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Hans Eysenck at the Institute of Psychiatry — the later years

I remember meeting Hans for the first time when he was the external examiner for my PhD thesis in 1981. Thanks to Paul Kline, my supervisor at Exeter University, an arrangement had been made that gave me access to the EPQ Gallup sample data that Hans and Sybil Eysenck had collected during the late 1970s. So, given the number of analyses that I had undertaken on these data and other EPQ sets collected by myself, it was no surprise that Hans agreed to act as an external examiner. He was both charming and rigorous.

I did not see Hans again until 1983 — when out of the blue he contacted me to obtain a couple of references and clarify some results from my PhD thesis, for a paper he had been working on. I was unemployed at the time. He wondered if I was interested and able to help him in some new work on the biological correlates of IQ. Frankly, I would have helped him peel wallpaper with Chinese chopsticks if it meant working for him! Anyway, I travelled to the Institute to meet with him. Within 15 minutes of meeting, I had been hired as his postdoctoral researcher to work on something called the Hendrickson paradigm, starting work in a few days time. Further, it appeared I was also likely to be helping his wife, Sybil Eysenck, on something to do with cross-cultural analyses of the EPQ. This decisiveness was a hallmark feature of Hans. I remember saying to him a few years later just how lucky I had been in picking up the 'phone that day. His reply was that the luck was all his — he had been looking for someone with some experience in physiological psychology as well as general psychometrics, statistics, and programming, and had never even thought about me! It was just that he needed those references and information, remembered my background while talking with me, and realised that I might be just what he was looking for. I have a sneaking suspicion that what really clinched it for Hans was when he heard I played match tennis for my local tennis club! The last thing he'd said to me when I left his room that day was "make sure you bring your tennis kit in when you start"!

So, I began work, sharing an office with his wife in the Institute, and working closely with their superb data processing assistant and all-round administrator, Jackie Marshall. The routine was coffee/tea with the Eysencks and Jackie in the morning, tennis with Hans, Glenn Wilson, and one of the Institute "sparring partners" that Glenn could rustle up (e.g. David Nias, Chris Brewin or Mike Gossop!) at lunchtime, then back to work in the afternoon. We used to play on one of two run down hard-courts that used to be contained within the Maudsley hospital grounds. Hans was a very nice player — very smooth.

On the work front, Hans introduced me to the Hendricksons and their work on the theory of nerve transmission and EEG measures, with the postulate that IQ was directly related to characteristics of

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neural pathway signalling. Hans had some money from somewhere (I never did know where that initial money came from on which I was employed) to replicate the Hendrickson work. My task was to buy the equipment required to replicate and extend the Hendrickson work. The trust Hans put in me was absolute. Also, he wanted action. The fact that I had never bought so much as a light bulb in an experiment setting did not seem to even register with him. In his mind, he had already decided I was capable — and basically told me to get on with it. I think Hans gave people two choices — you either "did the business" or you left. He delegated absolutely — I really did have full responsibility to order anything I thought was necessary (within cost constraints) to achieve the goal set by Hans. I'm not sure whether this is a good or bad feature — for me it was heaven personified! There is nothing quite like knowing that what stands between success or failure is your own abilities and skills — however meagre — and having the chance to succeed under that kind of pressure. I think Hans knew this about me — even though we had never spoken about anything like this. Frankly, this kind of insightful leadership that Hans showed is priceless; Hans would accept nothing but the best, but somehow never seemed to have to convey this other than through a wry smile or a look of mild disappointment when you were relaying some news about a mistake you made etc. Basically, you just knew that you had to aspire to his standards — and so you tried very hard to so do.

Of course, if all I had to keep my eye on was the Hendrickson replication study, then life would have been straightforward. However, Hans was far too productive to confine himself to just one area, and I have a weakness for multi-tasking challenges. As I write this, I get the feeling that he must have known this as well! Anyway, Owen White had been doing a lot of the psychometrics work for Hans and Sybil, especially concerned with helping Sybil complete her cross-cultural factor analyses and factor comparison work. However, when Owen left, Hans suggested I give Sybil a hand — and work with Jackie. I remember Sybil showing me the cabinets full of punched cards, and I remember thinking how I was going to continue on the IQxEEG work as well as figuring out how to proceed with Owen's programs now that the card readers were slowly being phased out of use. Sybil was almost as voracious as Hans in wanting results!

So, I sat down with Jackie and together we figured out where the bottlenecks were in the analyses, and how I could use Jackie as both data processor for Sybil's work, and experiment administrator on Hans' EEG work. Because one of the big bottlenecks was in gaining mainframe computer access — I decided to try and integrate both Sybil and Hans' work — using a minicomputer, with PC-based EEG equipment feeding data directly into the system. Looked good on paper except for the cost of the minicomputer, which had to be usable by Jackie (who had only ever used punched cards), would require all Owen White's software to be re-programmed, and all of Sybil's punched cards to be placed on magnetic tape, readable by our new minicomputer. I took all this to Hans, complete with detailed plans and several well-rehearsed logical arguments. I may as well have taken him a matchbox with a few scribbles on. Basically, his attitude was "if that's what you think we need — that's what we'll get — now let's get onto that court". It was, and we did. No committees, no agonising, no hand-wringing, no pretence at caution as a way of avoiding difficult decisions — just firm leadership, insightful trust, and purposeful decision-making. Excellent.

So, having transformed the cross-cultural work and completed our first EEG study — Hans was hungry for more. Jensen had completed his initial RT x IQ studies, prompting Hans to want to replicate and extend this work. Further, our own work on the Hendrickson paradigm was

unable to replicate the initial work. So, Hans negotiated a rather large sum of money from somewhere; more equipment was purchased, we designed a new RT box (from Jensen's original specifications), and yet more programming was undertaken. Hans was having real fun throughout this period — he had a PhD student (Warwick Frearson) who was working full-time on the Odd-Man-Out and Choice reaction Time paradigms. He had me working flat out on the EEG paradigms and some chronometric task correlates of IQ, and Jackie was working both as experiment administrator and analyst for Sybil Eysenck's cross-cultural work. On top of this, he had become interested in the cross-cultural aspect of Reaction time in children and obtained money to implement a series of studies using children from diverse countries. So, more equipment design, this time a completely automated Choice reaction Time and Odd-Man-Out experiment task — run from a floppy disc — with data stored on other floppy discs. An entire PC, screen, and restricted keypad was embedded in a grey metal box. A reaction time button box connected into it, and off the package went around the world. A few months later and 10 or so floppy discs would arrive containing the data — finally the box would follow! And still the tennis — always the tennis — I could be in the middle of a complex program or analysis, or debugging hardware, or trying to fix problems with Jackie, Sybil, or Warwick — but Hans' tennis racket would insert itself into any situation — and that was that! These really were magnificent days to be doing research!

So, one would have thought Hans' appetite for research was satiated with all this diverse activity. As if! Ian Deary's work on Inspection time suddenly looked hot. Hans' radar picked this up and in one of our chats one day after tennis — Hans enquired mildly if we might be able to do something on this. By now, after about 5 years with Hans, I was "Eysenck-ready" — that is, I knew the mildest enquiry from either Hans or Sybil was likely to turn within 24 hours into — "let's do it". And so it came to be — whilst simultaneously coping with our ongoing projects, I set about designing the inspection time equipment with Bill Withers (an electronic equipment designer and builder), who was also a family friend of Hans and Sybil. The software algorithm also had to be generated — and so was born the BRAT algorithm — and the first of our series of 5 IT experiments.

By this time, Hans's research and personnel were slowly taking over almost one-third of the Psychology Department's room-space! I forgot to mention; he retired in 1983. The reason I forgot is that there was actually no difference to our working whether he was retired or not — nothing actually changed except that his office was now located 300 yards away from the Institute instead of in the department. In fact, if anything, I would wager we were just increasing the work-rate year on year! In 1987, we had finished most of our initial replication and extension work on the EEG paradigms, and main chronometric paradigms. Our PRIME minicomputer was limiting our research capability, our PCs were old, and we were discussing a different kind of bio-signal research that would require processing power beyond the PC systems we were currently using. Hans was formulating the nerve transmission variability hypothesis as a causal factor of psychometric IQ — we decided to test this directly using a peripheral nerve action potential as an evoked response. We designed an experiment that we thought would provide a direct test of this but it required hardware that was at the cutting edge (in those days) of technology. I remember sitting with Hans in the canteen of the Institute, discussing all this and listening as in a dream as my costings were quickly agreed and the business done by Hans. Again, no committees, no long-drawn out agonising, no second thoughts, no preliminary reports or justifications — and no hint of doubt from Hans in my ability to produce the goods. Later that night, came the realisation of

just how much Hans must have trusted me. This was such a key feature of Hans — massive trust in the ability of others whom he judged capable.

The consequence of the commitment to this project was that we physically could not fit all the equipment into the Institute room-space. We had made an initial purchase of a Masscomp real-time Unix computer, complete with 1 MHz sampling on 24 acquisition channels. Our first experiment with this system was the median nerve conduction variability study. But, in order to purchase all the required equipment, we were going to have to move. So, in 1989, Hans negotiated with the Institute administration to take over Jeffrey Gray's old animal lab-space at the Bethlem Royal Hospital. He and Sybil would also take an office in this suite, and so the Bio-signal Lab was born. This was great fun — we had room-space beyond the dreams of avarice — along with a central area in which we were able to test groups of individuals, 20 at a time. However, what was even better I'm sure for Hans was that there were four tennis courts at the Bethlem — in a nice garden area — within walking distance of the lab. Heady days indeed! During the four short years 1989 through to 1993, we were able to assemble one of the finest electrophysiological labs in the UK, and implement a series of experiments whose results really made a difference in the area in which we were working. Although Hans did not get involved with the technical features of what we were doing, he was there, every step of the way in examining how we were achieving solutions, the kinds of problems we were encountering, digesting results, and thinking about the consequences to theory and his own ongoing writing and thinking. Sybil's work with Hans on the cross-cultural examination of the EPQ also burgeoned at this time — the Masscomp system was continually processing data overnight as well as acquiring data during the day!

Working with Hans (and Sybil) on a daily basis for the best part of 11 years was an adventure. There really is no other word for it. Many have commented on the extraordinary impact Hans had on other researchers and practitioners, not only could he inspire, enthuse, and motivate others, he could also provide leadership with what looked like effortless ease. He could also inflame others to the point of apoplexy by his impassive style of argument. In fact, arguing with Hans was like arguing with a logic machine with a large working memory and huge long-term storage capacity. But how he loved debate and challenge! I kept my head down for the first couple of years with Hans, but as usual, my own love of debate and challenge got the better of me and so I would begin to slowly probe some of Hans' work and thinking. Usually I was soon shown the error of my thinking rather smartly — but I was able to learn. And this again was such a marvellous feature of Hans, regardless of where he was in his own work, he would always take time out to explain a concept or piece of evidence for you. Often, he would bring me photocopies of the articles he had been mentioning. I know from speaking with many others who also interacted with Hans that this characteristic of offering help and advice was global. I think the one thing that always stood out with Hans, almost irrespective of which domain of psychology within which he was working at the time, was that he could always see the big picture. That is, his grasp of theory was such that many diverse results in different domains were construed as part of some integrated picture in his own representational system. Thus, he was able to suggest propositions and provide insights that might sometimes have eluded others because of this global theoretical view. This, allied to his fundamental principles of what constituted scientific investigation, laid the foundation for the enormous impact and stimulus that he provided to so many for such a long time.

Although generally calm and assured, one source of irritation to Hans was being kept waiting (usually by me) for tennis! I'd often get dragged out by Glenn or Hans — right in the middle of

some work. I remember sometimes wondering (whilst turning the air blue) whether I should give up any pretence at work and just settle for tennis partner as my occupation! However, truth be told — we were as bad as one another about our love for the game. When I went to the second ISSID conference in Toronto — I was "requested" to bring my tennis stuff — and there we were, mid-conference, playing in almost 90% humidity in 90°F heat. The trouble was, I was as worn out as Hans — and he was nearing 70, almost twice my age.

Maybe some of the above surprises some readers. But I worked closely with Hans on a day-to-day basis for many years. There were times when we had major arguments and disagreements — and I once or twice gave notice to quit. However, Hans' good grace, his wonderful sense of humour, and my slow realisation of how to manage my own more explosive energies ensured that we would continue our working relationship! The mark of a great man is that not once in my time with him did he ever assume the absurd posturing of so many academics who aspire to greatness, but never really succeed. I am sure that all those who worked with Hans and knew him will recognise what I am saying, even from our own vastly differing contextual perspectives.

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