Personality and Individual Differences 53 (2012) 79–84

Contents lists available at ScienceDirect

Personality and Individual Differences

journal homepage: www.elsevier.com/locate/paid

A conversation with Richard Lynn

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ARTICLE INFO

Article history:
Received 3 February 2011
Received in revised form 24 February 2011
Accepted 28 February 2011
Available online 9 April 2011

Keywords:
Evolution
Personality
Intelligence
Race
Eugenics
Dysgenics
Cross-national differences
Sex differences

ABSTRACT

This special issue - *Evolution of race and sex differences in intelligence and personality: Tribute to Richard Lynn at eighty* – testifies to his many research interests over time, where Richard often pioneered. To mention a few examples, he clarified the confusion about the existence of an average sex difference in IQ, by demonstrating that children below 15 do not show the later adult sex difference. More importantly, Richard was the first to establish average IQs for all countries with more than 40.000 people, and then, with Tatu Vanhanen, illustrated the impressive predictive power of these IQs for understanding essential aspects of a nation's economical and social infrastructure. Richard was also the first to realise that classical eugenic measures do not suffices to avert serious consequences of dysgenics and to suggest that modern reproductive technologies may entail better eugenic potentials.

The present conversation provides sufficient details to get a glimpse of the person behind these pioneering efforts this and of his courage. As a prominent member of the London School of Differential Psychology he paints broad consequential strokes on our deeper understanding of human nature and what makes civilizations rise and fall. The behavioural sciences need extraordinary people like him.

HN: Let us begin with your roots. Where are they?

RL: They are all from the east of England. My father's family are Viking stock from North Yorkshire and were small trade people until my father obtained a scholarship to King's College, London. My mother's family are from the southeast and are Saxon stock from the North plain of Germany.

HN: And your childhood: it is often said that our early years are the formative period of our lives. Were yours particularly favourable for future achievement?

RL: Not at all. I was born to a single mother of quite average intelligence, and it has typically been found that children born and brought up in these circumstances are disadvantaged. However, I do not subscribe to this conventional view. I believe the genes we inherit are much more important determinants of our life than our early years.

HN: So were your genes particularly favourable?

RL: They were certainly better than my environment. My father was Sydney Cross Harland and was one of the leading plant geneticists of the 1920s – 1940s. His specialism was cotton, on which he wrote the standard text *The Genetics of Cotton*, and for which he was elected a Fellow of the Royal Society. He was a friend of most of the big names in genetics of his day, including Ronald Fisher, J.B.S. Haldane, and Julian Huxley. He died in 1981. His obituary appeared in *The Times* on November 18 of that year, and concluded "his distinguished career as an applied botanist was marked by a remarkable blend of the agricultural and the academic; for although he made outstanding contributions to the improvement of tropical crops, most notably cotton, his work also had a profound influence on evolutionary theory and the understanding of gene complexes".

HN: In addition to transmitting half his genes, did your father have a significant environmental influence on you during your childhood and adolescence?

RL: No. My parents split up when I was quite young. I did not see anything of my father during my childhood and adolescence because in my early childhood he was working in Trinidad as Director of the Imperial Cotton Research Institute. He was sacked from this position in 1937. My father had an aptitude for annoying people in authority, which I seem to have inherited. Fortunately, he had a marketable skill as a plant breeder and secured a position in Peru as Director of the Institute of Genetics, with the task of reviving cotton which had been attacked by a virus. I did not meet my father until 1949, when he returned to Britain as Professor of Genetics at the University of Manchester.

HN: Did you see much of him and did he influence you from this time onwards?

RL: We met about once a year. I have certainly been influenced by my father's ideas, especially his conviction that our lives are much influenced by our genes, and also the importance he

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doi:10.1016/j.paid.2011.02.033

attached to eugenics. He was one of the signatories of *The Geneticists' Manifesto*, drawn up in 1939 by Hermann Muller (1939), which posed the question "How could the world's population be improved genetically?" My father has also served as a role model and has given me the confidence to advance theories that have sometimes been controversial.

HN: But you only received half your genes from your father. You received the other half from your mother and you said that she had quite average intelligence.

RL: Yes, but her father graduated in Botany as the top student of his year at Imperial College and entered the agricultural service of the British Colonial Office, whose task was to breed improved crops in the extensive British colonies. He ended up as Director of Agriculture in Trinidad, and it was in the small British community of botanical scientists in Port of Spain that my mother and father met in the late 1920s. However, unlike my father who was a workaholic and spent his evenings poring over his data on cross-bred strains and writing them up for journals, my grandfather was quite lazy and preferred to play bridge in his club. I seem to have inherited the workaholic gene from my father.

HN: Tell me now about your childhood and adolescence.

RL: I was born in February 1930 and brought up in Bristol. I went to the Bristol Grammar School, but although my family had all been scientists, I did not find school science interesting. The subject I liked best was history. At the end of my school career I won a scholarship to the University of Cambridge, but I did not go up straight away. At this time all 18 year olds were conscripted into the armed services and in July, 1949, I received my call up papers requiring me to report for military service. It was not a future to which I particularly looked forward. Remarkably, the army decided I would make a good officer and I was duly commissioned second lieutenant. I was put in charge of the training of new conscripts. One of the things I had to do was to teach them how to use a rifle. I had never found any difficulty in this, but I was surprised to find that the new conscripts found this very hard. Generally they failed to hit the target at all. I used to give them a demonstration of how it was done, and the sergeant would bring the target and show it to them with five neat little holes in the bull's eye. They would gather round with exclamations of "Cor, blimey, look at the officer's". I realised later that this apparently simple task must be *g* loaded.

HM: So then you went up to Cambridge. How did you like Psychology there?

RL: Not much. When I started, the Professor was Sir Frederic Bartlett. He was already renowned for his books *Psychology and Primitive Culture* (1923) and *Remembering* (1932). I dutifully read these books and could not find anything much of interest in either of them. Apart from Bartlett, information theory was the dominant research paradigm. The theory was taken from communication engineers who used it to analyse the transmission of information, as for instance along a telephone line. The Cambridge people applied this model to explain the transmission of information through the nervous system. The two leaders of this group were William Hick, who published his famous paper *On the rate of gain of information* in 1953, and Donald Broadbent. I came to know Broadbent quite well and we remained on friendly terms up to his death. However, we did not have much of a meeting of minds. His caste of mind was for developing micro-theories phenomena, whereas I have always preferred broad brush macro-theories.

HN: I think Bartlett must have been quite elderly when you were a student, so you did not have to endure him for that long?

RL: Yes, in 1952 Oliver Zangwill was appointed to the professorship. I looked forward to this new broom and eagerly read his book *An Introduction to Modern Psychology* that had been published in 1950. I was not impressed by this slim volume. It ran to only 220 pages and about 60,000 words and the very idea that it was possible to provide an adequate account of psychology in such a short book seemed absurd. What was the point, I wondered, of writing such a book? I found to my dismay that Zangwill had an uncritical acceptance of psychoanalysis and even wrote that "as a result of Freud's researches, psychology today differs from psychology of fifty years ago in a manner so fundamental as to justify the comparison with biology before and after Darwin". I thought that regarding Freud as comparable in stature and achievement to Darwin was preposterous.
My chief interest became the work on intelligence done at University College, London, developed by Charles Spearman, Cyril Burt and Raymond Cattell, and extended to personality by Cattell and Hans Eysenck. I thought this was much more interesting than the experimental psychology that was being done at Cambridge. I took the final exams in 1953 and did my best to conceal the antipathy I had developed for Cambridge experimental psychology. Apparently I succeeded as I was awarded the *Passingham Prize*, which is given annually for the best psychology student of the year. On the basis of this I was awarded a three year research studentship to work for a Ph.D. I decided to examine the relation between anxiety, intelligence and educational attainment in school children. I completed my Ph.D in the spring of 1956 and was disconcerted to be told by Zangwill that he had appointed Sir Cyril Burt as my external examiner and himself as the internal. I was a bit alarmed at having Burt as my external examiner because he had recently failed two Ph.D. students from Cambridge. However, the viva went well and he passed my thesis.

HN: So then you needed a job.

RL: Yes, and I obtained a lectureship at the University of Exeter. I was now to enter the wilderness years and did not succeed in doing anything that I considered significant for the next twelve years. In 1959 I published a paper *Environmental conditions affecting intelligence*, in which I said that it was now established that genetic factors are the major determinant of intelligence, but that environmental factors are also involved. I proposed that these consisted of the quality and quantity of cognitive stimulation from others in the family. I suggested that this explained the tendency for only children to have the highest IQs, and for IQs to decline with increasing family size, and also that eldest and youngest children have higher average IQs than those in the middle of the family. I sent the paper to Sir Cyril Burt, who replied with a friendly letter saying that he agreed with me. After this, I corresponded with Sir Cyril from time to time and I always found him very friendly and helpful.

HN: Your theory of the quality and quantity of cognitive stimulation from others in the family as the environmental determinant of intelligence sounds like the so-called *Zajonc* effect.

RL: Yes, Zajonc later formulated a very similar theory and managed to get his name attached to it. However, I do not find this annoying because I now think that Joseph Rogers, Boruch, Stoms, and DeMoya (1991) has disproved the theory.

HN: What did you do next?

RL: I fell under the spell of Hans Eysenck's theory that he published in 1957 in his book *The Dynamics of Anxiety and Hysteria*.