

THE EQUAL ENVIRONMENT ASSUMPTION OF CLASSICAL TWIN STUDIES MAY NOT

HOLD

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Abstract

The classical twin method - comprising comparisons of MZ and DZ twins – in the domain of cognitive abilities and attainments has led to wide acceptance of results suggesting large amount of additive genetic variance, with far-reaching implications both for the nature of future studies on the causes of cognitive variance and for intervention policies, as in education. However, that interpretation is only valid if the method observes a number of conditions which have to hold. Here we show that the most crucial of these, namely the equal environments assumption (EEA), may not hold. Consequently, differences in twin correlations might be at least partly explained by treatment effects from parents, teachers, peers, and so on. In addition, well-known interactions at various levels confound the model of simple additive effects on which the classical twin method is predicated and results of it interpreted. For example, at a sociocognitive level, DZ twins may respond to treatments differently from MZ twins. This interaction may further explain MZ-DZ correlation differences. There is abundant evidence for such interactive effects in published twin data. In the Discussion we suggest that there is a need for a more thorough examination of these problems.

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Introduction

The classical twin research design has been frequently used to describe sources of variance in cognitive abilities and attainments. Because monozygotic (MZ) twins are genetically identical, whilst dizygotic (DZ) pairs share only half their variable genes, on average, it is assumed differences in intra-pair correlations can be used to estimate the amount of the total variation that is genetic in origin. The practice of doubling the difference to obtain estimates of heritability – the proportion of trait variation attributable to genetic variation within the population sampled - is now commonplace. Heritabilities of 0.5 - 0.8 for cognitive abilities and attainments have been widely accepted (Plomin & DeFries, 1998; Scarr, 1997; Walker et al, in press). However, the conceptual simplicity underlying the method requires that a number of empirical pre-conditions be met. As Rutter (2002, p4) explains, ‘all too often there has been a cavalier ignoring...of violation of key assumptions of the twin design’. Although there are many examples of the possible violations (see Richardson, 1998 for review), in the first part of this paper we focus on the most crucial of these, the ‘equal environments assumption’ (EEA), which presupposes that treatment effects, through which parents, teachers, peers and others treat the different kinds of twins differently, are randomised across the two sets of twins. In this paper, we review the evidence why the EEA may not hold for cognitive abilities and attainments. Where appropriate, we highlight associated educational and psychological implications. Surprisingly, there have only been three attempts to test the EEA. Each of these attempts seems to have methodological problems. Also discussed, is another

key assumption of this design. The practice of estimating heritabilities from twin correlations (e.g. by doubling the MZ-DZ correlation difference) assumes that variance-creating genetic and/or environmental factors only have additive/independent effects. However, there is now considerable evidence for interactions between factors at and between several levels of development. At the one extreme, interactions between genes and between genes and environments have been well-demonstrated in animal research, such that there appear to be very few truly independent/additive gene factors underlying complex traits (Glazier, Nadeau & Aitman, 2002). At the other extreme are sociocognitive interactions. For example, because of the way they have been differently treated, DZ pairs may have more different perceptions of their social worlds, and adapt to them as active centres of cognition, rather than passive recipients of genetic and environmental forces, with different self-concepts, different learning aspirations, and so on, compared with MZ pairs. These interactive effects might further depress DZ correlations and further explain MZ-DZ correlation differences. We consider evidence that such interactive effects are present in published twin correlations.

Together, these problems can readily account for the classical patterns of twin correlations and compromise the twin method for 'genetic' research except in the very special circumstances that we raise in the Discussion. Note that this critique also applies to more recent statistical model-fitting analyses to twin data which are based on similar assumptions. In the Discussion we will also consider the implications of 'interactive' as opposed to additive models of cognitive development and variation for social policy, especially education.

The EEA problem in MZ-DZ designs

The main problem with the classical twin design is put succinctly by Kendler et al (1993) as follows: "The traditional twin method, as well as more recent biometrical models for twin analysis,

are predicated on the equal environment assumption - that monozygotic (MZ) and dizygotic (DZ) twins are equally correlated for their exposure to environmental influences that are of etiological relevance to the trait under study". Hettema, Neale and Kendler (1995, p327) explain how "The validity of the EEA is crucial to these studies because, if incorrect, excess resemblance of MZ twins compared to DZ twins usually ascribed to genetic factors could be partly or entirely due to environmental factors".

It is known that numerous aspects of home and other experience are markedly more similar for MZ twins than for DZ twins. In one recent review, Evans & Martin (2000, p77) say that 'There is overwhelming evidence that MZ twins are treated more similarly than their DZ counterparts. As children, MZ twins are more likely to have the same playmates, share the same room and dress alike. As adults, MZ twins are more likely to keep in contact, than same-sex DZ twins'. In another review, Joseph (2000) cites questionnaire studies revealing very large differences between MZ and DZ pairs in experiences like "identity confusion" (91% vs. 10%); being "brought up as a unit" (72% vs. 19%); being "inseparable as children" (73% vs. 19%); and having an "extremely strong" level of closeness (65% vs. 19%). It is also known that parents "hold more similar expectations for their MZ than DZ twins with respect to social responsibility and independence" (Scarr and Carter-Saltzman, 1979, p528).

A proper test of the EEA cannot, of course, be straightforward because it is really necessary to understand what the 'environmental' variables are that might causally be involved in trait variance. These will almost certainly be different for different traits. As Sternberg (1995, p260) says with respect to IQ, psychologists "do not have a very good understanding of the factors that affect IQ. Neither does anyone else". Bouchard (1997) agrees that, "in spite of years of concerted effort by psychologists, there is very little knowledge of the trait-relevant environments that influence IQ".

Much the same applies to other aspects of ability such as school attainment. It is usually assumed that, if MZ and DZ samples are balanced for a few demographic correlates of abilities or attainments, such as the socio-economic status of the home, then similarity of trait-relevant environments has been established and effects will be suitably randomised. But this practice does not provide a genuine causal connection with trait variance, which might actually arise from other factors not balanced across twin types.

In spite of these intrinsic uncertainties about the EEA, some claims are made in defence of it. 'This possible confounding effect has been examined and, in research to date, does not appear to represent a major problem for the twin design' (Plomin and Daniels, 1987, p3). Walker et al (in press) claim that the EEA 'is supported by numerous studies'. In what is probably the strongest recent defence of the EEA, Bouchard (1997, p135) says that 'A large number of studies have now been carried out on this problem'. Bouchard cites eleven papers in support of the EEA. However, further examination shows that only three are at all concerned with the EEA for cognitive abilities or attainments, the remainder being about personality traits, re-citations or book reviews. Likewise, Walker et al, cite three studies, where only one of them is related to cognitive abilities or attainments, the others being about psychiatric illness. Indeed, across the numerous reports of twin studies of cognitive abilities/attainments we are aware of, support for the EEA, when it is invoked, almost invariably relies on one or more of only three studies, all done in the 1970's: those of Loehlin and Nichols (1976), Matheny, Wilson and Dolan, (1976) and Scarr and Carter-Salzman (1979). It is those studies we now go on to examine.

Defending the EEA

The study most cited in support of the EEA is that of Loehlin and Nichols (1976). They found little association between how much the twins played together, were dressed similarly, and so

on, and intra-pair differences on the National Merit Scholarship Test. It is the absence of such association that is said to support the EEA.

This conclusion illustrates two problems common to all defences of the EEA so far. The first is that any strategy that searches for the absence of an association (or 'proving a negative') is acutely prone to Type II errors, which is increased because measures contain different sources of variance which should be controlled. Loehlin and Nichols used mailed self-reporting techniques, which have weaknesses of biased impressions, poor memory, and poor reliability (McCartney, Harris and Bernieri, 1990). For example, such measures typically show reliabilities of only 0.3 to 0.5 (e.g. Ge and Chadoret, 1996; O'Connor and Deater-Deckard, 1998).

This unreliability is compounded by the second problem which lies in the use of difference scores as correlates. As Bouchard (1997, p134) says, "Statistically sophisticated readers may note that difference scores are notoriously unreliable". Loehlin and Nichols (1976) themselves say that their results do "not altogether exclude a completely environmentalist position". The data do, it seems to us, have something important to say concerning the environment. And the upshot of what they say is that it operates in remarkably mysterious ways" (p94). This does not suggest a clear scientific test of the EEA.

The second study often cited is that of Matheny, Wilson and Dolan, (1976). They used 121 MZ twin pairs and 70 DZ's that had appeared in a large longitudinal study of child development in the USA. The subjects had an age range of 3.5 to 13 years. Scores of physical similarity were obtained by sending a questionnaire to parents containing ten items on physical resemblance, including facial appearance, height, whether or not the twins were ever confused for one another, and so on. Intra-pair differences on the composite scores that resulted were then correlated with

differences in intelligence test scores (the Stanford-Binet or the WISC depending on age). No significant association was found: support for the EEA was thus concluded.

This study suffers from the unreliability of difference measures just mentioned, for this reason, its conclusions can be doubted on those grounds alone. The small numbers of DZ twins in comparison to the numbers of MZ twins (70 c.w. 121 pairs) suggest a large measure of selection in the sample, implying a tendency only for the parents of twins, DZ as well as MZ, who were most similar to respond (which would reduce associations). In addition there are problems stemming from the peculiar structure of the physical similarity scale. On six of the questions (e.g. height, hair colour, facial appearance), parents were asked to respond with '0' (not at all similar), '1' (somewhat similar), or '2' (exactly similar). With another four questions (e.g. "is it hard for strangers to tell them apart?"; "Does father/mother ever confuse them?"), only a dichotomous scale was used: 0 ('no') or 2 ('yes'). Responses were then added together to make a 20-point scale of twin similarity. Such similarity scores will tend to lead to acute restrictions of range: indeed, 90% of MZs had similarity scores ranging from 12 to 20, while 80% of the DZs had scores between 0 to 8. A rank order correlation was used, but, with large numbers of pairs ranked on scores from 0-8 or 12-20, there are, inevitably, many tied ranks, further reducing the possibility of significant association. In addition the twins covered a wide age range, from 3-13 years, and although results are presented for 'younger' and 'older' children separately (age ranges not given) it isn't clear that age ranges are homogeneous across zygosity groups.

Note that such results report IQ-difference associations with parental perceptions of twin similarity, not how similarly parents actually treated the pairs, which is what a test of the EEA actually demands. However, Matheny et al also asked parents to rate the frequency with which they dressed their twins alike, which can be taken as a rough index of actual treatment. There was a

significant correlation between physical similarity and similarity of dress for the fraternal twins (but not for the MZ twins, which is to be expected given the restriction of range). In addition, 'for both identical and fraternal twins, there were significant correlations between socio-economic status and similarity of dress...the trend was for parents of lower socio-economic status to dress their children more alike' (p349). Since a correlation between socio-economic status and IQ is well known, this result suggests more intense similarity of treatment toward the lower range of IQ with possible attendant effects on IQ difference scores. Such an interaction may further detract from the overall correlation being sought. Again, we suggest that this study does not constitute a very powerful test of the EEA.

The third relevant study is that reported by Scarr and Carter-Salzman (1979) which focused on disagreements among adolescent twins about their true zygosity. Although parents are rarely wrong about their twins' zygosity (e.g. Rietveld et al, 2000), twins themselves seem less certain. In this sample, of 174 pairs of MZs only 104 agreed that they were MZ; of 169 DZ's only 101 agreed they were DZ: i.e. only 60% agreement (true zygosity assessed by blood typing). The point of the study was to examine (i) whether the IQ resemblances among pairs were more associated with their true zygosity than their perceived zygosity; and (ii) whether their facial and physical resemblance as perceived by others, and thus as a presumptive index of 'treatment' by others, was associated with resemblance in IQ.

Twins' perceived zygosity and similarities of appearance were given in answer to straight questions in an interview, including questions such as "Do you and your twin look as alike as carbon copies?" Other people's perceptions of twins' resemblances in appearance were estimated from ratings of black and white photographs of faces by eight independent psychologists. Various anthropological measures (height, weight, and so on) were also taken, as well as differences a dozen

blood group markers. The children, who were 10-16 yrs old, were tested on four different tests: the Raven's Matrices test; the Peabody Picture Vocabulary Test; the Columbia Mental Maturity Scale, and the Revised Visual Retention Test (a memory test). In some tables these scores are presented separately, and sometimes as an average 'cognitive measure', the reason for which is unexplained and confusing. With respect to at least some comparisons, however, there was "a highly inconsistent pattern of results across tests" (p532).

According to the authors, the first results show that "On intellectual measures co-twins were found to resemble each other according to their true, not self-perceived, zygosity" (p532): i.e. the data supported the EEA. The average absolute differences on the standardized scores are presented in Table 1 (taken from Scarr and Carter-Salzman's, 1979, Table II). These results do not seem to us to be as categorical as the authors suggest: those MZs who think that they are DZs, presumably because they look or behave less alike, have inter-twin differences that are substantially greater than those who agree that they are MZs (first row of correlations). Indeed they are only slightly smaller than those DZs who agreed they are DZs (third column of correlations). Our other worry is that only results using the pooled test scores are reported, especially as the authors say that the differences fail to reach statistical significance because of 'a highly inconsistent pattern of results across tests', and every other Table in the paper gives the scores of the different tests separately. Again we stress that such correlational results, in any case, give no direct test of the EEA because self-perceptions of similarity, differences in cognition and treatment effects may be causes or results of each other.

[Table 1 about here]

Scarr and Carter-Salzman's (1979) main results were from linear regression analyses in which intra-pair difference scores on IQ tests formed the dependent variable with all the other ratings and measures of similarity as independent variables. For the MZ twins it was found that

"The Ravens Matrices score differences were significantly predicted by differences in appearance as rated by eight psychologists" (p535). This result confounds the EEA (on the assumption that differences in appearance lead to differences in treatment). The regression results for the DZ twins "yielded a mixed picture" (p536), as they did for the combined sample, in that larger differences in Ravens scores were associated with smaller differences in appearance ratings and with larger differences in blood groups (the only other significant IV).

Again, these results do not offer unequivocal support for the EEA. In any case there are many problems in interpreting such analyses. The coefficients that emerge estimate 'effects' for each variable when the effects of all the other variables have been accounted for. An important assumption is that the effects of all variables are independent of each other. The dozen or so similarity measures used by Scarr and Carter-Salzman (1979) are clearly not independent of one another. In addition, since all these factors still accounted for less than 3% of the total variance in Ravens difference scores, there are obviously many variance-explaining factors missing. Glymour (1997) has shown that omitted variables in regression studies can seriously distort coefficients. As Cohen and Cohen (1983, p92) explain, interpretation resides "not in the coefficients but in one's understanding of the causal processes that give rise to the coefficients".

The difficulty of such an understanding in the Scarr and Carter-Salzman study is shown in the inconsistency, not only between test scores (as already mentioned), but between all the other ratings reported. Thus, twins' judgments of their own zygosity correlated only 0.55 with self-ratings of resemblance. Also surprisingly, ratings of twins' zygosity by psychologists using only black and white photographs turned out to be more accurate than those of the twins themselves (correlation of 0.66 with blood group differences compared with only 0.57). The psychologists' ratings and twins' judgments of similarity also correlated only moderately at 0.51. So twins' perceptions of their

similarity are not very reliable guides either to their mutual resemblance, or to others' perceptions of their resemblance (and thus others' treatment effects). Again, we stress that finding an association in such unreliable data is highly unlikely even where one exists and the idea that a genuine 'test' of the EEA has been carried out by Scarr and Carter-Saltzman (1979) seems optimistic.

In sum, there are methodological problems in the few attempts to assess the EEA for cognitive abilities and attainments that have actually been done. We submit that these studies do not support the validity of the EEA. In the next section we examine important aspects of twins' behaviour which, as well as offering further explanation for differences in twin correlations, coincidentally further undermine the EEA.

Interactions further explain patterns of correlations

Different treatment effects can explain at least part of the difference in correlations between MZ and DZ pairs. But there is a possible additional explanation. The classical twin design, including the interpretation of correlations, is based on the assumption that cognitive variation reflects the sums of additive 'effects' of genetic and environmental factors – i.e. with little if any 'effects' of gene-environment interaction or of adaptive processes within the organism itself. Yet an outpouring of research and theory at the molecular and epigenetic levels has increasingly revealed development through 'complex interactive systems with cascading levels of organization and numerous feedback systems' (Rollo, 1995; for other reviews see Carroll, Grenier and Wetherbee, 2001; Richardson, 1998).

Interactions at the genomic and epigenetic levels are further embedded in behavioural, cognitive and (in humans) sociocognitive interactions which can further modulate simple additive effects and reduce predictability. For example, animal studies report how hormonal regulation of

genes in response to stress can be transmitted from one generation to the next through maternal behaviour with offspring (Francis et al, 1999). Research with children has shown how physical factors like height and facial appearance influence others' treatments in a way which can affect self-concepts and behavioural variation (see Richardson, 1998, for discussion). Scholastic attainment involves interactions between parental self-efficacy beliefs, aspirations for and encouragement of their children, and the latter's perceptions of the importance of educational attainments (e.g. Bandura et al, 1996).

Interactions at the sociocognitive level may impact on twin correlations in several ways. By virtue of having cognitive systems and being part of social systems, MZ and DZ pairs will not only be treated differently, but also perceive them, reason about them and react to them differently as individuals. DZs tend to be physically more distinct than MZs. In his review of research, Bryant (1992, p136) says that "It is inevitable that parents should constantly compare and contrast...and hence look out for differences...(which) often leads to the exaggeration of character traits that in fact lie well within the normal range...parents and friends soon learn these stereotypes and, perhaps unconsciously, respond accordingly. More serious, however, is the fact that children tend to live up to parental expectations of being the one who is 'good' or 'naughty', 'quiet' or 'noisy'".

Future research needs to focus on the extent to which DZ twins tend towards distinct identities in the domain of cognition. The development of polarized identities could shift motivations away from similar learning in literate, numerate, academic and other ability-related domains. This is likely to be an active, not merely a passive, process, so that twins consciously shape different identities. In a review of parental ratings of temperament, Saudino (2003) has shown how such exaggerated contrasts with DZ twins begins early in infancy. Stewart (2000), in reviewing the 'social construction' of identity in childhood, describes how this can differ as between MZ and

DZ pairs, and across cultures. Akerman & Suurveen (2003) describe the special difficulties twins have in developing separate identities, and, again, how this varies between MZ and DZ twins. Schachter (1982) has hypothesized a process of de-identification among same-sex siblings as a result of competition or rivalry. It has been shown that DZs form individual identities earlier than MZs (Bryant, 1992). Segal (1984) found less cooperation among DZs than MZs. Loehlin and Nichols (1976) found that some twin pairs may contrive to make themselves different from each other through what they refer to as "effects of competition or contrast" (p93). Thus they reported a significantly greater similarity in the activities of MZs than DZs at 6 years. These distinguishing effects will also explain the increasing difference between MZ and DZ IQ correlations with age (McCartney et al, 1990) which have usually been interpreted as a 'genetic' effect on the basis of assumptions being challenged in this paper. Such difference-creating interactions, reducing resemblances among DZ twins reared together (DZTs), are evident in published twin correlations. Indeed, Plomin and Loehlin (1989) alluded to 'contrast' effects to explain puzzling results in twin correlations. The effects are perhaps best illustrated in the correlations of DZ twins who don't experience such rivalry – i.e. those pairs reared apart (DZAs). The simple additive model would expect such correlations to be substantially reduced from that of around 0.5 for DZs reared together. However, this does not seem to be the case (see Kamin and Goldberger, 2002; and Joseph, 2003 for review). For example, McGue and Bouchard (1989) reported a mean r of .49 for DZAs for the 5 subtests in a Verbal Reasoning factor derived from a special abilities battery. Newman, Tellegan and Bouchard (1998) report $r = .47$ for 26 DZA pairs tested with the WISC. For a measure of "Verbal Reasoning", the r_{DZA} was .53. McCourt, et al (1999) reported a correlation of .53 for "general cognitive ability" for another subsample of 38 DZAs (which erroneously included 10 opposite-sex pairs: c.f. Joseph, 2003). In a more general review, Plomin and Loehlin (1989) report

an IQ correlation for DZAs of 0.52. The effect was also found in Shield's (1962) study in which the rDZA was .51.

More interesting are results from studies which made direct comparisons with rDZTs, such as the Swedish Adoption/Twin Study of Ageing (SATSA), also critically analysed by Kamin and Goldberger (2002). For example, with respect to the Information scale of the Wechsler test, Pedersen et al. (1992) reported the following correlations: DZT =.20, DZA =.33. A measure of general cognitive ability (the first principal component from a battery of 13 brief subtests) produced the following correlations: MZT =.80, DZT =.22, MZA =.78, DZA =.32 (Pederson et al., 1992). Another wave of testing three years later produced the correlations shown in Table 2 (Plomin et al., 1994; c.f. Kamin and Goldberger, 2002). Note that it is unlikely that these anomalous correlation contrasts can be explained by extraneous factors such as age or gender differences. For example, SATSA researchers corrected results for age and gender effects, and multiple regression analysis showed, in any case, 'no significant effects of age on twin resemblance' (Pederson et al 1992, p351).

The higher than expected correlations among DZ pairs reared apart, where contrast effects will be absent, suggest the depression of correlations among DZs reared together where such effects are present. The correlations are difficult to explain within the standard behaviour genetic framework. They are more easily explained by reference to the sociocognitive realities of twins: i.e. active cognitive systems immersed in social dynamics consisting of positive and negative sociocognitive factors at work in complex interactions and in different proportions in different kinds of twins. Apart from explaining why rDZAs are as high as, or even higher than, rDZTs, they (at least partly) explain why rMZTs are higher than rDZTs. These social dynamics may also explain why heritabilities from 'direct' estimates (biological relatives reared apart where the 'contrast'

interactions are absent) are much higher than those from ‘indirect’ estimates (relatives and non-relatives reared together, where those interactions are present)(Plomin and Loehlin, 1989). Finally, the inter-twin interactions also explain why DZs develop less resemblance as they get older creating the illusion of rising heritabilities.

Discussion and Conclusion

Many concerns have been raised about the classical twin methodology over the years, mitigating interpretations from them about either genetic or environmental sources of variance. Nonetheless, the simple doubling of MZ-DZ correlation differences (or its parallel in model-fitting approaches) to obtain estimates of heritability of cognitive abilities and attainments has become increasingly common. Here we have explored two problems which make such practice unreliable, indicating alternative explanations for MZ-DZ twin differences, and suggesting that the far-reaching conclusions often taken from such data may be questionable. We have shown first that the EEA may not hold and that well-demonstrated treatment effects can, therefore, explain at least part of the classic MZ-DZ differences. Using published correlations, we have also shown how sociocognitive interactions, in which DZ twins strive for a relative ‘apartness’, could further depress DZ correlations, thus possibly explaining another part of the differences. We conclude that further conclusions about genetic or environmental sources of variance from MZ-DZ twin data should include thorough attempts to validate the EEA with the hope that these interactions and their implications will be more thoroughly understood.

To promote the validation of the EEA there needs to be clearer understanding of the nature of the variance in cognitive abilities and attainments and the nature of environmental causes of them

(which could then be properly controlled in EEA validation studies). The current assumption that equating across groups for a few physical and demographic correlates of trait variance amounts to controlling for real causal effects could be considered naïve in the light of the complex sociocognitive and other interactions which may affect such variance. One ‘short cut’ indicator of EEA validity may be to look at score variances of MZ and DZ twins respectively. If these are equivalent it may be argued that the EEA is valid (otherwise DZ variance would be significantly greater)¹. However, there are many reasons why equivalence of variance could not be taken as a ‘test’ of the EEA. For one thing, very few studies actually report test variances (see Kamin & Goldberger, 2002 for discussion). One reason for this may be that reported scores are often approximations or statistical reductions which can severely affect variance components - e.g. the first principle component commonly used as a surrogate of ‘g’ in recent twin studies typically accounts for only around 50% of raw score variance. In addition, sampling in twin studies is sub-optimal in many ways that can alter relative score variances regardless of EEA. For example, relatively few same-sex DZ twins tend to be recruited in most studies, raising the suspicion that only parents of those pairs who are most alike physically respond to adverts. This may be exacerbated by poor social class representativeness of samples (Rutter, 2002). In addition, it is well known that MZs tend to have more difficult gestations which, under the simple behaviour genetic model, could increase MZ variance (e.g. birthweight variance is higher in MZs than DZs). On the other hand, most MZs are monochorionic, all DZs dichorionic, which (under the same simple model) should increase DZ variance. Finally, it is found that not all MZ twins are actually genetically identical, and that DZ twins may share more than 50% of their genes (Gringras & Chen, 2001). The effects of all these factors make expected MZ/DZ variance ratios quite uncertain.

¹ We are grateful to an anonymous referee for raining this argument.

Of course, hardly anyone these days questions roles for both genetic and environmental variability in creating variation in cognitive abilities and attainments. The really important issue is the nature of that involvement. The classical twin design is predicated on the assumption that the involvement in both cases is a simple additive one in which the (variable) genes and (variable) environments act as independent units, allowing one-to-one predictability from variation in a given genetic/environmental factor to variation in cognition. However, reviews of the evolutionary genetics of complex traits in animals increasingly testify to the incorporation of independent alleles into interactive gene complexes, with attendant reductions in additive loci, and thus in heritabilities (in many traits to less than 0.1 or close to zero – see e.g. Merila and Sheldon, 1999; Schonemann, 1997). Within genetics it is now widely accepted that there are very few truly independent/additive gene factors underlying complex traits (Glazier et al, 2002). We would expect this picture especially to apply to highly evolved traits like human cognitive abilities.

Although often acknowledging interactions (e.g. Plomin, 1999) it seems strange that investigators keep on ‘fitting’ and favouring additive models, as if wishing to enjoy the force of argument both ways. But the implications of the rival (additive versus interactive) models for interventional policy, as in education, are serious. Multi-level interactive systems, which have evolved precisely for dealing with changeable, unpredictable environments, mean that it is very difficult to predict genuine developmental status from one time to the next, or the consequences of specific environmental experiences or interventions. This (among many other things) may explain why reported associations between single gene variations and variations in cognitive ability have failed to replicate (Plomin, 2003). They may also explain why raw correlations between school attainments and either university attainments or occupational attainments tend to be very weak (William, 2002).

In consequence, we suggest that standard behaviour genetic assumptions are only valid in specific cases (and at specific levels) where ‘additive effects’ do indeed arise. These include the effects of well-known deleterious gene mutations, and teratogenic environmental effects, which produce categorical disease states, entailing a causal pattern quite different from that involved in ‘normal’ adaptive variation in cognitive abilities. Otherwise, the violation of the EEA in the classical twin design, and the ignoring of interactions, can lead to serious misinterpretations of results, as well as the controversies the studies have produced within psychology and elsewhere.

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Table 1.

Standardised IQ score differences for MZ and DZ twins: means of four tests (numbers of pairs in brackets). Source: Scarr & Carter-Salzman, 1979.

	Agree MZ	Disagree about zygosity	Agree DZ
True MZs	0.66 (89)	0.71 (41)	0.77 (20)
True DZs	0.78 (16)	0.83 (33)	0.81 (84)

Table 2

Correlations for general cognitive ability reported by Swedish Adoption/Twin Study of Ageing (SATSA).

Source: Pederson et al, 1992.

	MZT	DZT	MZA	DZA
Full original sample	.80	.22	.78	.32
Longitudinal sample, 1 st testing	.84	.06	.84	.50
Longitudinal sample, 2 nd testing	.88	.03	.70	.48