feature? More specifically, can we form unitized representations of feature conjunctions, bypassing the binding process, and if so how? With another graduate student, Alfred Vieira, and my research assistant Amy Hayes (Treisman et al., 1992), I designed several studies to find out. In one experiment, we used the nonsense shapes also tested by Musen and Treisman (1990). Would these line patterns become unitized over successive sessions of search? If so, would they behave as features in other tasks such as mental rotation, boundary detection, and apparent motion in more complex displays? We found that although practice in search had a large effect in reducing search latencies for the practiced conjunctions, there was little transfer benefit to other tasks in which simpler features naturally speed processing. Even changing the layout of the search display or the contrast of the figures (black on white versus green on black) substantially reduced the benefit of practice. The main benefit we observed was only on tests for liking: the participants did come to like their practiced line patterns much more than the unpracticed ones!

In another test of whether the learned patterns behaved as features rather than conjunctions, we found little transfer across locations in search for such patterns. Repeated exposure to a multi-feature pattern yielded a search advantage that was mostly restricted to presentations in the same place. This result suggests that attention was still required to bind the features and that search was speeded by learning which locations to search first. We concluded that automatization through practice does not necessarily result in the creation of integrated conjunction detectors. Although search speeds up with practice for the learned conjunctions, we believe that participants develop other strategies to achieve this. In the case where search even becomes parallel, we suggest that what happens is that participants may discover one or more new "emergent" features and develop a grouping strategy that allows them to inhibit all items with this feature, even in intermixed displays. Consistent with this account is the fact that it has so far proved impossible to acquire parallel processing of displays in which the nontargets consist of different arrangements of the *same* features (e.g., T's among L's in any orientation, or a red-and-green circle differing only in which half of the circle is colored red and which half is green). These are just a sample of the research studies that my U.C. Berkeley lab did between 1986 and 1991.

In 1991–92, Danny and I spent a sabbatical year at the Russell Sage Foundation in New York. I was not a typical visitor there. Most of the fellows there are involved in social science with the goals of "diagnosing social problems and improving social policies." But Danny fell into that category, and they stretched the definition to include my research so that we could both spend the year there. It was a very interesting year for me, interacting with the other 12 visitors and attending talks outside my usual preoccupations, as well as writing up the data that I had accumulated in the previous few years.

While we were there, in October 1991, there was a huge fire in Oakland. Our Berkeley house burned down, together with 3,354 other homes and a large number of apartments. Twenty-five people were killed. On the day of the fire, my students gave me constant updates on the situation, telling me, "It looks bad." At the peak of the fire, houses were disappearing at the rate of one every 11 seconds. My daughter, Jessica, was living in our house while we were away, so we had some moments of panic, but we managed to get her on the phone and discovered that she and my other daughter, Deborah, had been spending the day on the beach. She was not able to go back into the fire area so, with just her shorts and T-shirt, she took refuge at Deborah's apartment in San Francisco while all her possessions were consumed, along with ours. The house burned to the ground, and when we later visited the site, we found that only the mailbox had survived along with a metal urn that was encrusted with ashes, some bed springs and the remains of a bicycle.

Despite this traumatic event, we enjoyed the experience of living in New York, and Danny became very interested in the idea of moving there, partly a result of losing our Berkeley house. He made some tentative enquiries with local universities, and we took the next year back in Berkeley to decide what we would do. Eventually, we chose to move to Princeton. I was reluctant to do this because I loved Berkeley, but New York also had strong attractions, including the fact that Deborah was working there. Her career was meteoric. She worked first at *Harper's*, then at the *New York Review of Books*, and was then the managing editor of a literary quarterly, called *Grand Street*, before taking the position of deputy fiction editor at the *New Yorker*. In 2002, aged 32, she became the fiction editor—her dream job, which she has held ever since. Jessica also moved back to New York, where she had done her doctorate in molecular biology at Rockefeller University before doing a postdoc at Berkeley. In 1996, she was made an assistant professor at the Skirball Institute of New York University and was later promoted to full professor there. My son, Daniel, in the meantime, had become a professor of political science at the University of California, Los Angeles (UCLA), after completing his PhD at Harvard.

Princeton: Research on Scene Perception and on Distributed Attention

We spent the next 17 years in Princeton. Here again, I found excellent colleagues and friends, including Sam Glucksberg, Charlie Gross, Phil Johnson-Laird, Marcia Johnson, and later Jon Cohen, Sabine Kastner, and Ken Norman. While we were there, the department persuaded the university to develop a program in cognitive neuroscience and to buy an fMRI machine. At the end of the 20th century, psychology was becoming more and more influenced by and integrated with neuroscience. The development of imaging techniques with which brain processes could be explored during the performance of various cognitive and social tasks made it possible to link psychological processes with brain activity and to develop models of how brain and mind interact. The department hired Jon Cohen to develop this approach, and he was able to attract several excellent people to join the thriving Neuroscience Institute.

My research on the binding problem was still a central thread in my activity, and I encouraged my graduate students to learn the techniques of imaging and of electroencephalography and to try to explore the brain mechanisms involved in binding. But I also began to explore a new psychological question that had troubled me for some time.

I had always been aware of the implausibility of the idea that we can perceive the world only by serially scanning each object in turn. When we open our eyes on an unknown picture or scene we do not experience objects appearing one at a time as their features are bound by serial allocation of local attention. Clearly, we know a great deal about the scene as soon as it appears. This raises a question: What information do we get in the first glance with attention spread over the scene as a whole? We already knew that focused attention is not needed to detect the presence of a unique color, shape, or motion. An item will pop out of the field and attract focused attention if it is distinguished from other items by a single feature. We also knew that in the state of diffuse/spread attention people cannot accurately bind the features of objects. What then do observers see when their attention is spread over the whole display? I tried two ways of answering this question.

Semantic Categorization of Scenes

With my student, Karla Evans, I tried to reconcile results by Li et al. (2002), showing apparently attention-free high-level categorization of natural scenes, with our claim that binding features requires attention. Li et al. found that participants could detect the presence of animals in natural scenes concurrently with another attention-demanding task. We wondered whether their performance reflected the detection of disjunctive feature sets rather than high-level binding. We presented very rapid strings of pictures of natural scenes, with occasional animals (or vehicles) included, and explored what information was available when participants reported one of these targets. We found that they frequently failed to identify or to localize the targets that they had correctly detected, suggesting that detection was based on only partial processing. If we have evolved the ability to detect such natural features as fur, legs, wings, wheels, or windows, it may be possible to effect this high-level semantic processing through the detection of disjunctive feature sets without the necessity to bind these features together. Our participants showed severe attention limits when they were asked to identify two concurrent targets, although when they were asked only to detect their presence they performed quite well. Detection can be based on one or more target features, whereas identification usually requires binding (Evans and Treisman, 2005).

This interpretation requires us to expand our set of potential features to include more than elementary visual properties. There is in fact neural evidence for the existence of single neural units that respond to hands or faces (Gross et al., 1972). Considerable plasticity in neural coding has also been demonstrated through the existence of neural units in monkeys that respond to learned combinations of object features relevant to a particular task (e.g., Sigala and Logothetis, 2002; Riesenhuber et al., 2002). We have not proved this alternative interpretation of performance in the processing of natural scenes under extreme time pressure, but we believe it is an alternative to the claim that binding poses no limits in the real world perception of natural scenes.

Distributed Attention

Another contribution to solving the problem of perception in the real world despite the attention limits shown in our laboratory experiments comes from distributed as opposed to focused attention. It is possible that the information available differs when we spread our attention over a scene as a whole rather than focusing it narrowly on one object.

My research on this topic was prompted by a 2001 paper by Ariely, which suggested that people may form an accurate statistical representation of the characteristics of an array of objects without forming representations of individual objects. Ariely (2001) used two different tasks. In both, he showed a display of multiple circles followed by a single probe circle. One task was to say whether that probe circle had been in the previous display. Another was to say whether the circle was larger or smaller than the mean size of the circles in the whole display. The results were surprising: People were very bad at saying whether the probe circle had been in the display but very good at judging whether it was larger or smaller than the mean size. Also, the accuracy of the size judgments was unaffected by the number of items. With my graduate student, Sang Chul Chong, I showed that these judgments can be made very fast, within 50 msec, which rules out any possibility of serial scanning (Chong and Treisman, 2003). We also found that statistical processing did not require attention; accuracy was little impaired by a demanding concurrent task (Chong and Treisman, 2004).

The ability to evaluate averages at a glance is not restricted to the dimension of size. Tatiana Emmanouil and I found similar results for orientation and for speed of movement (Emmanouil and Treisman, 2008). I subsequently found averaging even for more complex stimuli, such as shapes. In Figure 5, the average of the four shapes on the left, shown on the right, is produced by taking the average location of each of the six vertices

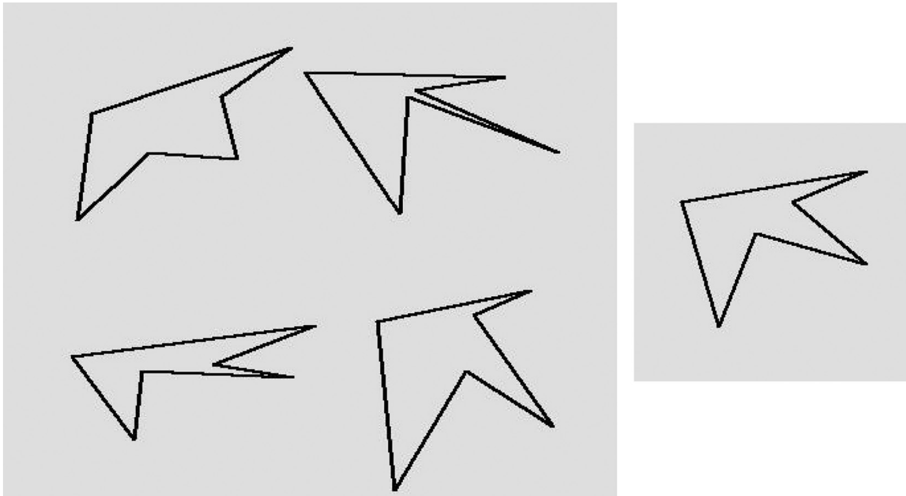

Fig. 5. Shapes used to test statistical averaging with distributed attention: The four shapes on the left are a typical display, and the one to the right is the average of the four in the display.

of each shape. When we gave observers a brief exposure of four shapes, then asked whether they recognized a new shape as one that had been included in the display, we found that they were more likely to "recognize" the average shape than to recognize any of the shapes that had, in fact, been presented.

The extraction of averages does not seem to require any effort. We showed that they can be estimated concurrently for two separate interspersed groups of items. We also found that the accuracy of the evaluation of the average was not reduced by dividing attention with another concurrent task. We used displays in which four shapes were interspersed with digits. Observers were required to attend to the digits interspersed in the display and report if a letter was present. They were almost as accurate in evaluating the average shape as they were without the extra letter-digit task, as shown by their tendency to report the average shape rather than one of the shapes that was actually presented in a recognition test. In a series of elegant studies by Jason Haberman and David Whitney (2007), observers could evaluate the average happiness of 16 faces from an exposure lasting less than 500 msec. Clearly, we are good at extracting a prototype from complex displays. Distributed attention supports a separate mode of global processing, which is distinct from the focused mode that binds and individuates separate objects.

The statistical capabilities of distributed attention suggest a solution to the puzzle that had troubled me. When first exposed to an unfamiliar scene, we can immediately segregate it into separate areas based on feature information. Top-down information (e.g., a wide area of green is likely to be a grassy field) provides hypotheses about the scene. Within each area, distributed attention provides statistical information about the objects it contains, allowing us to recognize a herd of sheep in the background. Meanwhile, more detailed representations are generated by focal attention binding local features at a rate of perhaps 20 to 40 a second, allowing us, for example, to recognize a friend's face in the crowd (Treisman, 2006).

During my years at Princeton, Shaul Hochstein from the Hebrew University of Jerusalem and I developed several joint grant proposals. We met two or three times a year, either in Princeton or in Israel, and had a large effect on each other's thinking—although (alas!) we did not get around to producing any joint publications. I was very impressed and influenced by his "reverse hierarchy" account of perception (Hochstein and Ahissar, 2002), and we had many discussions on how this related to my feature integration theory. One of the last experiments I did, together with my postdoc, Seth Bouvier, explored the possibility that feature binding is accomplished through a process of reentry, in which a first rapid pass through the visual system registers the features that are present and segregates the different areas of the scene (Bouvier and Treisman, 2010). Then, if required, processing returns to earlier visual stages to retrieve the locations of potential objects and to bind their features. We showed that masking the stimuli in ways that are known to specifically impair the perception of objects also impaired perception of conjunctions, leaving the features intact. This finding illustrates the benefit of combining our binding hypothesis with Shaul's reverse hierarchy theory.

My work has been recognized by a number of honors, including my election in 1989 to a fellowship of the Royal Society, London; in 1994, to the U.S. National Academy of Sciences; and in 1995, to the American Academy of Arts and Sciences. I won the Distinguished Scientific Contribution award of the American Psychological Association in 1990, the Grawemeyer prize in 2009, and (much to my surprise) the National Medal of Science in 2013. In 2002, Danny was awarded the Nobel Prize in Economics, and we had a dream week in Stockholm, together with the 16 guests we were allowed to share it with. In 2011, Danny published a book, *Thinking Fast and Slow*, outlining most of his past work and ideas, which to his surprise (but not mine) became and still is a best seller (Kahneman, 2011). I am very happy that his work is now collected and embodied in a single (large) volume, which will remain available for many future years.

Conclusions

The ideas I presented in this chapter have been quite controversial, and they are obviously provisional, reflecting the conclusions I reached at each stage in my research and all before I retired in 2010. The claims I made in the earlier papers have been expanded and/or modified in later papers, by me and by others. New findings have been made and alternative accounts proposed. In some cases, modifications to the original ideas were proposed (many by me), and in some cases, refutations were claimed (mostly by others). But that is the way that science progresses. A very useful sampling of developments, theoretical and empirical, can be found in a book edited by Jeremy Wolfe and Lynn Robertson (2012), which included a selection of my papers accompanied by commentaries of current experts. I was grateful to these colleagues and friends for their diligent work and grateful as well for the title of the collection, *From Perception to Consciousness: Searching with Anne Treisman*.

What are some general changes that seem to characterize this overview of my life in cognitive psychology? Psychology has changed enormously in the half century of my career, moving from a rather narrow behaviorism through the cognitive revolution to the interactions of linguistics, philosophy, and computer science under the cognitive science umbrella, and now with cognitive neuroscience, linking mind to brain.

Most obviously, the field has increased enormously in its number of findings and in their complexity. I checked the Web site of *Science* for the number of articles on selective attention published in each decade since 1950. It grew from 2 to 11,218—and that does not include such closely related topics as priming, distributed attention, or the underlying brain mechanisms. Inevitably, the papers have become far more specialized and detailed, but psychology is perhaps more interesting now than it ever was.

Probably the most important changes in my lifetime have been in the degree of interaction between behavioral findings in cognitive tasks and in our understanding of the neuroscience of the brain. The two fields met in the early years mainly in research on the effects of brain lesions on psychological tasks. Now, with the advent of brain imaging, they have essentially fused into one field, cognitive neuroscience. Partly as a result of this fusion, the behavioral models have moved from a pipeline of successive stages or operations to a much more interactive system with as many feedback as feedforward pathways. In perception, the notion of reentry allows stored sensory data to be compared with tentative hypotheses based on more central processing to either confirm or discard them.

One topic that I have not discussed much is the control of attention—how we select which stimuli to attend to and how we set the size of the focus. From the beginning, I assumed that the focus could be adjusted depending on the task, analogous to the zoom lens model of Charles Eriksen and James St. James (1986). My recent research on how distributed attention differs from focused attention suggests the differences that may result as we adjust the size of the focus. But the mechanisms that control the setting are now an important topic of research, with the frontal lobes implicated in the allocation of attention.

I have worked with many graduate students and have learned a lot from them, but I have not often collaborated with colleagues in my research.

Danny and I worked together on the moving-object project as well as on a theoretical paper on attention. Lynn Robertson and I had a fascinating time working together to explore feature binding in a Balint's patient and also in some patients with neglect. Finally I learned a great deal in my interactions with Shaul Hochstein and our two theories benefited from the discussions that modified each of them in turn. But otherwise, I mostly worked alone, perhaps because I am too fond of having my own way!

I was fortunate in being able to combine a large family with an academic career, and I am now blessed with three successful children and four adorable granddaughters. Stephen, my Down syndrome child, is perhaps the happiest of them all. My impression is that combining career and family is getting harder as the pace and pressure of academic work increase. Many young academic women are trying to decide whether they should have one baby or none. I find this sad, and I am not sure that so much is gained by accelerating the pace of papers published, tenure demands, and grant getting. If less emphasis were placed on quantity and more on quality, science would be unlikely to suffer and our personal and social lives would be enriched and more civilized.

The part of my career that I enjoyed perhaps the most was the interactions with my graduate and postdoctoral students. In Berkeley and at Princeton they formed a community, enjoying one another's company and supporting one another, as well as getting excited about our findings. In Berkeley, where Danny and I shared a large space, and in the early years in Princeton (before the building was rebuilt and we were relegated to small labs shared by the whole area), I spent most of my working day in the open area of the lab talking to students, trying things out, arguing about the results. The weekly meetings in which students presented their findings and ideas were an enjoyable social event, as were the birthdays when a talented baker would bring a cake.

I have mentioned some of my graduate students in connection with the joint papers that we wrote. In addition, I have fond memories of Gillian Cohen, Nigel Harvey, Maryanne Martin, Gail Walker-Smith, Robin Russell, and Geoff Cummings at Oxford; Mirjam Eglin, Randy Paterson, and Debbie Butler at the University of British Columbia; Marcia Grabowecky, Beena Khurana, Kathy O'Craven, Meg Wilson, and Todd Horowitz at the University of California, Berkeley; and Tracey Leacock, Paul Downing, John Zhang, Shlomo Sher, Mary Wheeler, and Ming Meng at Princeton. My postdoctoral students also often had an important influence on my work: they include Bill Prinzmetal in Vancouver; Nancy Kanwisher, and Nilli Lavie in Berkeley; and Ofer Fein, Miriam Berkowitz, Robert Ward, Adriane Seiffert, Liqiang Huang, Hagit Magen, Seth Bouvier, and Liat Goldfarb in Princeton. Working with them was a privilege and a pleasure.

As I said at the beginning of this chapter, I have been very lucky in the timing of my career. It has been an exciting ride and impressive progress has been made. I have thoroughly enjoyed being part of the evolution of ideas and communicating them to students in my undergraduate teaching. What I tried to pass on to my graduate students and postdocs is the thrill of collecting and making sense of data, testing ideas within the larger framework of current research, and generating their own new insights into the human mind. Although this chapter appears in a volume dedicated to neuroscience, it contains mostly behavioral experiments. I have always tried to interpret the results in terms of the underlying neural mechanisms when these were known. I hope that, together with many others, I have illustrated how behavioral questions and data are central to our understanding of the brain. To understand the brain's function one must look at the problems it is trying to solve and the errors and latencies with which it performs the tasks that face it. I do not think any account can be complete without this functional level of description.

Selected Bibliography

Ariely, D. Seeing sets: Representation by statistical properties. *Psych Sci* 2001;12:157–162.

Bouvier, S., Treisman, A. Visual feature binding requires reentry. *Psych Science* 2010;21:200–204.

Broadbent, D.E. *Perception and Communication*. London : Pergamon Press, 1958.

Bruner, J.S., Goodnow, J.J., Austin, G.A. *A Study of Thinking*. London: Chapman & Hall, Ltd. 1956.

Cherry, C. *On Human Communication*. New York: Wiley, 1957.

Chong, S.-C., Treisman, A. Representation of statistical properties. *Vis Res* 2003;43:393–404.

Chong, S.-C.,Treisman, A. Attentional spread in the statistical processing of visual displays. *Perc and Psychphys* 2004;66:1282–1294.

Corteen, R. S., Wood, B. Autonomic responses to shock-associated words in an unattended channel. *J Exp Psych* 1972;94:308–313.

Dehaene, S., Naccache, L., Le Clec, H.G., Koechlin, E., Mueller, M., Dehaene-Lambert, G., Van de Moortele, P-F., Le Bihan, D. Imaging unconscious semantic priming. *Nature* 1988;395:597–600.

DeSchepper, B., Treisman, A, Visual memory for novel shapes: implicit coding without attention. *J Exp Psy: Learning, Memory and Cognition* 1996;22:27–47.

Desimone, R., Duncan, J. Neural mechanisms of selective visual attention *Ann Rev Neurosci* 1995;18:193–222.

Deutsch, J.A., Deutsch, D. Attention: some theoretical considerations. *Psy Rev* 1963;70:80–90.

Emmanouil, T.A., Treisman, A. Dividing attention across feature dimensions in statistical processing of perceptual groups. *Perc and Psychphys* 2008;70:946–954.

Eriksen, C.W., St. James, J.D. Attention within and around the field of focal attention. *Perc Psychophys* 1986;40:225–240.

Evans, K., Treisman, A. Perception of objects in natural scenes: is it really attention-free? *J. Exper Psych: Human Perc. Perf* 2005;31:1476–1492.

Freedman, D.J., Riesenhuber, M., Poggio, T., Miller, E.K. Visual categorization and the primate prefrontal cortex: neurophysiology and behavior. *J Neurophysiol* 2002;88:930–942.

Gregory, R. *Eye and Brain: The Psychology of Seeing*. London: Weidenfeld and Nicolson, 1956.

Gross, C., Rocha-Miranda, C.E., Bender, D.B. Visual properties of neurons in inferotemporal cortex of the macaque. *J Neurophysiol* 1972;35:96–111.

Haberman, J. Whitney, D. Rapid extraction of mean emotion and gender from sets of faces. *Current Biology* 2007;17:R751–R753.

Hick, W.E. On the rate of gain of information. *Quart Exp Psych* 1952;4:11–26.

Hillyard, S.A., Hink, R.F., Schwent, V.L., Picton, T.W. Electrical signs of selective attention in the human brain. *Science* 1973;182:177–180.

Hochstein, S., Ahissar, M. View from the top: hierarchies and reverse hierarchies in the visual system. *Neuron* 2002;36:791–804.

Hubel, D.H., Wiesel, T.N. Receptive fields of single neurons in the cat's striate cortex. *J Physiol* 1959;148:574–591.

Kahneman, D. *Thinking Fast and Slow*. New York: Farrar, Straus, Giroux, 2011.

Kahneman, D., Treisman, A., Gibbs, B. The reviewing of object files: object-specific integration of information. *Cog Psyc* 1992;24:175–219.

Lavie, N. Perceptual load as a necessary condition for selective attention. *J.Exp Psych: Hum Perc and Perf* 1995;21:451–468.

Li, F., VanRullen, R., Koch, C., Perona, P. Rapid natural scene categorization in the near absence of attention. *Proc Nat Acad Sci USA* 2002;99:9596–9601.

Marcel, A.J. Conscious and unconscious perception: an approach to the relations between phenomenal exoerience and perceptual processes. *Cog Psych* 1983;15:271–300.

Miller, G. The magical number seven, plus or minus two: some limits on our capacity for processing information. *Psych Rev* 1956;63:81–97.

Miller, G.A., Galanter, E., Pribram, K.H. *Plans and the Structure of Behavior*. New York: Henry Holt, 1960.

Moray, N. Attention in dichotic listening: affective cues and the influence of instructions. *Quart J Exp Psych* 1958;11:56–60.

Morton J.A. Interaction of information in word recognition. *Psych Rev* 1969;76:165–178.

Musen, G., Treisman, A. Implicit and explicit memory for visual patterns. *J Exp Psych: Learn Mem Cog* 1990;16:127–137.

Nakayama, K., Silverman, G.H. Serial and parallel processing of visual feature conjunctions. *Nature* 1986;320:264–265.

Neisser, U. *Cognitive Psychology*. New York: Appleton-Century-Crofts, 1967.

Riesenhuber, M., Poggio, T. Neural mechanisms of object recognition. *Current Opinion in Neurobiology* 2002;12:162–168.

Robertson, L.C., Treisman, A., Friedman-Hill, S., Grabowecky, M. The interaction of spatial and object pathways: evidence from Balint's syndrome. *J Cog Neurosci* 1997;9:295–317.

Sigala, N., Logothetis, N.K. Visual categorization shapes feature selectivity in the primate temporal cortex. *Nature* 2002;415:318–320.

Sternberg, S. Memory scanning: Mental processes revealed by reaction-time experiments. *American Scientist* 1969;57:421–457.
Stroop, J.R. Studies of interference in serial verbal reactions. *J Exp Psych* 1935;18:643–662.
Sutherland, N.S. Stimulus analyzing mechanisms. In *Mechanization of Thought Processes,* Vol. 2. London: Her Majesty's Stationery Office, 1959.
Tipper, S.P. The negative priming effect: inhibitory effects of ignored primes. *Quart J Exp Psyc* 1985;37A:571–590.
Treisman, A. Strategies and models of selective attention. *Psy Rev* 1969;76:282–299.
Treisman, A. Perceptual grouping and attention in visual search for features and for objects. *J Exp Psych: Hum Perc Perf* 1982;8:194–214.
Treisman, A. Features and objects: The Fourteenth Bartlett Memorial Lecture. *Quart J Exp Psych,* 1988;40A:201–237.
Treisman, A. How the deployment of attention determines what we see. *Vis Cog* 2006;14:411–443.
Treisman, A., DeSchepper, B. Object tokens, attention, and visual memory. In *Attention and Performance: XVI Information Integration in Perception and Communication*, T. Inui and J. McClelland (eds.), pp. 15–46. Cambridge, MA: MIT Press, 1996.
Treisman, A., Gelade, G. A feature integration theory of attention. *Cog Psychol* 1980;2:97–136.
Treisman, A., Gormican, S. Feature analysis in early vision: evidence from search asymmetries. *Psych Rev* 1998;95:15–48.
Treisman, A., Sato, S. Conjunction search revisited. *J Exp Psy: Hum Perc and Perf* 1990;16:459–478.
Treisman, A., Schmidt, H. Illusory conjunctions in the perception of objects. *Cog Psy* 1982;14:107–141.
Treisman, A., Souther, J. Search asymmetry: a diagnostic for preattentive processing of separable features. *J Exp Psych: Gen* 1985;114:285–310.
Treisman, A., Vieira, A., Hayes, A. Automaticity and preattentive processing. *Amer J Psych* 1992;105:341–362.
Treisman, A.M. Contextual cues in selective listening. *Quart J Exper Psych* 1960;12:242–248.
Treisman, A.M. Binocular rivalry and stereoscopic depth perception. *Quart J Exp Psych* 1962;14:23–36.
Treisman, A.M., Riley, J.G. Is selective attention selective perception or selective response? A further test. *J Exp Psych* 1969;79:27–34.
Tulving E. *Elements of Episodic Memory*. Oxford: Clarendon Press, 1983.
Van Essen, D.C., Anderson, C.H., Felleman, D.J. Information processing in the primate visual system: an integrated systems perspective. *Science* 1992;255:419–423.
Wolfe, J., Cave, K.R., Franzel, S.S.L. Guided search: an alternative to the feature integration model for visual search. *J Exp Psyc: Hum Perc and Perf* 1989;15:419–433.
Wolfe, J., Robertson, L. (eds.) *From Perception to Consciousness*. Oxford: Oxford University Press, 2012.
Zeki, S. Functional specialization in the visual cortex of the rhesus monkey. *Nature* 1978;274:423–428.
Zeki, S. *A Vision of the Brain*. Oxford: Blackwell, 1993.