

David Reiss's *The Relationship Code*: The Book for a 'Wide Audience' We Struggled to Read

Peter Churven, Hugh Crago and Ian Wilson

The authors review *The Relationship Code: Deciphering Genetic and Social Influences on Adolescent Development*.

Hugh Crago

When I first heard about the existence of this book, I was very excited. David Reiss and a parade of co-researchers (including the eminent geneticist Robert Plomin) have laboured for years now in a massive attempt to disentangle nature and nurture. More precisely, Reiss's project has set out to see what contribution genetics makes to problems between parents and children, as opposed to the more obvious influence of the way that parents treat their offspring.

We are all familiar with the puzzling situation that sends so many parents to see therapists in confusion and self-doubt: why is one of my kids always getting into trouble, while the others seem just fine? Why does one brother do well at school, act in prosocial ways, and team up with an appropriate partner when the time comes, while another drops out, gets involved with drugs, and hangs around with highly unsuitable boyfriends/girlfriends, who seem to 'bring out the worst in him'? 'After all, they were both wanted children, we've always loved them, and they've been treated equally. So how come they've turned out so different?' These parents are aware that something complicates or modifies the *shared environment* which they have provided for all their children. Could that 'something' be genetic? Parents often think so ('He takes after his father', 'She's got bad genes'); but professionals, and especially family therapists, have been reluctant to take such a line.

A variety of theories have been advanced to 'explain' the anomaly. Is it birth order? Toman (much referred to by Murray Bowen), and more recently and persuasively, Sulloway, have argued that birth order variables *do* matter, and that firstborns are more likely

to be rule-maintainers — 'conservatives' (in the broad, not necessarily political, sense) and noninnovative leaders. Later-born children are more likely to be rule-avoiders or rule-challengers — 'radicals' (in the broadest sense) and innovators who seek new fields to conquer because their position in the sibling pecking order denies them leadership within the family. All of which is reasonably consistent with Adler's original birth-order theory, in which the 'will to power' originates in competition for parental love.

Yet birth order alone is clearly insufficient to account for the sorts of differential outcomes we have mentioned. While one 'middle' child may turn into a malcontent who acts as if she 'has no place' and acts out in consequence, another adopts the alternative route of becoming the 'family diplomat' and thoroughly prosocial. One 'free-spirited youngest' ends up as a dual-diagnosis client in a rehab facility, another forges a highly successful career as a comedian on TV.

What about another type of nonshared environment — the alliances and 'special relationships' that shape family dynamics, bonding a mother and her eldest son, for example, or a father and his youngest daughter, leading to differential treatment or, more



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subtly, to the child feeling different and 'special' despite the appearance of 'all being equal'? This is the factor that Freud observed when constructing his theory of the Oedipus/Electra Complex. This theory, too, despite its popularity and apparent explanatory power, is insufficient in itself. When there are several sons, or several daughters, what determines how the alliances will run? Not gender alone, since (in my own observation) where one parent has already bonded strongly with the preceding child, it is highly likely that the next child will bond more strongly with the other parent, regardless of the gender of the child. Again, this begins to look curiously like Sulloway's theory of 'spaces' within the sibling subsystem that are perceived to be either 'taken' or 'not taken'.

Into this complex of overlapping, sometimes contradictory, theories comes David Reiss and his research team with their longitudinal study of a large sample of families with adolescent children. Reiss's ambitious research agenda, over many years, has been to track both shared and nonshared factors at both genetic and environmental levels. Having read a review of this book on its first appearance, I was determined that the *ANZJFT* should write about the book, preferably at some length. The notion that genes 'pull' particular kinds of response from the interpersonal environment seemed intuitively right to me. In other words, a child's particular genetic makeup will go some way towards limiting the range of ways that parents and siblings will behave towards her, and think about her.

This is consistent with Thomas and Chess's temperament research, which seemed to show that children *are* different from birth, and that these differences in many cases persist throughout maturation, and even lifelong. A child who is temperamentally high in activity level (restless, an 'explorer', a 'wanderer') and with a high threshold for stimulus, is going to attract a different type of attention and reinforcement (positive or the reverse!) from its caregivers as compared with a sibling who is relatively peaceful, and can find her own amusement without troubling others. A temperamentally shy child (who tends to shrink away from new faces, and waits for the other to build the bridge) will be seen, and responded to, differently from an outgoing, sociable youngster, and so on.

With so much in my own thinking seeming to 'fit' with Reiss's views, I opened the book with some anticipation. Yet I found *The Relationship Code* extremely difficult. It is not that it is not well written: clearly, Reiss, Neiderhiser, Hetherington and Plomin have put considerable effort into trying

to render their hypotheses and their findings as lucidly and reader-friendly as possible. Yet the result is still dense, repetitive and somehow obscure. I put my doubts down initially to my own lack of familiarity with statistics and my difficulties in 'getting my head around' a large, sophisticated research study intended to tease out shared environment from nonshared, intact biological family structures from blended families, biological children from adoptees, and so on. This is a most ambitious piece of research and, of course, writing it up was always going to be complex. Yet somehow the difficulty in extracting the message of the book went deeper than that, and I was greatly relieved when others, whom we had asked to participate in this Refrains debate, had a somewhat similar reaction.

Ian Wilson

Having recently read *Gene-environment Processes in Social Behaviors and Relationships* (see page 55 of this issue), I wondered whether the book by Reiss et al. would have a similar finding. However, I was warned in advance that Hugh and Peter had 'problems' with the book and I was unsure why this was so. I speculated briefly that the results, with their emphasis on genetics and the nonshared environment, would be too much for systems-based therapists. I rejected this quickly as there is not a threat to family therapy, only an explanation for the different outcomes for adolescents seen in the one family.

I approached this book with interest but soon discovered why there was significant dissatisfaction. I expected this book to be a much easier read, but like Hugh and Peter I found it a very difficult process.

The topic of the interplay between genetics and the nonshared environment and its relationship to adolescent development is difficult to express simply and involves some heavy statistical treatment. Reiss et al. describe the background, method and results of one large study to explore this issue. The authors are meticulous in the way they express their messages, so much so that the sentences become convoluted and difficult to dissect. The presentation is slow and methodical.

By contrast the Deater-Deckard and Petrill book consists of a compilation of published papers. As such, the writing is concise and has with it some of the excitement of the research that is being described.

At a more fundamental level, Reiss et al. use structural equation modeling (SEM) to explain their results. By definition, SEM is not allowed to have a feedback loop so it presents a linear interpretation of

what is essentially a dynamic system that has significant feedback. For someone trained in systems theory, this starts to grate after a while.

If anyone wants to read about the influence of genetics and nonshared environment on adolescent development, I would recommend *Gene–Environment Processes in Social Behaviors and Relationships*; however, both books are four years old and a quick search of Medline indicates a small literature since their publication.

Peter Churven

I, like Hugh, excitedly opened this book hoping to discover increased understanding of the source of the mysteriously differing developmental paths of children in the same family. Such an understanding seems necessary for effective interventions in our work with children and families. Two years later, I continue to struggle to understand anything useful from this book. This seems a terrible way to feel when it is based on such a huge study of 720 families and has been some thirteen years in the making. Having found, through this Refrains, that Hugh and Ian share something of my difficulty, I am emboldened to problematise the issues rather than feel dumb: why is this book so hard to read? Are its findings useful in spite of the difficulty in reading it? Are the two related?

First, the presentation issue: paradoxically, in contrast to the linearity of the research methodology that Ian mentions, the presentation is exceedingly circular and reiterative. I imagined this was a failed attempt to help dummies like me. Findings, hypotheses and methodology are woven into an almost impenetrable and nonsequential maze. I suspect this problem arises out of Reiss's desire to write for 'a wide audience of people interested in children and families' and at the same time provide 'the only full summary of the findings of a major, complex study' (xvii). I think these aims may be incommensurable. The book, while very repetitive and theoretically discursive, fails to describe clearly the actual research methods at a level at which I can readily, critically evaluate them. For example, methods and measurement only have thirteen pages (113–126) allocated. Seven of these pages are devoted to fairly opaque tables of constructs. Furthermore, it is only on page 110 that the authors finally describe their sample, and then only for three pages. I do not think the lay reader could make sense of this book. This professional is left bored and wondering about what it all means.

I was most curious about how Reiss et al. would develop, and measure, their concept of shared and non-shared environments, their key independent variable. I remain perplexed. On the one hand Reiss talks of the 'unique family experience of the adolescent' (126). On the other hand, he states 'we did not want to ground our measurement strategy in the pursuit of the perceived family environment, [the] unique social world of the child' (118). In the analysis, Reiss states 'we focus on composite scores of the relationship subsystem' (119). I understand this to mean that when analysing a particular variable, such as parent–child disagreement, they would add together the ratings of the child, parent, and the observer rating of the videotape sample. In doing this, they have deliberately obliterated any examination of subjective perception, what I would call *experience*, in favour of examining 'the essential nature of the subsystem' as measured by composite scores (119). This methodology flies in the face of my professional intuition that the reason why different children in the same family develop so differently is that their actual lived, subjective experience of the same family environment is so different. (This is not for a minute to argue that the child's genetic potential is not very significant in determining these experiences).

Does this mean that the methodology of the study excludes the consideration of a hugely important variable in the equation that is being examined? I am also concerned that there is considerable slippage between the use of the terms *experience* and *environment*. I can imagine that Reiss et al. would say that they have explicitly stated that they are only attempting to examine 'the essential nature of the subsystem' and not individual perceptions. My response is that it is impossible to understand properly the emotional and behavioural outcomes of a person's developmental environment without measuring the individual's experience of that environment. Indeed, what is this notion of an 'essential nature of the subsystem' other than a composite score without any face validity? I fear it illustrates poor science, where relevant measures of subjectivity have been abandoned in some seeking after an 'objective', but unfruitful, holy grail.

A further major concern for me, in some senses stimulated by Hugh's piece, is with the dependent variables. The crucial thing we need to understand about genetic influences is *what* is being inherited. We need to know what we can change and what we must work with. Would it not be wise to identify variables that are likely to be genetically determined, as in the work of Thomas and Chess on temperament? Reiss et al. are measuring such complex variables as antisocial


behaviour, depression, social responsibility, and so on. These are high level, complex behaviours for which I would never expect any simple relationship with genes, except perhaps for depression.

I am frustrated at struggling with this work, only to be left confused about whether there is anything important to be learned here, be it about the limitations of the scientific method, the meaning of the results, or implications for therapy.

For me, the nature/nurture, genes/environment, either/or, debate has long ago been replaced by the 'both (genes) and (environment) perspective'. Reiss et al. have been superseded by the genome project on the one hand, and the developments in attachment and social construction theories on the other. The

important questions are what is inherited, and what are the environmental/family/developmental events and experiences that govern the genes' expression for better, or worse.

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Editors and their Sheds: Psychoanalytic Concepts

“Carl Whitaker agrees that Bowen had a great deal of personal power and probably not a little ambivalence about its use. ‘He said to me that he didn’t believe in transference,’ recalls Whitaker, ‘but I told him that five minutes after I saw him, I was transferred up to the ears myself, and he became my mother and father whether he knew it or liked it or not.’”

(Mary Sykes Wylie (1991), ‘Family Therapy’s Neglected Prophet: A Profile of Murray Bowen’, Excerpted in Richard Simon et al. (1992), *The Evolving Therapist: Ten Years of the Family Therapy Network*, Washington, The Family Therapy Network.


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