

The Environments of Adopted and Non-adopted Youth: Evidence on Range Restriction From the Sibling Interaction and Behavior Study (SIBS)

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Abstract Previous reviews of the literature have suggested that shared environmental effects may be underestimated in adoption studies because adopted individuals are exposed to a restricted range of family environments. A sample of 409 adoptive and 208 non-adoptive families from the Sibling Interaction and Behavior Study (SIBS) was used to identify the environmental dimensions on which adoptive families show greatest restriction and to determine the effect of this restriction on estimates of the adoptive sibling correlation. Relative to non-adoptive families, adoptive families experienced a 41% reduction of variance in parent disinhibitory psychopathology and an 18% reduction of variance in socioeconomic status (SES). There was limited evidence for range restriction in exposure to bad peer models, parent depression, or family climate. However, restriction in range in parent disinhibitory psychopathology and family SES had no effect on adoptive-sibling correlations for delinquency, drug use, and IQ. These data support the use of

adoption studies to obtain direct estimates of the importance of shared environmental effects on psychological development.

Keywords Adoption studies · Range restriction · Shared environment · Adolescent development

One of the most significant findings in the behavioral genetic literature concerns the nature of environmental, rather than genetic, influence. As first observed by Loehlin and Nichols (1976) and later confirmed in the classic review by Plomin and Daniels (1987), the predominant source of environmental influence on individual differences in behavior appears to be due to factors that create differences among reared-together relatives (i.e., non-shared environmental factors) rather than those that create similarities (i.e., shared environmental factors). It would be hard to overstate the significance of this finding, in part because it was wholly unexpected. In their search for the origins of individual differences in behavior, developmental psychologists have traditionally focused on what would appear to be shared environmental factors: factors like parenting style (Baumrind 1991; Maccoby 1992), socioeconomic status (SES) (Dohrenwend et al. 1992), marital conflict (McGue and Lykken 1992; Spotts et al. 2004), parental psychopathology (Lahey et al. 1998), and neighborhood characteristics (Levanthal and Brooks-Gunn 2000) that would appear to lead to similarities rather than differences among reared-together siblings.

The conclusion that shared environmental influences appear to exert minimal impact on psychological development has led some to question the primacy

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accorded parents in traditional theories of development (Harris 1995, 1998; Rowe 1994). This, in turn, has prompted a spirited defense of the role of parents in the socialization of their children (Collins et al. 2000). It has also resulted in the initiation of major research initiatives aimed at identifying and characterizing what behavioral genetic research implies to be the major source of environmental influence: non-shared environmental factors (Reiss et al. 1994, 2000; Turkheimer and Waldron 2000). In this way, the shared/non-shared environmental distinction and associated research findings have had a profound impact on psychology, an impact that extends far beyond the confines of behavioral genetics.

The inference of minimal shared environmental influences is based on several lines of evidence. First, studies of reared-together twins consistently show that for most psychological traits, the dizygotic twin correlation (r_{DZ}) does not exceed half the corresponding monozygotic twin correlation (r_{MZ}). In such cases, the standard estimate of the proportion of variance associated with shared environmental effects ($c^2 = 2r_{DZ} - r_{MZ}$) is at or close to zero (Plomin et al. 2000). The second set of findings comes from studies comparing the similarity of reared-apart versus reared-together twins (Bouchard et al. 1990). These show little difference in twin correlations as a function of living apart versus living together, again implying minimal shared environmental effects.

These first two lines of evidence are indirect, being based on the comparison of correlations from two types of twins. The final line of evidence is, however, direct and involves estimating shared environmental influences by the correlation for non-genetically related but reared-together siblings (i.e., adoptive siblings). On most psychological traits, the adoptive sibling correlation is near zero (McGue et al. 1996), consistent with minimal shared environmental influence.

Critics, however, have questioned the validity of some of the assumptions upon which the inference of minimal shared environmental effects is based (Baumrind 1993; Jackson 1993). Of particular note are the comprehensive critiques of adoption studies offered by Stoolmiller (1998, 1999). He argues that restriction in range in the adoptive home environment substantially attenuates the adoptive-sibling correlation, thus leading to a gross underestimate of shared environmental effects.

There are several reasons to expect that the range of environmental exposure for adopted children would be restricted relative to the range for non-adopted children. Parents willing to submit to the invasive and form-intensive process required to adopt a child are not likely

to constitute a random subset of the population. Moreover, most adoption agencies engage in some level of screening during the adoption process. This would seem likely to result in a population of parents that enjoy greater financial security, marital stability, and mental health than the general population. Surprisingly, however, there is little direct evidence of the magnitude of selection effects in adoption studies. Stoolmiller (1998, 1999) does review individual studies that show substantial range restriction in socioeconomic background (Fergusson et al. 1995) and parent criminality (Cloninger et al. 1982), but concludes that, “no study that I am aware of has documented the extent of range restriction of the adoptive [family environment]” (Stoolmiller 1999; p. 395).

In the absence of direct evidence of range restriction in adoptive home environments, Stoolmiller (1999) has had to rely primarily on indirect evidence: a comparison of means and standard deviations published in adoption studies to those published in other studies of non-adoptive families or in test manuals. When assessed this way, there is indeed substantial evidence of restriction in range for adoptive families participating in research. For example, the variance of the total score from the Home Observation for Measurement of the Environment (HOME) is only about 30% as large among adoptive families in the Colorado Adoption Project (Plomin and DeFries 1985) as the variance published in the test manual (Caldwell and Bradley 1978). However, while the 70% reduction in HOME score variance may be indicative of range restriction, it may also reflect different modes of sample ascertainment. Moreover, restriction in range of HOME scores, even if substantial, would only have an effect on the adoptive-sibling correlation for an outcome measure if the HOME is environmentally related to that outcome.

The purpose of the present study is to explore systematically the evidence for environmental range restriction in adoptive, as compared to non-adoptive, families. Moreover, we seek to determine whether range restriction attenuates adoptive sibling correlations and resulting estimates of shared environmental effects. The present study is designed to address several limitations in the existing literature. First, it is based on a sample of adoptive and non-adoptive families who were ascertained using similar methodologies as part of the Sibling Interaction and Behavior Study (SIBS). Second, our assessment of environmental exposure is extensive and includes measures of parent psychopathology, parent-adolescent relationship, sibling relationship, peer-group models, and family SES. Finally, unlike previous research that has attempted to assess the effect of range restriction on

shared environmental estimates indirectly, we assess the effects of selection directly.

Our assessment is based on three domains of adolescent functioning that have shown evidence of shared environmental influence in previous research: IQ (McGue et al. 1993; Plomin et al. 1997), delinquency (Lyons et al. 1995; McGue et al. 1996), and drug use (Han et al. 1999; Maes et al. 1999). The specific questions we sought to address are:

1. Which dimensions of environmental exposure show evidence of restriction of range in adoptive families and what is the extent of that restriction?
2. What impact does range restriction in adoptive family environments have on estimates of shared environmental influence on adolescent delinquency, drug use, and IQ?

Method

Sample

The SIBS sample includes 409 adoptive and 208 non-adoptive families, each consisting of an adolescent sibling pair and one or both of their parents. Adoptive families were ascertained from infant placements made by the three largest, private adoption agencies in Minnesota. Non-adoptive families were ascertained through Minnesota state birth records and selected to have a pair of siblings of comparable age and gender to the adoptive sibling pairs. Families were located using the names of parents obtained from birth or adoption records and using publicly available records (e.g., phone directories). The current addresses of 90% of the adoptive and 85% of the biological families were determined. Once located, a parent in each family (usually the mother, but occasionally some other rearing parent) was interviewed to establish study eligibility. Eligibility requirements for adoptive families included having: (1) an adopted adolescent between the ages of 11 and 21 who had been placed permanently in the adoptive home prior to the age of 2 years, and (2) a second adolescent in the home who was not biologically related to the adopted adolescent. This second child could have been biologically related to one or both of the parents or, like the first child, adopted and placed prior to age 2 years. Eligibility requirements for non-adoptive families included having a pair of full biological adolescent siblings. Additional eligibility requirements for both types of families included living within driving distance of our labs at the

University of Minnesota, siblings being no more than 5 years apart in age, and neither adolescent offspring having any physical or mental handicap that would preclude their completing our daylong intake assessment.

Among eligible families invited to participate, rate of participation was higher, albeit not significantly higher ($\chi^2(1 \text{ df}) = 3.42, P = 0.064$), among adoptive (63.2%) as compared to non-adoptive (57.3%) families. To assess the representativeness of participating families, a brief demographic interview was administered over the phone to 73% of non-participating but eligible families. Among adoptive families, participating and non-participating families did not differ significantly on mother's and father's education, mother's and father's occupational status, percent of original parents who remained married, or the number of parent-reported behavioral disorders (learning disability, substance abuse, attention deficit disorder, and depression) in their eligible offspring. Among non-adoptive families, participating and non-participating families differed significantly on only one of these six variables: rate of college education was significantly greater among participating mothers (43.8%) than non-participating mothers (28.6%) ($\chi^2(1 \text{ df}) = 10.0, P = 0.002$). Analysis of non-participants thus suggests that our samples of adoptive and non-adoptive families are generally representative of the populations of eligible families from which they were drawn, although there is some evidence of limited positive selection in the non-adoptive family sample.

To further assess the representativeness of the non-adoptive family sample, we made use of the integrated public use microdata series (IPUMS) 1% random sample (Ruggles et al. 2004) from Census 2000. We sampled from individuals age 35–55 living in the broader Minneapolis–St. Paul metropolitan area and living with two or more of their own children. This was done to make the Census 2000 group comparable in family composition and geographic location to the non-adoptive SIBS families; 93% of the non-adoptive and 78% of the adoptive families lived in the greater Minneapolis–St. Paul–Bloomington Metropolitan statistical area. In interpreting census comparisons, it is useful to recognize that individuals who live with two or more of their own children have higher rates of college graduation than the general population of adults. In the IPUMS Census 2000 sample, 47% of males and 39% of females have at least a college degree. These figures are similar to the 44% of dads and 44% of moms in the non-adoptive SIBS families with a college degree. Like our analysis of non-participants, this does suggest some slight positive

selection in non-adoptive moms. However, we find little evidence (from either the analysis of non-participants or comparison to Census 2000 data) that non-adoptive SIBS families differ markedly from families with parents living with two or more of their own children in the Minneapolis–St. Paul metropolitan region.

In all families, both rearing parents were invited to participate. In the 617 assessed families, 613 (99.4%) of the mothers and 551 (89.3%) of the fathers were assessed. An additional 13 (2.1%) fathers completed some of the mailed self-reports but did not complete an interview and so are not included in the sample here. Among the 1,234 targeted offspring in the 617 families, two (in two different adoptive families) were judged to be ineligible after they had completed their assessment (one because she was found to be biologically related to her participating sibling and the other because her IQ test performance suggested mild mental retardation, which would preclude completing the self-report forms). In total, 1,232 adolescents and 1,164 parents or (2,396 individuals) completed an intake SIBS assessment. At present, we have no information on the biological parents of the adopted individuals participating in SIBS.

Among the 409 adoptive families there were 124 families in which the second adolescent was a biological child of one or both of the adoptive parents (i.e., mixed adoptive/biological families) and 285 families in which both participating adolescents were adopted and placed prior to age 2 (i.e., adoptive/adoptive families). Average age of placement in the adoptive home was 4.7 months ($SD = 3.4$) for all adoptive youth. The gender composition of the sibling pairs in the adoptive families was 96 male/male, 148 female/female, and 163 male/female. The sample of adoptive families also included two female adolescents who did not have a participating sibling for the reasons noted above. The gender composition of sibling pairs in non-adoptive families was 62 male/male, 68 female/female, and 78 male/female. The mean age difference was slightly larger in adopted (mean = 2.4 years, $SD = 1.0$, $N = 407$) than non-adopted (mean = 2.1 years, $SD = 0.7$, $N = 208$) sibling pairs ($t = 3.80$, 613 df, $P < 0.001$).

Procedure

Families were assessed in our labs at the University of Minnesota. The assessment lasted approximately 5 h and included a clinical interview, video-taped family interaction tasks (not yet fully scored at the time of this report), cognitive testing, and completion of self-report inventories. Some of the routine self-report forms (e.g.,

religiousness) were mailed to family members before their visit with a request to complete the forms and return them at the time of their visit. In a few cases, assessments ran longer than planned and family members took uncompleted self-report forms home and returned them to us by mail. Available sample sizes vary somewhat across the different measures used in this report due to the consequent small amount of missing data. Each family member was interviewed by a separate interviewer unaware of the responses of other family members, but not unaware of the adoption status of the families, as this would have been hard to mask given that the parents and children in many of the adoptive families were of different ethnicity. Interviewers had either a B.A. or M.A. in psychology (or related field), underwent extensive training and evaluation prior to qualifying to administer the clinical interviews, and were monitored throughout the study by a consensus team staffed by individuals with advanced clinical training. Travel costs were paid and each family member received a small honorarium in return for their participation.

Measures

We describe here only the measures used in the current report rather than the full SIBS assessment battery.

Parent psychopathology

Parents were administered the Structured Clinical Interview for DSM-III-R (SCID-R) (Spitzer et al. 1987), updated to cover DSM-IV criteria for major depressive disorder (MDD), and an updated interview adapted from the Structured Clinical Interview for DSM-III-R Personality Disorders (SCID-II) to cover DSM-IV criteria for antisocial personality disorder (ASPD). Parents were also administered the expanded substance abuse module (SAM), developed by Robins et al. (1987) as a supplement to the World Health Organization's Composite International Diagnostic Interview (CIDI) (Robins et al. 1987), again updated to cover DSM-IV criteria. Symptoms of dependence on the following substances were assessed: nicotine, alcohol, cannabis, amphetamines, sedatives, cocaine, hallucinogens, inhalants, opioids, and PCPs. The evidence supporting the existence of each positive symptom was reviewed in a consensus case conference by at least two individuals with advanced clinical training. After consensus review, computer algorithms that applied the DSM-IV criteria were used to assign diagnoses.

The following lifetime diagnoses were used in the present report: ASPD, MDD, alcohol dependence,

nicotine dependence, and illicit drug dependence (the latter based on the 8 substances other than alcohol and nicotine listed above). Diagnoses were made at two levels of certainty: definite = all DSM criteria were met, and probable = one of the necessary symptom criteria was absent. Our use of two levels of certainty is predicated on the fact that the DSM was developed to assess acute rather than lifetime psychopathology, so that our approach attempts to minimize the likelihood of false negatives due to imperfect recall of symptoms over an extended period of time. Iacono et al. (1999) summarized the evidence for the reliability of our diagnostic procedures and reported kappas of 0.89 or higher for all diagnoses used in this report. For each diagnostic category, to provide a sensitive measure of exposure to parental psychopathology, we also made use of a symptom count variable computed as the sum of DSM symptoms coded 1 if positive, 0.5 if subthreshold, and 0 if negative. Symptom scales were log-transformed to reduce their positive skewness prior to statistical analysis.

Family socioeconomic status (SES)

Each parent's level of education was coded on a 5-point scale (1 = less than high school, 2 = high school of GED, 3 = some college, 4 = college degree, 5 = professional degree). Parents' occupational status was coded on a 6-point scale using the Hollingshead classification scheme (1 = professional/managerial to 6 = manual laborer). Students and fulltime homemakers were coded as missing on this scale.

Family functioning

Three measures of family functioning were used. First, the quality of the parent–adolescent relationship was assessed using adolescents' responses to the Parent Environment Questionnaire (PEQ) (Elkins et al. 1997). The PEQ is a 42-item self-report inventory that assesses five central features of the parent–child relationship: Conflict, Involvement, Regard for Parent, Regard for Child, and Structure. Each adolescent completed the PEQ up to two times, once for each of their rearing parents. The resulting scales were averaged across parents following McGue et al. (2005) to obtain aggregate scales for the parent–adolescent relationship. A complete description of the PEQ, including its psychometric properties and sample items, is given by Elkins et al. (1997). Because they found that the first 4 PEQ scales (i.e., all except Structure) loaded on a single factor reflecting a dimension of conflict versus warmth, we used this factor score as an overall

measure of the parent–adolescent relationship, with high scores reflecting high warmth and low scores reflecting conflict.

Second, adolescents reported on their relationship with their participating sibling using the Sibling Relationship Questionnaire (SRQ) (Furman and Buhrmester 1985). The SRQ consists of 15 primary scales (e.g., Competition, Companionship, Quarreling), each of which is assessed by three items. Scale internal consistency reliability averages 0.80. Factor analysis of the SRQ scales reveals two underlying dimensions: Sibling Positivity and Sibling Negativity. We used as an overall measure of the sibling relationship, the Sibling Negativity factor, which loaded principally on the Antagonism, Competition, and Quarreling scales of the SRQ.

Finally, the quality of the relationship between the rearing parents was assessed using the Dyadic Adjustment Scale (DAS) (Spanier 1976). The DAS was completed by both rearing parents, and an overall measure was obtained by averaging the parents' total DAS scores. The DAS total score is highly reliable, with an internal consistency reliability of 0.96.

Peer models

Exposure to peer models was based on adolescent responses to the 19-item Friends inventory (Walden et al. 2004). Factor analysis of the Friends inventory reveals two factors: Good Peer Models and Bad Peer Models. We used as our overall measure of peer relationships the Bad Peer Models scale, which is composed of items such as “My friends break the rules” and “My friends get into trouble with the police.” It has an internal consistency reliability of 0.85.

Adolescent outcomes

Three measures of adolescent functioning, chosen because previous research had suggested these measures would be affected by shared environmental influences and thus susceptible to selection effects, were analyzed here. The first measure was the 36-item Delinquent Behavior Inventory (DBI) (Gibson 1967). The DBI is a checklist of minor (e.g., skipping school) and more serious (e.g., using a weapon in a fight) delinquent behaviors that has high internal consistency reliability (0.96). The second outcome measure was the number of 10 drugs (tobacco, alcohol, marijuana, stimulants, tranquilizers, Quaaludes, cocaine, psychedelics, inhalants, opioids, and non-prescriptives) the adolescent reported having ever used in a computerized assessment of substance use behavior. The resulting NDRG measure assesses youth substance involvement

and has adequate internal consistency reliability (0.71). Adolescent IQ was assessed using an abbreviated version of either the WISC-R (for adolescents age 15 years and younger) or the WAIS-R (for adolescents age 16 and older). The abbreviated form consists of four subtests, two verbal (Vocabulary and Information) and two performance (Block Design and Picture Arrangement), selected because performance on these subtests correlates 0.90 with overall IQ when all subtests are administered (Kaufman 1990). Both the DBI and NDRG measures were log-transformed to reduce skewness, and age-sex adjusted using the regression approach described by McGue and Bouchard (1984) prior to statistical analyses reported here. IQ scores, which are already normed to account for age effects, were not transformed.

Data analysis

Evidence for restriction of range in the environments of adopted as compared to non-adoptive adolescents was investigated by comparing both the means and the variances on measures of rearing parent psychopathology, SES, family functioning, and peer models. The relationship between each environmental domain and each measure of offspring functioning was assessed separately in adoptive and non-adoptive families using both regression and correlation techniques. Our previous research has shown greater similarity among like-sex as compared to unlike-sex adoptive sibling pairs (McGue et al. 1996). Preliminary analysis of the current outcome measures also revealed some evidence for greater similarity among like-sex as compared to unlike-sex sibling pairs. For example, for the three main outcome measures investigated here, DBI, NDRG and IQ, the unlike-sex/like-sex correlations were 0.12/0.27, 0.05/0.25, and 0.10/0.21, respectively. We also did not find evidence that sibling similarity varied significantly between M/M versus F/F pairs. As a thorough investigation of the moderating effects of gender on sibling similarity is outside the scope of the present report, we focus here on evidence for shared environmental effects from like-sex adopted sibling pairs only. An analysis of gender effects on sibling similarity is included in another report from the SIBS project (Buchanan et al. 2006). Adoptive sibling correlations are reported both uncorrected and corrected for the effects of selection.

We used the Pearson–Lawley selection formula (Aitken 1934) to assess the impact of range restriction on correlations in adoptive families, including correlations between adoptive siblings and correlations between adolescent outcomes and environmental

indices. That is, we wished to model the effects of selection on p environmental variables on the correlation structure of q outcome measures. If we denote variance–covariance parameters in the unselected population by unscripted characters and in the selected population by characters superscripted by \sim , then the combined $(p + q) \times (p + q)$ variance–covariance matrix in the unselected population is given by:

$$V = \begin{bmatrix} V_{pp} & V_{pq} \\ V'_{pq} & V_{qq} \end{bmatrix}$$

and according to the Pearson–Lawley formulae, the $(p + q) \times (p + q)$ variance-covariance matrix in the selected population is given by:

$$\tilde{V} = \begin{bmatrix} \tilde{V}_{pp} & \tilde{V}_{pp} V_{pp}^{-1} V_{pq} \\ V'_{pq} V_{pp}^{-1} \tilde{V}_{pp} & V_{qq} - V'_{pq} (V_{pp}^{-1} - V_{pp}^{-1} \tilde{V}_{pp} V_{pp}^{-1}) V_{pq} \end{bmatrix}$$

Where the magnitude of \tilde{V}_{pp} relative to V_{pp} reflects the degree of range restriction. The Pearson–Lawley correction is based on the fact that restriction in range affects the correlations but not the unstandardized coefficients of the regression of the q outcome variables on the p selection variables. That is, in the standard regression model, the form of the distribution of the dependent variable (y) conditional on the observed values of the independent variable ($x = X$) does not depend on the distribution of the independent variables (Morrison 1976):

$$E(y|x = X) = X\beta + e, e \sim N(0, \sigma^2),$$

where E is the expectation operator, β is the regression coefficient and e is the residual term. The application of the fact that β is homogeneous across levels of the independent variable can be seen by comparing the matrix of coefficients for the regression of the q outcome variables on the p selection variables in the unselected population,

$$\beta = V'_{pq} V_{pp}^{-1}$$

with the matrix of regression coefficients after selection

$$\tilde{\beta} = V'_{pq} V^{-1} \tilde{V}_{pp} \tilde{V}_{pp}^{-1} = V'_{pq} V_{pp}^{-1} = \beta.$$

It is informative then to substitute β into the Pearson–Lawley formula to determine how the effects of selection depend on the relationship between the selected and outcome variables. The expected

variance-covariance matrix among the q outcome measures in the selected population is given by

$$\begin{aligned} \tilde{V}_{qq} &= V_{qq} - V'_{pq} \left(V_{pp}^{-1} - V_{pp}^{-1} \tilde{V}_{pp} V_{pp}^{-1} \right) V_{pq} \\ &= V_{qq} - \beta (V_{pp} - \tilde{V}_{pp}) \beta' \end{aligned}$$

So that the qxq variance-covariance matrix in the selected population is negatively biased only if both (1) the regression of the outcomes on the selected variables is non-zero (i.e., $\beta \neq 0$), and (2) there is range restriction (i.e., $\tilde{V}_{pp} \neq V_{pp}$). In cases where both these conditions are met, an estimate of the variance-covariance matrix of the outcome variables corrected for the effects of selection is given by

$$V_{qq} = \tilde{V}_{qq} + \beta (V_{pp} - \tilde{V}_{pp}) \beta'$$

The Pearson–Lawley model was fit to the adoptive and non-adoptive family data using the Mx software system (Neale et al. 1999) to get estimates of correlations in the adoptive families both uncorrected (i.e., using \tilde{V}_{qq}) and corrected (using V_{pp}) for the effects of selection. It was assumed that non-adoptive families had not experienced any selection, consistent with data presented above.

Results

Descriptive statistics

Table 1 gives descriptive information on the parents in adoptive, non-adoptive, and mixed adoptive/biological

families. As expected, parents in adoptive/adoptive and mixed adoptive/biological families were significantly older, more likely to have a college degree, and had higher occupational status (i.e., a lower mean on the Hollingshead scale) than parents in non-adoptive families. Consistent with the demographics of Minnesota, a high percentage (>96%) of mothers and fathers were Caucasian. Although adoptive parents in both types of families also had lower rates of ASPD, alcohol dependence, and substance dependence as compared to non-adoptive parents, only the latter difference achieved statistical significance. Rates of MDD and nicotine dependence did not differ significantly across the parent groups and, in general, parents from adoptive/adoptive families were similar to parents from mixed adoptive/biological families. Because the mixed adoptive/biological families appear comparable to the adoptive/adoptive families, parents from the two are pooled into a single adoptive-family sample in subsequent comparisons with parents from non-adoptive families. In analysis of parent–offspring resemblance, however, adopted and non-adopted offspring in the mixed families will be distinguished by effectively including data from the biological offspring from the mixed families with biological offspring from non-adoptive families and including data from adoptive offspring from the mixed families with adoptive offspring from adoptive families.

Table 2 gives descriptive information on the offspring samples. On average, the offspring were in mid-adolescence. There were slightly more females than males, reflecting in part that females constitute the majority of adopted infants from some foreign countries (e.g., Korea). While the non-adoptee sample is

Table 1 Characteristics of parents in Adoptive, Non-adoptive, and Mixed Adoptive/Non-Adoptive Families

	Mothers				Fathers			
	Adopt	Non-Adopt	Mixed	P-value	Adopt	Non-Adopt	Mixed	P-value
N	281	207	123		261	175	111	
Age								
Mean (SD)	48.2 (3.7)	44.1 (4.2)	47.0 (3.5)	<0.001	49.6 (3.6)	45.9 (4.9)	48.7 (3.8)	<0.001
White (%)	98.2%	97.1%	99.2%	ns	96.5%	96.6%	97.6%	ns
College Grad (%)	60.7%	44.2%	60.5%	0.001	66.1%	44.7%	58.1%	<0.001
Occup Status ^a								
Mean (SD)	2.6 (1.1)	3.4 (1.4)	2.8 (1.3)	<0.001	2.5 (1.4)	3.1 (1.6)	2.8 (1.7)	<0.001
ASPD (%)	0.7%	1.4%	2.4%	ns	5.0%	9.1%	4.5%	ns
Major depression (%)	24.8%	21.7%	22.8%	ns	18.8%	17.0%	12.5%	ns
Nicotine dependence (%)	19.8%	21.4%	18.7%	ns	33.6%	36.4%	32.4%	ns
Alcohol dependence (%)	4.3%	8.2%	4.9%	ns	16.1%	18.2%	20.0%	ns
Drug dependence (%)	2.1%	7.7%	4.1%	0.01	5.3%	18.8%	8.1%	<0.001

Note. P-value identifies those variables for which groups differ significantly; ns = not significant; ASPD = antisocial personality disorder. All parent diagnoses are lifetime according to DSM-IV criteria at a definite or probable level of certainty

^a Occupational status coded on a 6-point scale with 1 reflecting professional/managerial classes

Table 2 Characteristics of offspring in Adoptive, Non-Adoptive and Mixed Adoptive/Non-Adoptive Families

	Adoptive families		Non-Adoptive Families	
	Adoptive offspring	Biological offspring	Adoptive offspring	Biological offspring
<i>N</i> of Offspring	568	416	124	124
Age				
Mean (SD)	14.8 (2.1)	15.0 (1.8)	15.5 (1.1)	14.3 (2.2)
Female (%)	57.5	51.4	46.0	62.9
<i>Ethnicity</i>				
White (%)	20.4	95.0	25.8	96.8
Asian (%)	66.8	0.0	64.5	0.0
Other (%)	12.8	5.0	9.7	3.2
DBI				
Mean (SD)	3.9 (4.4)	4.2 (4.9)	4.6 (4.1)	3.2 (4.1)
# Drugs used				
Mean (SD)	0.8 (1.3)	0.8 (1.4)	0.8 (1.2)	0.7 (1.1)
IQ				
Mean (SD)	106.3 (13.8)	107.4 (12.6)	106.6 (15.6)	109.4 (15.1)

Note: DBI = Delinquent Behavior Inventory

predominantly white, the adoptee sample is predominantly of Asian ancestry. Among adopted youth, ethnicity was not significantly associated with scores on either the delinquency ($F(2,678) = 2.2, P = 0.12$) or drug use ($F(2,683) = 0.2, P = 0.85$) measures but was significantly associated with IQ ($F(2,687) = 9.3, P < 0.001$). Mean IQ was similar for Caucasian (105.2) and Asian (107.7) adopted adolescents, but the means for both these groups were moderately higher than the mean for adopted youth of other ethnicities (100.7). Given the minimal evidence for ethnicity effects in our sample, it is not considered further here. Adopted and non-adopted adolescents had similar means on the outcome measures of delinquency, number of drugs used, and IQ. Standard deviations were also similar in the adoptee and non-adoptee samples. Since strong selection effects among adoptive parents should reduce the variance in outcome measures in this sample relative to the non-adoptee sample (and might also be expected to affect the means), absence of systematic differences in the standard deviations across the offspring groups suggests there are not strong selection effects on these variables.

Evidence for restriction in range in adoptive families

Table 3 summarizes evidence for range restriction in the quantitative scales of the environments of adolescents. To facilitate comparisons, scales were linearly transformed to have a mean of 0 and a variance of 1.0 in the non-adoptive families. Consequently, the mean in the adoptive-family sample provides a direct esti-

mate of the standardized effect size of the differences, and the variance in the adoptive-family sample directly estimates the ratio of variances in the two samples. Several consistent trends can be noted in Table 3. First, except for symptoms of depression, for which there is little group difference, average levels of parent mental health symptoms are moderately lower in adoptive as compared to non-adoptive families (standardized effect sizes generally less than 0.25). Variances for these parent symptom scales are also lower in adoptive families, especially for symptoms of parent drug dependence, which were only 30%–50% as large in the adoptive family sample relative to the non-adoptive family sample. On measures of family functioning and peer group models, there is little consistent difference between the two groups. On measures of family SES, means are 0.35–0.58 SDs higher and variances 66%–84% as large in adoptive as compared to non-adoptive families. Analysis of the individual measures in Table 3 thus suggests that selection in adoptive families is greatest for measures of SES and parent disinhibitory psychopathology.

To facilitate interpretation of overall results, we formed composites of variables in the areas of parental disinhibition, parental depression, family climate, and family SES. We formed each composite by adding together the standard scores for each of the individual indicators in that domain. We allowed there to be as many as three missing indicators in the formation of the Parent Disinhibition Composite (i.e., the composite was still formed if data was missing on one of the two parents); up to one missing indicator in the Parent Depression Composite (allowing one missing parent); up to one missing indicator in the Family Functioning

Table 3 Evidence for restriction in range for multiple quantitative indicators of the environments of adoptive offspring

Indicator	Non-Adoptive Families			Adoptive Families		
	<i>N</i>	Mean	Variance	<i>N</i>	Mean	Variance
<i>Parent disinhibition</i>						
Mother's AAB	207	0.0	1.0	406	−0.21***	0.92
Mother's Alcohol	207	0.0	1.0	405	−0.16**	0.71
Mother's Substance	204	0.0	1.0	406	−0.19***	0.51
Father's AAB	176	0.0	1.0	373	−0.27***	0.93
Father's Alcohol	176	0.0	1.0	371	−0.14*	0.72
Father's Substance	176	0.0	1.0	373	−0.31***	0.30
Disinhibition Composite	208	0.0	1.0	408	−0.33***	0.59
<i>Parent Depression</i>						
Mother's Depression	207	0.0	1.0	406	0.11	1.04
Father's Depression	176	0.0	1.0	372	0.13	1.04
Depression Composite	208	0.0	1.0	409	0.14*	0.99
<i>Family Functioning</i>						
PEQ Factor Scale	200	0.0	1.0	387	0.20**	1.19
SRQ Sibling Negativity	199	0.0	1.0	387	0.00	0.99
Dyadic Adjustment	203	0.0	1.0	397	−0.01	0.62
Family Composite	203	0.0	1.0	396	0.06	1.03
<i>Family SES</i>						
Mother's Education	208	0.0	1.0	409	0.39***	0.84
Mother's Occupation	97	0.0	1.0	195	0.56***	0.66
Father's Education	208	0.0	1.0	404	0.41***	0.74
Father's Occupation	196	0.0	1.0	383	0.33***	0.83
SES Composite	208	0.0	1.0	408	0.53***	0.82
<i>Peer Relationships</i>						
Bad Peer Models	206	0.0	1.0	401	−0.06	0.87

Note: All variables are scaled to have a mean of 0 and variance of 1.0 in the sample of non-adoptive families; consequently for adoptive families the mean corresponds to an estimate of the standardized effect size and the variance to a variance ratio. Adoptive families include all families with at least one adopted child. *N* refers to the number of families. AAB = adult antisocial behavior; PEQ = Parent Environment Questionnaire; SRQ = Sibling Relationship Questionnaire; SES = Socioeconomic status. Parent symptom scales are based on the log-transformed number of lifetime DSM-IV symptoms

Mean group difference significant at * $P < 0.05$, ** $P < 0.01$, *** $P < 0.001$, two-tailed

Composite, and up to two missing indicators in the Family SES Composite (again corresponding to one missing parent). Because each component was standardized before it was added to the composite, this method essentially involves substituting the overall mean score (i.e., 0 for a standard score) for missing data. Composite scores were again standardized in the sample of non-adoptive families and the resulting means and variances are included in Table 3. The composites suggest that range restriction in adoptive families lies principally in two domains: (1) symptoms of parent disinhibitory psychopathology, where adoptive parents score on average 1/3rd of an SD lower and have a variance only 59% as large as non-adoptive parents, and (2) family SES, where adoptive parents score on average ½ SD higher and have a variance only 82% as large as non-adoptive parents. Given these results, we have focused our analysis of the effects of range restriction on the parent disinhibition and family SES composites.

Uncorrected and selection-corrected correlations in adoptive families

Table 4 gives the like-sex sibling correlations for our outcome measures in both the non-adoptive and adoptive-family samples. The former are in every case larger than the latter, suggesting genetic effects, while the latter are in every case statistically significant, suggesting shared environmental effects. Shared environmental effects appear to account for 20%–30% of the variance in the adolescent outcome measures.

Stoolmiller (1999) has argued, however, that because of selection effects, the sibling correlation in adoptive families underestimates the true magnitude of shared environmental influences. But as we have mentioned, for selection to have an effect on the adoptive sibling outcome correlations, two conditions must be met: (1) there must be variance reduction on the selection variables, and (2) the selection variables must be associated with the outcomes. Above we

Table 4 Like-sex sibling correlations and regression of adolescent outcome measures on parent disinhibition and family SES composites

Outcome	Non-Adoptive Families			Adoptive Families		
	Like-sex Sibling Correlation	Regression Coefficient (95% CI)		Like-sex Sibling Correlation	Regression Coefficient (95% CI)	
		Parent disinhibition	Family SES		Parent disinhibition	Family SES
DBI	0.40** (N = 126)	2.9 (1.9, 3.8)	-1.8 (-2.8, -0.8)	0.30** (N = 236)	-0.5 (-1.6, 0.7)	0.1 (-0.9, 1.1)
No. of Drugs	0.35** (N = 119)	2.2 (1.3, 3.1)	-1.7 (-2.6, -0.8)	0.21** (N = 240)	-0.7 (-1.9, 0.6)	0.4 (-0.7, 1.4)
IQ	0.40** (N = 129)	-1.6 (-2.8, -0.3)	3.2 (2.0, 4.3)	0.19** (N = 242)	-1.3 (-2.9, 0.4)	0.7 (-0.7, 2.1)

Note: DBI = Delinquency Behavior Inventory; SES = Socioeconomic status. Outcome data were age–sex adjusted prior to analysis. Adoptive families include all families with at least one adopted child

Correlation significant at * $P < 0.05$, ** $P < 0.01$, *** $P < 0.001$, two-tailed

showed that variance reduction in adoptive families is most marked on measures of parent disinhibition and family SES. Table 4 gives the coefficients for the regression of the three adolescent outcome measures on the two composites in adoptive and non-adoptive families. As expected, in the non-adoptive sample, parent disinhibition is significantly positively associated with adolescent delinquency and drug use and significantly negatively associated with IQ. The family SES composite was also significantly associated with each outcome in the expected direction in the non-adoptive offspring sample. In contrast, all regression coefficients are non-significant and numerically small in the adopted offspring sample. Since selection on the independent variable does not attenuate the coefficients of the regression of the outcome on the selected variables, failure to observe significant regression in the adoptive offspring sample suggests that the adoptive sibling correlations do not underestimate shared environmental effects.

Figures 1 and 2 provide further evidence of the lack of association of parent disinhibition and family SES with adolescent outcomes among adopted youth. Plotted in the figures are mean DBI score as a function of parent disinhibition (Fig. 1) and mean IQ as a function of family SES (Fig. 2). In both cases, the predictor variables have been linearly transformed to have a mean of 0.0 and an SD of 1.0 in adoptive families. For example, a score of less than -1.0 on the standardized family SES composite corresponds to the same SES level in the two samples even though more non-adoptive than adoptive offspring would fall in this score range. As is evident from the figures, parent disinhibition and family SES have no relationship with adolescent outcomes in the adoptive sample even though they are predictably related to outcome in the non-adoptive sample.

Table 5 gives correlations in the adoptive-family sample corrected for selection on the parent disinhibition

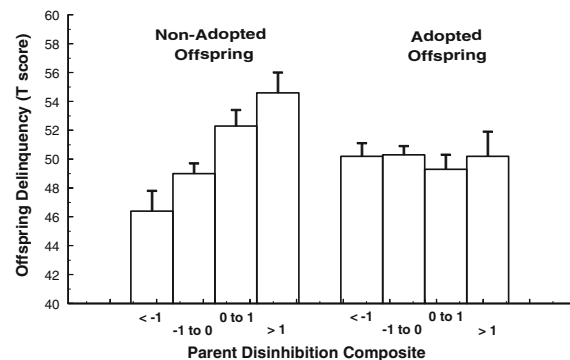


Fig. 1 Mean offspring Delinquency Behavior Inventory (DBI) score as a function of Parent Disinhibition Composite among adopted and non-adopted offspring. DBI scores have been age–sex corrected and scaled to a mean of 50 and an SD of 10 (i.e., a T-score metric) in the overall offspring sample. Parent Disinhibition Composite is scaled to have a mean of 0 and an SD of 1.0 in the non-adoptive family sample. Error bars demarcate one standard error of the mean

and family SES composites using the Pearson–Lawley selection formula. Correlations were estimated using Mx. As is apparent, the corrected correlations between the family environment composites and the adolescent outcomes are uniformly small, and the corrected sibling correlations differ minimally from their uncorrected values given in Table 4.

Discussion

The purpose of this study was to determine both the extent to which adopted individuals are exposed to a restricted range of environments and the impact of any range restriction on estimates of shared environmental effects. The study was motivated by critiques of the adoption study design, which have claimed that shared environmental effects are substantially underestimated in adoption studies because of selection effects. In the

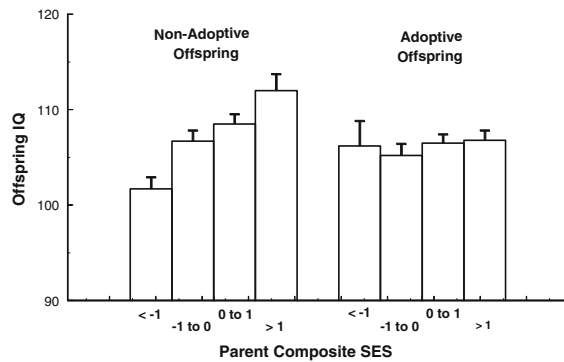


Fig. 2 Mean offspring IQ as a function of Family SES Composite among adopted and non-adopted offspring. Family SES Composite is scaled to have a mean of 0 and an SD of 1.0 in the non-adoptive family sample. Error bars demarcate one standard error of the mean

Table 5 Corrected correlations for like-sex siblings and for parent disinhibition and family SES composites with adolescent outcomes in the adoptive family sample

Outcome	Like-sex Siblings	Parent disinhibition	Family SES
DBI	0.30	0.00	0.07
# Drugs Used	0.20	-0.04	-0.02
IQ	0.20	0.00	-0.02

Correlations have been adjusted using the Pearson–Aitken formula. Outcome data were age–sex adjusted prior to analysis

most mathematically rigorous of these critiques, Stoolmiller (1999) concluded that indices of the family environment are only about 1/3rd as variable in adoptive families as in non-adoptive families, and that this reduction in variance markedly attenuates adoptive sibling correlations. For example, using data from the Texas Adoption Project (TAP) (Horn et al. 1979), Stoolmiller (1998) estimated that environmental variance was reduced 63% in the TAP sample, resulting in the observed adoptive-sibling IQ correlation of 0.22 being attenuated from a true value of 0.55. Clearly, estimates of shared environmental effects that are negatively biased by a factor of two or more would pose a major problem for those who would use the adoption study design to draw conclusions about the importance of shared environmental effects.

There are, however, several limitations to the Stoolmiller approach. First, his evidence for range restriction involved comparing variance estimates obtained in studies of adoptive families with variance estimates obtained in separate studies of non-adoptive families or from published norms. Thus, in the above mentioned example from the TAP, evidence for range restriction came from a comparison of the observed IQ

standard deviation of 11.4 in the adoption sample with the normative value of 15 (i.e., an $(11.4/15)^2 = 58\%$ reduction in variance). As Loehlin and Horn (2000) have pointed out, however, there are many reasons why descriptive data from a specific study might differ from normative data other than restriction of range in environmental exposure. For example, variance estimates are likely to be sensitive to ascertainment, making it difficult to compare samples ascertained using different approaches. In SIBS, we exclude from participation adolescents with IQs less than 70 because they would have difficulty understanding and reliably responding to the many self-report and interview questions we ask. This ascertainment criterion will certainly result in a reduction in IQ variance in both adoptive and non-adoptive families, and indeed the IQ standard deviation we observed in our sample of adopted individuals was less than the normative value of 15. This reduction in IQ variance has, however, nothing to do with range restriction specific to the adoptive families, as evidenced by the comparable level of IQ variance we observed in our similarly ascertained non-adoptive sibling sample.

The second limitation to the Stoolmiller approach is that it is based on the assumption that lower variance in adoptive family samples relative to published norms or to variance in non-adoptive family samples, is due entirely to restriction in environmental variance. This assumption is untested in the Stoolmiller approach and indeed appears to be very problematic given evidence, for example, that low IQ appears to be highly heritable (Petrill et al. 1997) and that measures like the HOME reflect genetic as well as non-genetic variance (Plomin et al. 1994).

We sought to address these limitations through an analysis of environmental measures from SIBS. SIBS allowed us to compare variance in these measures from adoptive and non-adoptive families ascertained using comparable procedures. It also allowed us to determine whether the relationships between variables with restricted variance and adolescent outcomes were mediated entirely by shared environmental factors, as assumed in the Stoolmiller analysis. Our findings indicate that while there was evidence for restriction of range of environmental exposure in adoptive relative to non-adoptive families, this range restriction appeared to have no demonstrable effect on estimates of shared environmental variance.

We looked for evidence of range restriction in five different domains of environmental exposure: parent disinhibitory psychopathology, parent depression, family climate, family SES, and peer models. In only two of these domains were variances consistently and mark-

edly lower in adoptive than in non-adoptive families. We found a 41% reduction of variance in adoptive as compared to non-adoptive families for a composite measure of parent disinhibition and an 18% reduction of variance for a composite measure of family SES. Neither of these composites, however, was related by regression to measures of delinquency, drug use, or IQ in adopted adolescents. Selection on parent disinhibition and family SES in adoptive families would affect the correlation but not the coefficient of the regression of adolescent outcomes on these variables (Aitken 1934). Consequently, the adoptive sibling correlations for delinquency, drug use, and IQ were all unaffected by correcting for range restriction on these variables. Estimates of shared environmental effects on these three variables ranged from 20% to 30% whether or not they were corrected for selection effects.

Our findings thus suggest that adoption studies, although not without their limitations, can nonetheless provide a powerful approach to the assessment of shared environmental effects on psychological outcomes. This finding is significant for several reasons. First, there is a growing recognition that traditional approaches to assessing psychosocial risk in developmental psychology have severe limitations due to confounding of genetic and environmental influences (Rutter et al. 2001). Of greatest relevance in the present context is the observation that most so-called measures of the environment are confounded with genetic variance (Plomin et al. 1994). Adoption studies provide the most direct approach to assessing psychosocial risk unconfounded by genetic effects (Plomin et al. 2000). Our findings support their continued use in this regard. Second, estimates of shared environmental effects on personality, IQ, and adolescent antisocial behavior are generally consistent across twin and adoption studies (Bouchard and McGue 2003). Consequently, a finding that adoption studies underestimate shared environmental effects by a factor of two or more would necessarily mean that findings from twin studies should also be called into question. Our results indicate, however, that questioning in this regard is not necessary, as we find no evidence of substantial negative bias in shared environmental effects from adoption studies.

The findings of our study should be interpreted within the context of several limitations to our research design. First, our evaluation of selection effects has focused exclusively on those factors that differentiate the adoptive and non-adoptive families that participated in SIBS. As Stoolmiller (1999) has noted, selection effects are likely also to affect who does and does not participate in a research study (Rosenthal

and Rosnow 1975). We attempted to rule out volunteer effects by interviewing non-participants and found that non-participants differed minimally from participants in both the adoptive and non-adoptive family samples. Nonetheless, we were unable to interview 27% of the non-participating families and those families may differ systematically from both the participating and the non-participating but interviewed families. Comparison of educational attainment in our sample of non-adoptive parents with a similarly ascertained sample from Census 2000 further supports the conclusion of minimal sampling bias. It is important to recognize, however, that imposition of our inclusion criteria likely results in a sample of parents that is positively selected for education and SES relative to the general population of parents, albeit to the same degree in both types of parents. That is, families were recruited only if the two siblings in the families shared both parents. Although SIBS parents were not required to be married, and some of our parents were not, they were required to have a sufficiently stable relationship to have had two children, either through adoption placement or birth. This requirement no doubt selects against parents with short-term and dysfunctional relationships that result in only one child. Nonetheless, volunteering to participate in a research study (Krueger et al. 2001) and having a dysfunctional relationship (McGue and Lykken 1992; Spotts et al. 2004) are not likely to be entirely environmentally determined, so that selection on these variables would, to some indeterminate degree, result in restriction in both genetic and environmental variance. In any case, while it would be theoretically useful to also control statistically for selection effects in the biological family sample through application of the Pearson–Lawley formulae, it is practically difficult to do so in the current sample. Application of the Pearson–Lawley formulae requires information on the variance of the selection variable in the unselected population, which for parental disinhibition we do not have.

A second limitation of our study is that there may be environmental factors we have not measured but which do show selection effects and are related to offspring functioning in both adoptive and non-adoptive families. Child maltreatment and neglect is related to child outcome (Cicchetti 1996) and would be a likely candidate for selection effects. We suspect that child maltreatment is underrepresented in many developmental studies, behavioral genetic or otherwise, and it is for this reason that Scarr (1992) has characterized behavioral genetic research as applying to the broad middle class of parents. We agree with her characterization. Finally, census data indicates that parents from

Minnesota are more likely to complete a college degree than parents from other U.S. states.

Some may view our results as surprising given the screening prospective adoptive parents presumably undergo before a child is placed in their home. Conversations we have had with social workers at the agencies we have worked with suggest that adoption practices have changed over the past generation, at least in Minnesota. At these agencies, adoptive parents must: demonstrate a commitment to raising a child, meet a modest minimal income level, undergo a criminal check, although existence of a criminal record does not necessarily preclude placement, and meet age and, in placements from some foreign countries, weight standards. Beyond this, social workers state that they do not feel they have the moral authority to decide who can and cannot become a parent. These agencies did not explicitly screen for mental health, marital stability, wealth and academic achievement. The restriction in range that does exist in adoptive families may be more self-imposed than agency-imposed.

Summary

Analysis of multiple environmental indicators in 409 adoptive and 208 non-adoptive families indicated that adoptive families were most strongly selected on indicators of parent disinhibitory psychopathology and family SES. We did not, however, find any evidence that these measures were associated with adopted adolescent delinquency, drug use, and IQ. Consequently, the range restriction we observed did not appear to attenuate adopted sibling correlations and thus did not lead to underestimation of shared environmental effects in studies of adoptive families.

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