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Book Review

# *The Gene Illusion: Genetic Research in Psychiatry and Psychology Under the Microscope* by Jay Joseph. PCCS Books, Ross-on-Wye, 2003

Reviewed by Richard Holdsworth

Although the debate about the relative weight of genetic and environmental factors in the ontogeny of human behaviour has been pursued at a number of different levels, ranging from the molecular to the economic and the political, and although Jay Joseph - to judge from his own statements - would be willing to engage in it on all of them, the interest and value of his book *The gene illusion* lie in its specific character as a detailed methodological critique of certain studies in quantitative behavioural genetics.

Joseph considers both research into the normal range of behaviour and research into the aetiology of schizophrenia. With respect to the normal range, he devotes one of his chapters to the method of research that compares reared-together identical (monozygotic, MZ) and reared-together same-sex fraternal (dizygotic, DZ) twins (Chapter 3), and another chapter to the method that compares data from reared-apart twins with data from reared-together twins (Chapter 4). Devoting two chapters to the genetics of schizophrenia gives Joseph an opportunity to present, in Chapter 6, an overview of twin studies that have sought to establish and compare pairwise concordance rates in identical twins and in same-sex fraternals, and then, in Chapter 7, to develop a vigorous critique of schizophrenia adoption studies. The issues of criminality and IQ are dealt with in Chapters 8 and 9 respectively, and there is a chapter on molecular genetic research in psychiatry and psychology (9). There is also a useful critique of the fallacious use of the concept of heritability in Chapter 5, but - with the exception of one more topic that needs to be mentioned - the meat of the book is in the major attack on the methodologies employed in a number of widely cited twin and adoption studies. Here the reader will find critical analyses of studies by Heston, Kety, Loehlin and Nichols, Scarr and Carter-Saltzmann, Bouchard, Tienari, Gottesman as well as other authors.

Joseph's other main theme is history. In Chapter 2, "Twin Research: Misunderstanding twins from Galton to the 21st century", his thesis is that the history of twin research should be understood in the context of the history of eugenic ideas and practices. He also considers that modern twin researchers have been reluctant "to provide details on the more unsavoury

aspects of this history", by which he means "the origin of twin research as a tool of eugenics, 'racial hygiene,' and Nazism" (p.2). He devotes attention to the work and influence in the 1930s of Ernst Rüdin, director of the Kaiser-Wilhelm Institute of Psychiatry in Munich, and of Otmar von Verschuer. Joseph states that Josef Mengele studied under von Verschuer in Frankfurt. Then a point arises that requires clarification. Joseph goes on to quote from a contemporary (1937) American text which mentions that, at that time, von Verschuer was the head of "a large group of students of twinning at the Kaiser-Wilhelm Institut für Anthropologie...".[1] Joseph states that "It is likely that Mengele was one of these students". The necessary clarification is that the institute referred to here was in fact the Kaiser-Wilhelm Institut für Anthropologie, menschliche Erblehre and Eugenik in Berlin-Dahlem, which von Verschuer headed after he had been director of the Institut für Erbbiologie und Rassenhygiene in Frankfurt am Main from 1935 to 1942. Further light on this episode has been shed by the Max Planck Society's research programme on the "Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus", and a brief summary of von Verschuer's movements and his scientific relations with Mengele is given in its press release of 2001.[2]

For Joseph, the case of Ernst Rüdin is salient because of the link he draws between Rüdin's role in the founding and promotion of psychiatric genetics, including the use of twin studies, and his affinity for the climate and policies that, after the advent to power of Hitler and the Nazis on 30 January 1933, yielded the German "Law for the Avoidance of Genetically Diseased Offspring" of 14 July of the same year. Indeed, as early as 1905, Rüdin had been a co-founder - with Alfred Ploetz and Anastasius Nordenholz - of the German Society for Racial Hygiene (*Gesellschaft für Rassenhygiene*). Joseph notes that researchers from other countries came to study in Munich under Rüdin and his associates in the pre-war years, and that these workers often went on "to have long and influential careers in psychiatry" in other countries. He cites as examples Franz Kallmann (who, partly Jewish, emigrated to the USA), Eliot Slater (UK), Eric Strömgren (Denmark) and Erik Essen-Möller (Sweden). However, eugenic ideas had already developed in other countries than Germany. Joseph cites the law passed in Denmark in 1925 "forbidding 'insane and highly feeble-minded persons' from marrying" and the Danish law of 1929 that "legalized sterilization in cases of mental retardation or mental disorder". He states that eugenic sterilisation was performed in Sweden until the mid-1970s and in Japan from 1948 until the 1990s (p.300). He also refers to the fact that laws for compulsory sterilisation were adopted by numerous states of the USA.

Joseph's text was presumably concluded before the public "apology for Oregon's forced sterilisation of institutionalized patients" was issued on 2 December 2002 by the then Governor of Oregon, John Kitzhaber. The statement by Kitzhaber, who is a doctor, said that Oregon was one of 33 states that enacted laws to provide forced sterilization.[3] The Board of Social Protection in Oregon (earlier called "Board of Eugenics") was abolished in 1983. This apology is one of a number of recent developments in American states and European countries in which some form of accountability has been accepted for past eugenic policies and measures. Another example is legislation passed in Sweden in 1999 on compensation to persons affected by the Swedish sterilisation law of 18 May 1934 that was in force from 1935 to 1975.[4]

What Joseph is concerned about is sources of bias. He argues in Chapter 3 that twin research based on the comparison of the behaviour of identical twins with that of fraternal twins inevitably makes the assumption of an 'equal environment'. This is the assumption that environmental conditions are similar for both kinds of twins: for instance, in that they receive approximately even-handed treatment by their

parents. Joseph, however, would be unwilling to allow that this criterion is ever satisfied. His case is that "identical twins experience more similar environments than fraternals", but he says that as this point has gradually come to be acknowledged by twin researchers, they have tended to counter it with the demand that critics of twin studies should concentrate on 'trait-relevant' departures from equality. Moreover, he cites twin researchers such as Edith Zurbin-Rüdin as arguing that the more similar environment of MZ twins is a product of their genetic make-up. She wrote (1972):

If MZ twins create a similar environment through their greater similarity, they do so because of the greater inherited similarity in their appearance and response modes. Thus, in a roundabout way, we still come back to the importance of heredity.[5]

Readers with sceptical reflexes will wonder if 'roundabout' ought not to have been replaced here by 'circular'. In any event, Joseph here reminds the reader that the similarity in the physical appearance of identical twins is not an inherited characteristic, but the result of the splitting of a fertilised egg. He says that what has to be considered first is the similarity of the twins to each other, rather than to their parents. One could imagine that both are important, but it is indeed an interesting point that becoming one of a pair of identical twins is something that happens to an individual after the genotype is in place. One could add that being born having an identical twin sibling is an experience that the environment delivers. The environment delivers it to members of MZ twin pairs, and to nobody else.

Concerning parent-twin interaction, the issue has been whether twins "create their own environments" by being so similar that their parents have to give them a 'similar' reaction, or whether parents actually approach MZ twins in a way different from that which they would manifest towards DZ twins. Here Joseph gives critical consideration to K. S. Kendler. He acknowledges that by 1994 Kendler had come to agree that there could be this difference in approach, but he quotes from a work by Kendler and others of that year a version of the trait-relevance argument, saying that differential parental treatment would only invalidate twin studies of psychiatric disorders if the type of parental treatment concerned influenced the risk for the psychiatric disorders under examination. Joseph takes issue with this.

Joseph's Chapter 4, 'Genetic Studies of Twins Reared Apart: A critical review', brings us to the heart of his critique of behavioural genetics as concerns behaviour in the normal range. In particular, he sets his sights on MISTRA - the Minnesota Study of Twins Reared Apart. MISTRA was launched in 1979 in the Psychology Department at the University of Minnesota and is still yielding research findings, including a paper by Segal, Hershberger and Arad that appeared in *Evolutionary Psychology* in March 2003. In that article the authors described MISTRA as "a longitudinal study of twins separated at birth, reared in separate homes and reunited as adults".[6] The epigraph to the paper is a quotation from an article in the New York Times Magazine of 9 December 1979 in which a man called Jim Springer describes his emotions on meeting his twin brother for the first time on reunion.

Joseph announces his intention of looking at 'twins reared apart' studies, first at the level of the 'folklore' and then as 'science'. The term 'folklore' sounds disparaging, but one recognises that there is a kind of narrative about separated twins and their being reunited which appeals to many people - most of us, perhaps - at a level that has no aspiration to be scientific. This one takes its place alongside other narratives of life-events - courtship, say, marriage, illness or death - that may validly be communicated in various different types of discourse. In this part of the chapter Joseph introduces four of the twin pairs from MISTRA: Jim Springer and his long-lost brother Jim Lewis, Oskar Stöhr and Jack Yufe, Daphne and Barbara ("the Giggle Twins"), and Jerry Levey and Mark

Newman, New Jersey firemen.

The question is how amazed we ought to be at the similarities in life-history that some pairs of identical twins show. Joseph criticises the use of a photograph of Jerry Levey and Mark Newman by Nancy Segal in her book *Entwined lives* (1999). As Joseph relates it, the picture shows each man holding a can of beer, and each in the same unusual grip, with the little finger curled under the can: a curious feature that Segal draws attention to in her text. Joseph questions the choice of this photo in place of others available in which the curled finger phenomenon was absent. He goes on to offer seven apparently unexplored, non-genetic explanations for the phenomenon, ranging from cultural influences to the simple possibility that the photographer asked the brothers to hold their cans that way. Another consideration that Joseph stresses is the "influence of the cohort". Coincidences of behaviour that may seem bizarre if we are told they have been displayed by identical twins can appear merely banal if we are just told they come from two people in the same age-group.

Turning to his scientific critique of 'twins-reared-apart' studies, Joseph is concerned by studies that have purported to exemplify this research paradigm but have been vitiated by departures from some of its crucial principles of design, notably the requirement that the twins should indeed have been reared apart. He cites 15 cases of twins that occur in the study by Shields (1962) in each of which, he complains, there is a problem of late separation, contact during the period of supposed 'separation', or both. Joseph is able to make his detailed critique of the 15 Shields pairs because, he says, Shields followed the practice, now less common, of publishing a large quantity of case history material to accompany the report of the study. By contrast, Joseph complains about MISTRA's failure "to make life history and test score data available for independent analysis". He says the MISTRA researchers have claimed to be prevented by legal and ethical constraints from releasing life history information. Yet, he says, during the 1980s Bouchard and his associates in MISTRA supplied data on some of their twins to journalists, leading to the publication of some of the stories "in leading U.S. magazines and newspapers".

This did not only occur in the US media. This reviewer has a copy of the Observer Magazine for 15 December 1991 in which there was an article on the MISTRA study by the science journalist Robin McKie, based on an interview with the Director of MISTRA, Thomas Bouchard. [7] This article appears to mention by name 14 pairs of twins researched under MISTRA, of whom 12 pairs are shown in the 12 photographs that accompany the article. In each photo one pair of twins is shown. There are eight colour photos, which appear to have been specially taken for the article by the photographer Abe Frajndlich, and each of these is a carefully posed double portrait in which the two twins are dressed in identical clothes. The four black and white photographs are more naturalistic, and they are credited to other photographers or photo agencies. The twins in the monochrome pictures are precisely the four pairs that Joseph had heard about and named in his book. Was there a MISTRA press pack?

In Joseph's chapters on the genetics of schizophrenia, where he criticises twin and adoption research, he raises a series of problems arising from the limited number of persons suitable to be the subjects of research. Schizophrenia has a lifetime population expectancy rate of not quite one per cent. By the criterion that we are interested in for the time being, this means that the number of sufferers from schizophrenia is a small proportion of the population. Now we need to look at the method of research. Leonard Heston, in 1966, identified 47 adopted-away offspring of institutionalised women diagnosed with schizophrenia. Briefly, the aim was to compare these experimental group adoptees with a control group of another 50 adoptees whose parents had no record of admission to a state psychiatric hospital in the

American state where the study was conducted, which was Oregon. It is easy to see why the numbers of people available to be studied by such a method is - fortunately - never likely to be large. The mother must have been a schizophrenic, and the child must have been adopted. Moreover, reliable information about them must exist in some records somewhere, and the information must be accessible for purposes of research. The researcher may encounter the problem of doing statistics with a small sample. The statistical significance of findings may turn on one or two individual cases. In these circumstances, researchers may be anxious to secure a sample that is as large as reasonably possible. They may be correspondingly anxious not to lose any cases. These anxieties can place the method under stress.

There is, for example, the problem of the diagnosis of schizophrenia, more particularly the range of disorders to be counted under the heading 'schizophrenia'. A recognised diagnostic standard is the Diagnostic and Statistical Manual of Mental Disorders published by the American Psychiatric Association, of which the fourth edition (DSM-IV) came out in 1994. Joseph considers that researchers ought to announce their reference standard, and should also provide enough life-history material on the adoptees in the experimental group for independent reviewers to check the validity of the diagnosis. He complains that Heston neither specified his standard nor provided the supporting material. In one case where Heston did provide a case history, Heston argues, the data suggests that the man concerned might have been diagnosed (retrospectively, in terms of DSM-IV) as having either schizophreniform disorder or brief psychotic disorder, rather than schizophrenia. This change of status for one case would, he says, "have been enough to render the experimental-control comparison statistically non-significant". This example of Joseph's criticisms will have to stand for many.

It is necessary to say something about his argument concerning selective placement bias. Here Joseph's case is that a number of studies involving the adopted-away children of schizophrenic parents were undertaken at times and in places where there was a widespread social belief in the hereditary 'taint' of schizophrenia, and that adoption agencies and adopting couples would have been biased against children at risk of carrying that taint. The result would be that the children of schizophrenic mothers tended to be placed in less favourable adoptive homes - some of them, perhaps, so unfavourable as to furnish the conditions for an environmental aetiology of schizophrenia. In this way the logic of the research study would be confounded. This argument of Joseph's ties in with his historical analysis, since the studies that he criticises were carried out in places which happened to provide a favourable climate of thought for the propagation of eugenic ideas during relevant decades of the twentieth century. He would argue that the very existence in these states or countries of the institutions and registers that make twin research feasible are the historical expression of a eugenic intellectual atmosphere from the influence of which such research can never escape.

Joseph's concentration on quantitative behavioural genetics means that his treatment of molecular genetics is comparatively thin. The chapter he devotes to the topic is brief, which gives its polemical title - "Molecular genetic research in psychiatry and psychology: An exercise in futility?" - a gratuitous air that does not strengthen the author's case. At the end of the book, Joseph states baldly that, in the end, molecular genetic research in psychiatry, as well as "the search for behavioral and IQ genes", "will prove to be a gigantic waste of time, energy, and money". It is inconsistent that, at the end of a book in which the author has demanded so much analytical and methodological rigour from other workers, he should allow himself the latitude to indulge in this indiscriminate, a priori dismissiveness.

Nevertheless, *The gene illusion* is a forceful presentation of the criticisms that have been

made of quantitative behavioural genetics. Some of these Joseph has published before in journal articles and some he attributes to predecessors such as Don D. Jackson, Richard Lewontin, Steven Rose, Leon J. Kamin and Susan Farber. The book also makes a historical and sociological argument that claims attention. Joseph is a clinical psychologist who addresses his readers with a plea for a more humane vision of human psychological distress than he has found in the literature of behavioural genetics, as well as of the social conditions that cause it.

Research into the molecular genetics of behaviour and behavioural genomics may certainly be expected to generate hypotheses about the genetic origin of human behavioural traits, but these will not escape critical evaluation. There must be few people left who believe the outcome will be settled by arm-wrestling between twin researchers and their critics. Another view is that the new understandings that we need of the origins of human behaviour can only emerge out of interactive processes of research, communication, comparison and criticism involving many different disciplines that aspire to enrich this field of study.

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**Notes**

1. The words cited are from a quotation in Joseph (pp.32-33) from Newman, H.H., Freeman, F.N., and Holzinger (1937): Twins: A study of heredity and environment, The University of Chicago Press, Chicago, p.19.
2. 'Erinnerung und Ausblendung. Ein kritischer blick in den Briefwechsel Adolf Butenandts (MPG-Präsident 1960-1972)', Pressemitteilung vom 17. mai 2001, Max-Planck Gesellschaft, Forschungsprogramm "Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus", Berlin. Available online at http://www.mpiwg-berlin.mpg.de/KWG/Presse170501.htm.
3. 'Proclamation of Human Rights Day, and apology for Oregon's forced sterilization of institutionalized patients', Governor John Kitzhaber, Salem, Oregon, 2 December 2002. Available online at http://arcweb.sos.state.or.us/governors/Kitzhaber/web_pages/governor/speeches/s021202.htm.
4. The Swedish situation and the action on compensation are described in a Swedish Government paper of 2000 that was submitted to the Riksdag (parliament) on 15 March 2001. It is Regeringens skrivelse 2000/01:73, 'Redegörelse för steriliseringsfrågan i Sverige åren 1935-1975 och regeringens åtgärder'. Available online at http://www.social.regeringen.se/propositionermm/skrivelser/pdf/s200001_73.pdf .
5. Edith Zurbin-Rüdin (1972): 'Genetic research and the theory of schizophrenia', International Journal of Mental Health, 1, 42-62. Cited Joseph, p.63.
6. Nancy L. Segal, Scott L. Hershberger and Sarah Arad: 'Meeting one's twin: Perceived social closeness and familiarity', Evolutionary Psychology 1: 70-95, 31 March 2003.
7. Robin McKie: 'Same difference', Observer Magazine, The Observer, London, 15 December 1991. Photographs by Abe Frajndlich.