Memories of Phil Bryden

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Abstract

Phil Bryden was a seminal figure in the development of the field of cerebral lateralization in the last half of the 20th Century. Three colleagues and friends of Phil reminisce about their professional and personal relationships with Phil and his wide-ranging influence in the field and in their own careers.

Keywords

Phil Bryden; history of Psychology; cerebral lateralization

Memories of Phil Bryden from Three Friends and Colleagues

Much as we three would love to have met to compare notes, to reminisce about Phil and how much he meant to us, recording the results to produce a transcript, we all live on different continents and are all happily busy working (ICM), retired-but-really-still-working (MCC) or really retired (BB-F), so couldn't get together. Because Mike knew Phil before Chris and I (BB-F) did, we thought his story should go first.

Mike's story: The early years

When I arrived at McGill in August 1963 to begin my PhD, I already knew of Phil Bryden's work. I had come from Auckland, where I had completed a masters degree with Hubert ("Barney") Sampson, himself a graduate of McGill and newly appointed Professor of Psychology at Auckland, and I had been partially basted in McGill psychology. Those days were the beginnings of what would later be called cognitive psychology, a title conferred by Ulric Neisser's stylish book of the same name, published in 1967. Earlier, though, the major influences were perhaps Donald Broadbent's 1958 book *Perception and Communication*, and Karl Lashley's provocative but elusive chapter "The problem of serial order in behavior," which appeared in 1951. Behind it all, though, was Donald Hebb, influential but almost never appearing in joint publication with the talented individuals he attracted to McGill, yet almost every article from McGill in those days acknowledged a grant to Hebb. In various ways, I think we were all trying to escape behaviorism, which still held a vice-like grip on psychology, especially in North America.

When I arrived in Montreal, Phil had just left McGill to take up a position at a new university, founded on "mud and dreams," in Waterloo, Ontario. He did make a return visit to McGill late in 1963, and I was delighted to meet him, not least because he took an interest in my work. I was, I think, confused and uncertain as to my future in psychology. Barney

Sampson had been a charismatic influence in Auckland; most of us, though, found him eloquent but incomprehensible, qualities that flowed, as it were, from his fondness for alcohol. Phil, by contrast, was lucid, and I began to see what it was all about. I came to look upon Phil as a wise Owl, with his round face and large spectacles, his disarming smile, and slow, considered manner of speech that somehow conveyed both encouragement and authority. I was a sort of befuddled Pooh bear.

In 1952, Mortimer Mishkin and Donald Forgays, both then also at McGill, had published a seminal paper showing that tachistoscopically presented words were recognized more accurately if presented in the right than in the left visual field. This was reversed in readers of Yiddish. Mishkin and Forgays suggested that the asymmetry was due to selective training of parts of the retinal surface, cautiously suggesting that this might also have induced opposite cerebral dominance for reading in the two language groups. But cerebral asymmetry scarcely featured over the following decade even as a tachistoscopic era became established at McGill, at least among those doing human experiments and not inserting electrodes into rats' brains. Woodburn (Woody) Heron, later Phil's PhD supervisor, sought explanations of field effects in tachistoscopic perception in terms of eye movements, influenced by Hebb's notion that perception itself was heavily dependent on successive visual fixations. Phil's first two papers on this topic, published in 1960 and 1961, effectively ruled out eye movements as a contributor to the accuracy or order in which tachistoscopically presented items were reported. Doreen Kimura had reached a similar conclusion in a paper published in 1959.

But laterality seeped in, and eventually dominated. Doreen Kimura, then at the Montreal Neurological Institute, adapted another technique that was later to sit beside the tachistoscope in studies of laterality. This was dichotic listening—the simultaneous presentation of material, via headphones, to each ear. The early studies of Broadbent and others in England had been concerned with attention and the order in which people reported

items presented dichotically, but in two papers published in 1961 Doreen showed that a temporal-lobe excision impaired report of digits arriving at the contralateral ear, setting the stage for the theory that the right-ear advantage evident in most people reflected left-cerebral dominance. Phil also picked up on dichotic listening as a measure of cerebral asymmetry in a paper published in 1963, but he also published a number of dichotic-listening papers dealing with problems of attention and order of report, and not with laterality.

Work on tachistoscopic perception also gradually became tethered to cerebral asymmetry, although it took a while for Phil to be persuaded. A paper he published with Christopher Rainey in 1963 showed a right visual-field advantage for tachistoscopically presented letters and familiar geometric forms, and noted Kimura's earlier suggestion that the right-ear advantage might be due to left-hemispheric dominance. With characteristic caution, they wrote "While the present experiment present no concrete evidence relevant to this suggestion the data would seem to be consistent with it" (Bryden & Rainey, 1963, p. 571).

The rest is history—or more accurately, history and herstory. Between them, but to a large extent independently, Phil and Doreen went on to establish laterality dynasties, much of it based on those two work-horses, tachistoscopic perception and dichotic listening. Despite the later influence of brain imaging these techniques are still with us, but sadly Phil and Doreen are not—Doreen died in Vancouver in 2013. And of course Phil took laterality into many other directions, later developing a special interest in handedness and its ramifications. For some 20 years, he was Mr Laterality, and much loved by large numbers of students and hangers-on who passed through his domain at Waterloo. Like Hebb, and indeed in something of a McGill tradition of tolerance, he encouraged his students to develop their own ideas. He coveted new theories, but was always a rigorous and careful researcher.

And I might add that it was a pleasure to re-read the old articles, Phil's and others', many of them published in the *Canadian Journal of Psychology*, in many respects the

forerunner of *Laterality* and in its day a journal of experimental psychology with strong international presence. I marvel at the succinctness of these articles, the crisp Methods and Results, the absence of sprawling Introductions and Discussion that blight and bloat our modern scientific literature.

After my own PhD at McGill in 1965 and a year as a postdoc there with my supervisor, the wonderful and vastly tolerant Dalbir Bindra, I returned to Auckland for two years, but then joined the faculty at McGill from 1968 to 1977. During that period I saw Phil quite often, and once (I've forgotten exactly when [I think it was 1985, Mike]) spent a week in Waterloo at his invitation. Even after my return to Auckland, I saw him at conferences and during more fleeting visits to Waterloo, and he once paid us a memorable visit to Auckland with his wife Pat Rowe and daughter Pam, who is now herself an enthusiastic and productive laterality researcher. Chris McManus entered Phil's orbit later, and in the early 1990s the three of us, from different hemispheres, hatched a plot to establish a new journal, to be called *Laterality*. I was slightly skeptical, wondering if the laterality era might be drawing to a close by the early 1990s, but am delighted that it is still going strong after twenty years.

My last encounter involving Phil was the most poignant. I was due to give a talk at the International Congress of Psychology in Montreal in 1996, and Phil was to introduce me. The day before my talk, Pierre Jolicoeur came up to me, looking stricken, and simply said "Phil died." That was a terrible day, and I don't remember anything else about the Congress.

1996 was the year *Laterality* first appeared.

Chris's story: Stimulating the brain cells

I knew Phil for less than a decade, from June 1987 to July 1996, but he reinvigorated my research interests in handedness and lateralization, and his influence continued long after that.

Above all, though, he was just great fun to work with, being fascinated by everything, psychological or otherwise. I did, though, never quite manage to understand his enthusiasm for baseball, or even properly to understand the rules, despite having sat through several Blue Jays games. At Phil's memorial meeting in 1997, which Barbara Bulman-Fleming co-organized, we were particularly thrilled to include a talk by Stephen Goldstein, whose work on an evolutionary stable strategy for handedness in baseball (Goldstein & Young, 1996) would surely have been the talk that Phil would have enjoyed most.

It all began with Phil in 1987 with an airmail letter. Looking back it is a shock to realize how in an age without faxes or internet we relied on old-fashioned air-mail for most purposes. By 1990 fax was becoming commonplace, and by the time of Phil's death the emails had been bouncing around for a few years, even if those before 1994 seem not to have been properly archived.

Phil had written because he was coming over to the UK in June as the Experimental Psychology Society (EPS) and the Canadian Society were having a joint meeting in Oxford, and suggesting it would be nice to meet. In Oxford I found Phil standing in front of his poster, and we immediately just got on, sharing ideas and thoughts about handedness and lateralization. We sat outside in the sun chatting for an hour or two, and later that evening I met him and his daughter Penny for a curry (which I'm embarrassed to recall was pretty dire). There was a strong Canadian presence in Oxford and Phil seemed to know everyone, and it was good in particular to talk with Clare Porac and Justine Sergent. Already plans were being hatched for a visit to Canada.

The airmails bounced back and forth across the Atlantic over the next year, as well as an occasional and very exotic fax. At first we weren't certain what to work together on, but we both got more and more interested in the theories of Geschwind, Behan and Galaburda (or GBG as we soon nicknamed them). In 1982, G&B had published a provocative paper

claiming that handedness and immune disorders were linked (Geschwind & Behan, 1982), three years later G&G had published three large papers in *Archives of Neurology* linking lateralization to what seemed to be almost everything (Geschwind & Galaburda, 1985a; Geschwind & Galaburda, 1985b; Geschwind & Galaburda, 1985c), with it all being put into a book in 1985 (Geschwind & Galaburda, 1987). Everyone seemed to be aware of the GBG theory, but the theory was so protean in its nature that it seemingly could be invoked to explain anything and everything. Clearly a critical review was needed, and it seemed a perfect topic on which to collaborate. I'd arrived in Canada on April 24th, and instantly found Phil, like most of his fellow Canadians, deep in the middle of the end of April ritual of the filing of the tax return. While he did that I recovered from my jet lag and read *Cerebral Lateralization* from cover to cover. Phil and I spent many mornings on the Waterloo Campus in the newly opened and wonderfully modern and hi-tech Davis Centre, the computer centre designed around the idea of a micro-chip, and which had more types of coffee than I realized existed, complemented by a similar variety of muffins. As we grappled with making sense of the theory we slowly got more and more annoyed with GBG, realising that a major problem was how to test such a theory. Ever since reading David Kenny's book on path modelling (Kenny, 1979), I'd been getting to grips with structural equation modelling, and it provided the conceptual framework for making sense of the theory. Slowly our ever-expanding diagram explicating the model grew and grew, and we pasted ever more bits of paper to the sides, eventually including everything that GBG seemed to be saying in causal terms. GBG was "a grand theory", and in that lay both its attraction and its vulnerability, with testability being a major issue. Phil at that time was on a three-year Killam Research Fellowship which gave him extended leave from teaching, and so in the autumn of 1989 he came over to England, working both at UCL, and also spending time with Geoff Underwood in Nottingham, who was also a long-time collaborator. Eventually the GBG paper was finished and sent off (McManus & Bryden, 1991).

Phil loved conferences, and he took me to many, small and large, be they in Southern Ontario (particularly the influential SONG – Southern Ontario Neuropsychology Group), or further afield, and in 1989 we went to CPA in Halifax (memorable not least for the welcome from a prominent local politician who said he hoped we would enjoy the "liquid sunshine", which indeed poured down for the duration). The next year the newcomer on the block (for me) was TENNET (Theoretical and Experimental Neuropsychology – Neuropsychologie Expérimentale et Théorique), its palindromic abbreviation showing its bilingual origins, the meeting always being held in Montreal, a city which Phil loved and visited as often as possible. Phil knew everyone at all of these meetings and loved networking, and in particular bringing together his "family", the group of researchers working around him at Waterloo, with researchers from elsewhere. Later, describing his early experiences of Canadian conferences, I remember Phil describing how when he was at McGill, if, say, CPA was meeting in Edmonton, then the entire department would get on a giant Canadian Pacific train, and for three days would eat, sleep and above all talk on the train for the entire journey, everyone knowing everything that everyone else was doing research-wise. And on the way back they would do the same about all of the papers they had heard. Only after Phil's death did I come across the picture of him and Doreen Kimura in 1958 (figure 1), *en route* by train to a conference, and it evokes the time perfectly.

After our success with the GBG paper, Phil was keen to take things further, and when I was over in 1990 we finalized plans for a much lengthier visit the next year, from May until December 1991. Once again, GBG was to be the focus, but this time concentrating on a proper meta-analysis of what seemed to be a key issue in the GBG theory, of an association between handedness and auto-immune disorders, not least because the association seemed to come so far from left field (as it were). I had been dabbling in meta-analyses before that (Seddon & McManus, 1991) (Van Horn & McManus, 1992), and this was a perfect candidate

for a research synthesis, although meta-analysis was still a sport for enthusiastic amateurs, with no dedicated software, and plenty of doubts in the air (and at the time the *Lancet* was publishing articles counselling caution coupled with optimism (Anonymous, 1987; Thompson & Pocock, 1991)). Barbara Bulman-Fleming was by now a central member of Phil's family of researchers, and she helped to integrate the mass of data which eventually became the meta-analysis, and which, after our surnames, we inevitably called BMB (Bryden, McManus, & Bulman-Fleming, 1994a; Bryden, McManus, & Bulman-Fleming, 1994b; McManus, Bryden, & Bulman-Fleming, 1993).

Once Phil and I had got the bug for meta-analysis, a further one on handedness and eye-dominance was to follow, with Rick Bourassa doing the legwork on that one (Bourassa, McManus, & Bryden, 1996), and the paper appearing in the first volume of *Laterality*. A journal that specialized in lateralization was something that Phil and I had talked about whenever we met (and I was over in Canada most years, 1992 being an exception, but that year Phil and I met in Belgium at the International Congress, one of Phil's favourite meetings). A recurring frustration was that no journals seemed to treat lateralization seriously, and the obvious solution was a journal specialising in it, and helping to form the field. Michael Foerster, at Psychology Press (then a part of Lawrence Erlbaum) was a dynamic, enthusiastic publisher who was concentrating on psychology, and in a phone call he told me that he would be interested in a new journal. I was in Waterloo in May 1994 as was Michael Corballis so it was the perfect opportunity to put together a proposal with three editors from three continents and three hemispheres (east, west and southern). We sent it to the publishers, who sent it out for review and, as they said, "we have had quite a mixed response!" Because this year is not only the 20th anniversary of Phil's death, but also the 20th anniversary of the birth of *Laterality*, the journal now beginning its 20th volume, a few of those comments might be of interest:

"Is a new journal needed in this field? My response must be no, there are too many specialist journals already. More specifically, laterality is not interesting in itself… It is my strong impression that most serious academics pay no attention to laterality literature – because there is so little work of any substance".

"I am in two minds about this proposal. … The laterality industry is in decline, and I wonder if, as a consequence, the arguments for a journal devoted specifically to the topic are becoming weaker."

"Certainly there will be enough material to fill a journal; the problem is rather that I suspect [the editors] may be inundated with poor quality manuscripts, and have to spend a lot of time sifting out the good ones."

"The question that looms in my mind is whether we will advance our understanding and utilization of research on laterality to inform general theories of cerebral function by the creation of a new journal… "

"[Notes from a phone discussion]. Liked the idea, but found the topic area too narrow – suggested calling it *Brain Dominance* (is this too narrow though?)."

"This looks like a promising proposal. … As the outline states, there is a great deal of research in this field. Much of it is of poor quality, but I would trust this editorial team to adopt stringent standards. Altogether I am enthusiastic."

The reports were sent to a distinguished arbitrator, who said:

"What a mixture of opinions! I have read all of those and pondered on them. I know [the editors] all quite well. I think the critical point is that everyone agrees that all three are good, and capable of sifting through the dross of laterality literature to find the gold – and there is gold there all right. A second important point is that this is still a popular topic in

psychology, so the journal should sell well (including to the many people who generate the dross). I think, taking everything into consideration, that LEA should go ahead with this. I expect it to succeed".

Reader, he was right! On the 8th September 1994 I sent a fax to Phil and Mike saying "Well, they've bitten!", and the journal went ahead. A few weeks later Phil was in the UK, and on the 19th October we had a day out by the seaside to visit the Psychology Press offices in Hove. Of course there were still complex decisions to be made, such as what the cover should look like... Phil, Mike and I were sent various renderings by the designer, in what were called "the stripey graphic", "the horizontally-split circle" and "the vertically-split circle", all of which could come in vibrant pink, creamy-yellow, orangey-red, and greenish-turquoise. For those reading the print edition, turn back and you will see we chose the vertically-split, greenish turquoise; for the horrors that might have been, see figure 2 (and reading online will get a better idea of that image). The designer had been asked to produce "an abstract representation of the concept of laterality, based around the idea of sidedness, contrast and balance [and to be] bold and striking". And it works well and is still looking good twenty years on. The first issue of *Laterality* came out at the beginning of 1996, and in November 1997 *Nature* reviewed the new journal and said, "*Laterality* will provide lively and welcome reading for those interested in mulling over how and why the left and right halves of mammalian brains are different" (Purves & White, 1997).

There was a decade in which it could really be said that the centre of research in lateralization was in Southern Ontario, not only in Waterloo, but in Guelph, St. Catherine's, London (Ont.), and other places, with detours to Montreal for TENNET in particular. And Phil was the spider at the middle of that web, with connections to almost all the key players, both in Canada, the US, and further afield, to Norway, India, Japan, Australia and New Zealand. A particular meeting of note was the joint EPS/CSBBCS (Canadian Society for Brain,

Behaviour and Cognitive Science) in Toronto, which on 17th July 1993 held a symposium organized by Phil and Dorothy Bishop, with a title that is still as good now as it was then: "Why do humans show handedness and does it matter?". Many key players were present, and those giving talks included Marian Annett, Dorothy Bishop, Phil Bryden, Stan Coren, Chris McManus, George Michel, Michael Peters, Clare Porac and Runa Steenhuis. Figure 3 shows Phil with George Michel and Stan Coren at the dinner that evening (figure 3).

I last visited Phil in June 1996. It was a working visit, rather than a conferencing visit, and we spent a lot of time batting around ideas, both in Waterloo and at his cottage at Sauble Beach on Lake Ontario. As Phil put it, we were "stimulating each other's brain cells". One of the big ideas was to apply to the Human Frontier Science Program for a large multicentre grant, although of course that never happened in the end. The other idea was driven by Phil's cross-cultural studies of handedness, particularly with Maharaj Singh, where despite equivalent questionnaire measures, it was clear that the rate of left-handedness in India was half of that in Canada (Singh & Bryden, 1994), and similar work with Yokahida Ida was also finding a large difference between Canada and Japan (Ida & Bryden, 1996). Phil also had a new draft paper with Maharaj Singh and Tim Rogers, dated "July 14/96" on my copy, which was probably never published but is now available on my website (Bryden, Singh, & Rogers, 1996). There was a joint realization between Phil and me that, although simple prevalence data could not distinguish genetic and cultural explanations, with family data it was possible to distinguish genetic effects from learned, cultural effects, and hence ask whether gene frequencies might differ between societies. That idea took a long while to fructify, and it was only properly explained in a paper written in 2009 (McManus, 2009).

Phil died on 18th August in Montreal at the International Congress, which he had gone to after travelling to various other places. The last two emails I can find from him were sent just before he left Waterloo on 8th August, one going to all members of the Laterality Board,

inviting any who would be in Montreal to lunch, and the other, sent fifteen minutes later, was to me about the grant application. We were still sorting out the finances, and it ended, "Think big!"

To finish on a happier note, when Phil was 60 his lab had organized a surprise celebration. One of the contributions to it was emailed by one "William Michael Corblake", and it is the perfect way to end.

The Bryden

Bryden! Bryden! Burning bright
In the forest of the night
What immortal hand or eye
Escapes thy feared asymmetry?

Barbara's story: Mutual admiration

It was Phil's one and only question (as I recall) at my PhD orals in 1988, almost exactly twenty-seven years ago, that made me want to get to know him better. Characteristically serious and searching, it probably hit at what my weakest characteristic was as a scholar, and yet it was delivered without nastiness or any desire to embarrass: "So what?!" he asked me. I had looked into the effects of maternal environment on the incidence of an anomaly of corpus callosum development in an inbred strain of mouse and had just hinted at the possible significance of my findings at the end of my thesis. At the beginning of my graduate work, Mike Corballis's 1984 visit to the University of Waterloo (UW), as well as an undergraduate course in Biopsychology at UW in 1982, had sparked my interest in lateralization, and Phil was on my thesis committee and indicated a real interest in the animal research. This was really welcome, as it was rather under siege at the time at Waterloo.

My post-doctoral year under Pat Wainwright's direction in the Department of Health Studies at Waterloo focused on pawedness in mice, so Phil and I started to talk and become interested in what each other was up to. Gradually, we began to work more and more as a team, he engaging me in human laterality research and I involving him in my pawedness studies. Figure 4, 7? (figure 4, or 7?) shows the two of us at the INS meetings in Seattle, WA in the winter of 1995.

Phil had some wonderful graduate students at UW at the time including, but not limited to, our esteemed co-editors of this special edition, Gina Grimshaw and Dan Voyer. Phil and I had an enormous mutual respect for each other's abilities, and I think were complementary in many ways, he being the big thinker and ideas man, and my being the people-loving organizational type. So, we had a great time co-supervising several undergraduate students from Systems Design Engineering, for example, who tried very hard to come up with an apparatus to automatically classify paw reaches by our mice – without complete success it must be said! Also, Tim Rogers was a bright Psychology undergraduate at the time, who has since gone on to a stellar career in Cognitive Neuroscience – we had a wonderful time with Tim's foray into paw preference research and I will never forget his Herculean efforts to attend Phil's memorial service at Conrad Grebel College at UW in August of 1996. Near the end of his PhD studies, Lorin Elias, now at the University of Saskatchewan, was cheated out of Phil's guidance as a supervisor, Chris McManus and I trying to fill in after Phil's untimely death. Fortunately, Lorin was one of those students who need very little supervision.

Naturally, not all students at Waterloo were stellar and well-rounded, though, and I consider it one of Phil's most glorious characteristics that he looked for what were people's strengths and celebrated those, all the while doing everything he could to encourage their overcoming their weaknesses. Notable also was his desire to get to know the whole student, not just his or her academic interests. Those who got to know Phil well were crazy about him. He would

have been absolutely tickled had he known how much he and his work meant to so many people, and that this wonderful special edition was put together in his honour.

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Figure captions

Figure 1: Phil Bryden and Doreen Kimura in 1958 on their way by train to a CPA conference.

Figure 2: Alternative proposed covers for *Laterality.*

Figure 3: Phil Bryden (centre) with George Michel (left) and Stan Coren (right) at a dinner after the EPS/CSBBCS meeting in July 1993.

Figure 4: Phil Bryden in deep winter at the University of Waterloo in February 1993.

Figure 5: Phil Bryden in Barbara Bulman-Fleming's office in Waterloo, probably July 1993.

Figure 6: Phil Bryden in Barbara Bulman-Fleming's office in Waterloo, with Daniel Voyer (left) and Chris McManus (right) in November 1991.

Figure 4 (or 7?): Barbara Bulman-Fleming and Phil Bryden in front of their poster at the INS meetings in Seattle, February, 1995.

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