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London Review of Books

Consider Jack and Oskar

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Born Together – Reared Apart: The Landmark Minnesota Twin Study by Nancy Segal
Harvard, 410 pp, £39.95, June 2012, ISBN 978 0 674 05546 9

In a tongue-in-cheek editorial in the February 1927 issue of the *Journal of Educational Research*, the psychologist Guy Whipple announced that 'the age-old perplexity of heredity has been banished; the old riddle of nature versus nurture has been solved.' For the previous half-century, psychologists, geneticists, pedagogists and eugenicists had been trying to determine how personality attributes such as intelligence, manual skill and temperament were passed from parents to their offspring. Now, Whipple deadpanned, a rising movement in psychology had invalidated this whole line of research, insisting instead that 'there are no inherited traits, characters, talents; that every normal person is born with the capacity to learn any behaviour that man has ever known, and that a body organisation [i.e. psychological constitution] can be built up in the first five years of life which makes it impossible for the person to kill or steal.' This may be a tall order to place on teachers, Whipple continued, but too bad: anyone who wanted to keep up with the latest scientific thinking must embrace 'nurture' and 'kiss Nature goodbye'.

For the next half-century, American psychologists did, overwhelmingly, come to base their work on the principle that it is environment – society, culture, upbringing, the random events that make up day to day life – and not heredity which has the greatest sway over the differences between individuals. Nancy Segal's *Born Together – Reared Apart* tells the story of one effort to return to hereditarian accounts: the Minnesota Study of Twins Reared Apart, or Mistra, which between 1979 and 1999 examined 137 pairs of identical and fraternal (non-identical) twins separated in early childhood and raised in different households, assessing such traits as intelligence, conservatism, personal dynamism, creativity, religiousness and sexuality. After a series of more than four thousand tests, as well as observations and interviews with the twin pairs, Mistra scientists proposed that a huge number of personality traits previously thought to be influenced by environment and upbringing – such things as career choice, reading habits, food preferences, when we have sex and whom we tell about it – were, in fact, driven by genes. 'Behaviour[al] geneticists,'

Segal declares, 'have shown that virtually all measured traits display genetic variation.'

Segal spent three years first as a postdoctoral fellow under the study's director, Thomas Bouchard, and then as the assistant director of the Minnesota Center for Twin and Adoption Research. Her book answers critics of Mistra who argue, as Joseph Jay did in the *American Journal of Psychology* in 2001, that 'the studies of separated twins contain serious flaws, and the authors' conclusions are questionable,' and attempts to demonstrate the power of genetics to shed new light on human behaviour. Yet what emerges clearly in the course of the book is less a sense that nature has been vindicated, or that a new age of insight has dawned governed by behavioural genetics, but rather a feeling that the big questions about nature and nurture have gone begging.

In 1876, Francis Galton published 'The History of Twins, as a Criterion of the Relative Powers of Nature and Nurture'. Fascinated by 'mental heredity' but unable reliably to distinguish the attributes his subjects were born with from 'those that were imposed by the circumstances of their after-lives', Galton hit on a novel idea: assemble a sample set of identical twins, ask them about their life histories, physical development, moods and intellectual aptitudes, then discern which attributes seemed most prone to environmental influence and which to heritability. His survey of 35 twin pairs showed – quite contrary to his expectations – that the dispositions of adult twins tended to show remarkable similarities, sometimes in spite of 'very different conditions of life'. This suggested to Galton that mental traits were much more heritable than he had anticipated, and also that the twin method was a viable research tool.

Galton's idea had legs. Throughout the late 19th and early 20th centuries researchers went about assembling case histories of twins and their parents, using new psychometric concepts such as the 'intelligence quotient' or 'IQ', and novel batteries of questions for indexing temperament such as the Woodworth-Mathews Personal Data Sheet and the Pressey Test of the Emotions. In 1924, Curtis Merriman and Hermann Siemens explored the idea of comparing pairs of identical (monozygotic) twins with fraternal (dizygotic) twins, as a means of further clarifying the effects of heritability. In 1937, Horatio Newman, Frank Freeman and Karl Holzinger introduced the idea of using twins raised in different households, in a study of 19 pairs of twins. And three studies by Cyril Burt between 1943 and 1966 put a number on the heritability of IQ (77.1 per cent). In the early decades of twin research the exact mechanism by which human 'germ plasm' (i.e. reproductive material) transmitted such traits had yet to be determined. The prominent American biologist Charles Davenport, for instance, speculated in 1915 that 'factors', which he labelled C and E for 'cheerfulness' and 'excitability', combined in human reproductive material to yield general personalities which he described as 'choleric', 'nervous', 'calm', 'cheerful', 'phlegmatic' or 'melancholic'. But the important point, Davenport emphasised, was that 'the hereditary nature of temperament is demonstrated by the facts of the

personal history of identical twins as given by Galton.' Twin studies were now seen as a vital tool of behaviour study.

Yet the gloss on twin studies faded over the course of time. For one thing, behaviourism – the name given to the school of thought that Whipple had mocked – grew in influence throughout the early 1900s, eclipsing other modes of thinking about human development by the middle of the century. In their belief that all organisms' behaviours could be attributed to systems of stimulus and response (ring bell, dog salivates), behaviourists had little need for germ plasm, cheerfulness factors or heritability. Perhaps more significant, studies of the biological bases of psychological traits came into stark disrepute in the 1930s and 1940s owing to their prominent place in Nazi eugenics programmes. Davenport's work, for instance, was influential in Nazi Germany, and Josef Mengele was fascinated with twins, collecting subjects with the cry, 'Zwillinge, heraustreten' – 'twins, step forward' – on the railway platform at Auschwitz. The memory of Nazi racial science had hardly dimmed when, in 1969, the Berkeley psychologist Arthur Jensen argued in an article in the *Harvard Educational Review* that low IQ scores and poor academic performance among disadvantaged minorities in America was genetic in origin; after all, Jensen explained, twin studies had shown that IQ was largely heritable, so no amount of social intervention could staunch the expression of genes for low intelligence. Disadvantaged minorities were disadvantaged by their very nature. Scholars across the social and natural sciences were outraged, and challenged Jensen on the validity of heritability studies, the statistical methods he used to compare groups, the usefulness of the concept of IQ, and the spuriousness of tying one social construct (race) to another (intelligence) in the name of objective science. The cycle was repeated in 1994 with the publication of Richard Herrnstein and Charles Murray's *The Bell Curve*, which extended Jensen's basic thesis into a 900-page exposition of the genetic bases of social inequality, crime, poverty and unemployment. In the meantime, a re-examination of Cyril Burt's work in 1974 found that he had falsified or at least severely erred in interpreting his twin data – small potatoes compared with scientific racism, but still.

This rather fraught history notwithstanding, Segal presents Mistra as a model of social science. Statistical correlations are its backbone, but it is the stories of the twins, as Segal says, which bring the data to life. Consider Jack and Oskar, the former raised as a Jew in Trinidad, the latter as a Catholic in Nazi Germany, but both possessing a strong inclination to sneeze loudly in crowded elevators, a habit of compulsive washing and a tendency to read books back to front. Or Jim Springer and Jim Lewis, who, although they were raised forty miles apart from each other in Ohio, had both worked in law enforcement, were both hobbyist carpenters, drove Chevrolets, took holidays on the same beach in Florida, smoked Salems and drank Miller Lite; both had been married twice, first to women named Linda then to women named Betty, and had sons with nearly identical

names (James Alen and James Allen). The reunited twins in the study often became friends; in one instance, they became lovers.

These eerie inventories of common traits and interests weren't, according to Mistra researchers, simply coincidences, but evidence of genetic causality free from the obscuring haze of shared social environment. This does not mean, Segal insists, that environment is unimportant. Parenting, education and home environment have a great deal of influence on adult measures of happiness and intelligence, while the 'guidance of parents, teachers and other mentors can significantly affect the career paths that children eventually choose'. Moreover, it must be kept in mind that the concept of 'heritability' – the fundamental measure of twin studies – is a group rather than an individual measurement. To say that intelligence is 77.1 per cent heritable simply means that in a given population, 77.1 per cent of differences in intelligence can be accounted for by genetics. It doesn't mean that in any given individual, 77.1 per cent of observable intelligence is genetic while the other 22.9 per cent is down to upbringing and life events.

Instead of nature v. nurture, Segal promotes Bouchard's notion of nature *via* nurture. The idea is that genes for behaviour express themselves according to the possibilities available in social environments. Children with genes for athletic interest, say, will choose to do sports if sports are available; children with genes for academic interest will seek out books or other forms of intellectual activity as their environment allows. By way of example, Segal points to Oskar, who discovered after being reunited with his twin that he enjoyed the spicy Trinidadian food Jack offered him. In Germany, Segal speculates, Oskar didn't have access to spicy food, so hadn't had the chance to realise his genetic predilection.

Yet it isn't obvious what larger conclusions about the genetic influence on individual behavioural differences can be taken away from this research. The verdict that genes matter, but so does environment, seems obvious. That might, as Segal argues, be because of our increasing acceptance of genetic explanations of behaviour – brought about in no small part by Mistra itself – or it might be the effect of attempting to weigh the contributions of two already overdetermined quantities, genetics and environment, to another overdetermined quantity, general behaviour. Indeed, one comes away from *Born Together – Reared Apart* with the impression that the flexibility of genes as go-to causal mechanisms for almost any particular individual behaviour impeaches rather than reinforces their explanatory power. With a little creativity, a just-so story can link seemingly any behaviour to genes, provided one doesn't have to specify the genes in question, or think too closely about the behaviour. For instance, Segal presents the case of two identical twins, both 'extremely talented in mathematics'. One of them, raised in China with little education, found work as a cashier; the other, raised in the US, 'obtained an advanced science degree'. Segal treats this as evidence of identical genes expressing themselves in the face of different environmental circumstances, but the explanation raises

more questions than it answers. Does one use the same 'quantitative skills' as a cashier and as a scientist? What, precisely, are the cognitive processes involved in 'mathematics'? For that matter, what conditions (meritocratic, bureaucratic, personal etc) affect job selection in China as against the US, and how is the expression of genes for mathematical ability affected by them? From this and other examples of ostensibly genetically driven twin behaviour, one is left with the impression that we wouldn't be much worse off assigning personal differences in behaviour to the levels of cheerfulness factor in germ plasm.

There is further cause for concern. Mistra's inventories encoded a surprising array of complex, highly individuated behaviours by means of standardised, uni-dimensional measures. For instance, fascinating as it is to learn that a vague, complicated and subjective trait such as 'conservatism' is genetic in origin, it's even more interesting to learn that there's a psychological metric – the 'Wilson Patterson Conservatism Scale' – that Mistra researchers used to measure it. But what, precisely, is it that the conservatism scale measures, and why should we take Wilson and Patterson's version of conservatism as the official definition? The psychologist Barrie Stacey wrote in 1978 that the W-P conservatism scale appears to have 'three major components – blimpish religiosity, racialism and a rather prurient sexuality'. These, in his view, 'add up to a greatly constricted view of conservatism'. Whose conservatism is the one reflected in our genes: Wilson and Patterson's? Stacey's? Both? Neither? Segal's circular explanation, that the conservatism scale captured a subject's conservatism, does little to clarify the matter. The same can be said of numerous other Mistra measures, including religiosity, creativity, leisure time activity, intelligence and mathematical skill: these are not natural kinds, but social kinds – social values which we identify by names (like 'conservatism') for convenience in colloquial conversation. Segal dismisses such concerns with a terse note that the 'intelligence tests, personality inventories and most of the interest questionnaires that were administered had been used widely in prior research. The reliability and validity of these instruments had been well established.' Perhaps, but when what's at stake is the cause of human behaviour, and therefore the extent of biological influence on such social issues as education reform, economic inequality, unemployment, crime and sexuality, one could wish for closer scrutiny of the means used to demonstrate that genes 'explain' behaviour.

Segal and her colleagues appear baffled and exasperated that Mistra's findings have met with resistance. Critics of the study, in Segal's view, are 'faultfinders' who 'cling' to the environmental view of behaviour and base their criticism on 'assumptions rather than facts'. Bouchard wondered at the reason for the 'interminable' criticism, given the robustness of his data, and complained in an interview in 2009 that 'academics, like teenagers, sometimes don't have any sense regarding the degree to which they are

conformists.' E.O. Wilson expressed a similar sentiment in a personal letter to Bouchard, remarking that Mistra's work would silence 'all but the diehard critics, who would go down fighting even if you laid out the full nucleotide sequences with mathematically perfect forms of reaction.' 'Science,' Segal concludes, 'rests on data, not on dialogue.'

This, however, gets it exactly wrong. The building blocks of science are data of one form or another, but dialogue is the basis on which science rests; discussion is what gives meaning to the numbers; the tales that we tell about data are what give it substance. Newman, Freeman and Holzinger recognise this in their 1937 monograph when they call their readers' attention to the fact that their general conclusions 'represent a consensus of the views of all three authors arrived at after considerable discussion of divergent interpretations of the data'. Didn't the Mistra scientists ever experience divergent interpretations of their own data? Didn't they ever argue among themselves over what their data meant? If not, on what basis did they conclude that their results made sense? Whatever Bouchard and Segal's protestations, it's stories about data – interpretations of observation, assumptions underlying test design, explanations of statistical correlation – on which the Mistra scientists based their findings of 'genetic' behaviours given that not a single gene was actually examined in the study.

Today, twin studies are falling increasingly out of fashion as a tool for probing the intricacies of human behaviour, replaced by technologies for mapping genomes, measuring gene expression and analysing neurochemical activity. Studies that make use of these point towards a more dynamic model of 'nature' and 'nurture' than previously expected, one in which variations in environment produce variations in gene expression: a feedback loop of nature and nurture rather than an opposition. Yet the issues raised by such studies are as pertinent as ever. The assumptions that we bring to the scientific study of the human are deeply conditioned by our cultural, social and historical situation. Galton was writing at a time when novel ideas about heredity and radical upheavals in the organisation of modern society made sense of the claim that cultural character traits, both good and ill, were subject to universal laws of statistics and biology. More than a century later, new concepts, new technologies and new assumptions about the relationships between biology, society, genes, environment and culture condition our own understanding of human behaviour. It may well be that a century from now, the very notion of a 'nature/nurture debate' will appear as jejune as 19th-century theories of germ plasm and E factor do today. In closing the chapter on twin research, we might end up kissing both nature and nurture goodbye.

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