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# **The Role of Parenting in the Prediction of Criminal Involvement: Findings From a Nationally Representative Sample of Youth and a Sample of Adopted Youth**

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The role of parenting in the development of antisocial behaviors, including crime and delinquency, has been the source of a long line of empirical research. Findings from this body of scholarship consistently link various dimensions of parenting to variation in virtually every measure of criminal and delinquent involvement, ranging from drug use and petty theft (Baumrind, 1991; Chilcoat & Anthony, 1996; Hoeve et al., 2008) to serious violent offending (Chung & Steinberg, 2006). This association between parenting and antisocial behaviors is so consistent that it is typically accepted without serious consideration that the findings could be the result of a methodological artifact. In recent decades, however, a growing number of critics have emerged to argue that parental and family socialization play a more limited role in the development of offspring's behavior and personality (Cohen, 1999; Harris, 1995; Pinker, 2002; Rowe, 1994).

Despite this highly controversial critique of the parental socialization literature, to date there has not been much empirical research fully addressing these critiques. Perhaps nowhere is this truer than in studies designed to identify the etiology of delinquency, crime, and other antisocial outcomes. As a result, there remains much confusion regarding the magnitude of the true effect of parenting on the development of antisocial behaviors. The current study is designed to shed additional light on this issue by examining the influence of maternal and paternal socialization on criminal justice outcomes using a research design capable of providing relatively unbiased estimates of parental influence.

## **The Genetic Confounding Critique of the Parental Socialization Literature**

The most prominent critiques of the parental socialization literature focus on the methodological designs that are typically used to estimate parental influences. Specifically, critics argue that almost every study examining the influence of parenting on child outcomes fails to control for genetic confounding (Harris, 1998; Rowe, 1994). Given that personality and behaviors are under substantial genetic influence (Ferguson, 2010; Loehlin, McCrae, Costa, & John, 1998), and given that parenting practices are also under genetic influence (Kendler & Baker, 2006; McGuire, 2009), it stands to reason that shared genetic influences could be driving the association between parenting and examined child

outcomes. Although not always formally stated, this problem of genetic confounding is known to stem directly from what is known as a passive gene–environment correlation (Plomin, DeFries, & Loehlin, 1977; Scarr & McCartney, 1983).

Passive gene–environment correlation is grounded in the fact that offspring inherit two sets of influences from their biological parents: genes and a rearing environment. Because both sets of influences stem directly from the child’s biological parents, they are likely to be correlated. For example, a parent who is aggressive and violent is likely to pass along a genetic propensity for his or her child to be aggressive and violent. Moreover, aggressive and violent parents have been found to be more abusive, less engaging, and more withdrawn (Conger, Neppl, Kim, & Scaramella, 2003). The end result is that the rearing environment is not orthogonal with the parent’s genes or with the child’s genes; rather, the rearing environment is a partial extension of the parent’s genetic makeup. Passive gene–environment correlation represents a serious threat to parental socialization research, because if genetic influences are not directly modeled, then any significant association between parenting and a given child outcome could be the result of genetic confounding (Cleveland, Beekman, & Zheng, 2011).

There is not much empirical research directly examining the associations among genetic influences, parenting, and child antisocial outcomes. The limited studies that do exist, however, tend to provide at least partial support in favor of the critique of the parenting literature (Harris, 2000, 2009). For example, Wright and Beaver (2005) examined the association between five measures of parental socialization and levels of self-control in kindergarten and first-grade students drawn from the Early Childhood Longitudinal Survey, Kindergarten Class (ECLS-K; Tourangeau et al., 2009). The results of their statistical models that did not include controls for genetic influences revealed that more than one half of the parenting measures examined were statistically significant predictors of variation in levels of self-control. After accounting for genetic influences, the effects of these parenting measures dropped from statistical significance. In another study, which employed a sample of twins from the National Longitudinal Study of Adolescent to Adult Health (Add Health; Harris et al., 2003), Wright and his colleagues (2008) analyzed the association between parenting measures and levels of self-control, associating with delinquent peers, and delinquent and criminal involvement. Once again, the results revealed that after genetic influences were held constant, the effects of parenting on these antisocial outcomes were negligible. Harden et al. (2007) analyzed the association between marital conflict and conduct problems using the children-of-twins design in a sample of Australian twins. Their results revealed that the association between marital conflict and conduct problems was the result of genetic mediation—that is, genetic influences on both marital conflict and conduct problems were driving any observed association.

Taken together, the previous research on parenting and offspring antisocial behavior provide partial support for the argument that the connection between parenting and child outcomes is confounded by those genetic influences that remain unmeasured in most studies (but see Harold et al., 2013). As with all bodies of research, though, these studies possess limitations that have yet to be resolved in the extant literature. The key limitation cutting across all of these studies is that they analyze samples of twins, and thus use traditional twin-based methodologies or extensions of them. Given that one of the main concerns with behavior genetic research is the heavy reliance on twins (Levy et al., 1996; Moilanen & Ebeling, 1998; Neale & Cardon, 1992; Petersen et al., 2011), there is a need to triangulate findings from twin-based studies with findings from studies that use additional research designs. When it comes

to assessing the role of parenting, however, there are limited alternatives to twin-based research designs. One such design that can be used is the adoption-based research design.

The adoption-based research design provides a straightforward way to estimate environmental effects on child outcomes while controlling for genetic confounding. With the adoption-based research design, adopted-away children can be compared to their biological and adoptive parents. If the child was adopted early in life and had little or no contact with their biological parents, the only reason they should resemble their biological parents is because of shared genetic material. In contrast, if the child is affected by the environment (e.g., parenting) provided by their adoptive parents (who share no genetic material with the child), this effect would be unaffected by genetic confounding. Note that genetic effects are eliminated without actually having to measure specific genetic influences or having to eliminate them statistically; the very nature of this research design removes genetic influences prior to estimating the effects of parenting influences. The adoption-based research design, in short, represents one of the most powerful research designs that can be used to effectively partition genetic and environmental effects from one another (Beaver et al., 2011; Natsuaki et al., 2013; Rutter, 2006; Rutter et al., 2001), making it an appropriate alternative to the twin-based methodology in assessing critiques of the parenting literature.

The current study adds to the literature on the parenting–child outcomes nexus by examining the role of parenting in the prediction of criminal behavior. To address the criticisms of parenting effects, we included an analysis of adoptees drawn from the Add Health study (Harris et al., 2003). Any significant parenting influences can not be attributed to genetic confounding, a point we consider important. As a result, this study represents a rigorous examination of the influence that parenting has on criminal involvement and, to our knowledge, is the first adoption-based study using contemporary data to examine the influence of parenting on criminal justice outcomes within a sample of adoptees.

## **Method**

### *Participants*

Data for this study were drawn from the Add Health study (Harris et al., 2003). Detailed information about the sample, including the sampling design, have been published previously (Harris et al., 2003; Resnick et al., 1997). Briefly, the Add Health is a four-wave prospective study of a nationally representative sample of American youth who were attending middle or high school during the 1994–1995 academic year. Initial data were collected when approximately 90,000 students at 132 schools completed self-report surveys (in-school surveys; Wave 1). Follow-up interviews were conducted with 20,745 youth and their primary caregivers (usually their mothers) to collect more detailed information about a wide range of topics (Wave 1 in-home surveys). The second round of in-home interviews was completed approximately 1.5 years later when 14,738 of the sample participated in the study. The third wave of data was collected between 2001 and 2002 when most of the 15,197 participants were young adults. The last wave of data was collected between 2007 and 2008. A total of 15,701 subjects ranging between the ages of 24 and 32 years were included in this wave of the study.

Embedded within the nationally representative sample of youth is a sample of adoptees. At Wave 1, respondents were asked whether they were adopted. In addition, respondents were also asked whether

they currently resided with either of their biological parents. This item was used to help distinguish between youth who lived with one biological parent and were adopted by a stepparent or guardian from those youth who lived with two adoptive (nonbiological) parents. The final analytical sample of adoptees consisted of (a) youth who indicated that they were adopted and (b) youth who indicated that they did not live with either of their biological parents or relatives. Although we were unable to identify the specific age at which the adoption occurred, the final analytic sample consisted of respondents who indicated that they were adopted and lived with adoptive parents who were not related to them. This same strategy has been used previously by researchers analyzing the adoptive subsample of the Add Health study (Beaver, 2011).

### Materials and Procedure

A total of four criminal involvement measures were included in the analyses. At Wave 4, respondents were asked questions about their lifetime involvement in the criminal justice system. Specifically, they were asked whether they had ever been (a) arrested, (b) incarcerated, (c) sentenced to probation, and (d) arrested multiple times. All of these responses were coded dichotomously (0 = *no*, 1 = *yes*) and have been used in previous studies analyzing the Add Health (Barnes, 2013; Miller & Barnes, 2013; Schwartz & Beaver, 2014). Table 1 contains descriptive statistics for all of the criminal involvement measures we used, as well as the other variables and scales included in the analyses.

Table 1  
*Mean, Standard Deviation, and Variance for Study Measures*

	Nonadoptee sample			Adoptee sample			<i>F</i> statistic
	Mean	<i>SD</i>	Variance	Mean	<i>SD</i>	Variance	
<b>Parenting variables</b>							
Maternal disengagement	8.99	3.48	12.11	9.37	3.75	14.03	9.30**
Maternal attachment	9.40	1.11	1.24	9.23	1.26	1.58	19.03**
Maternal involvement	3.98	1.99	3.95	4.11	2.12	4.48	6.57**
Maternal education	5.10	2.37	5.64	5.38	2.56	6.54	12.37**
Paternal attachment	9.00	1.44	2.07	9.07	1.46	2.12	.01
Paternal involvement	2.95	1.96	3.86	3.19	2.22	4.94	18.83**
Paternal education	5.33	2.46	6.08	5.54	2.58	6.65	2.00
Parental permissiveness	5.17	1.56	2.44	5.18	1.49	2.22	1.00
<b>Criminal justice system involvement</b>							
Arrested	.26	.44	—	.37	.48	—	—
Incarcerated	.14	.34	—	.20	.40	—	—
Sentenced to probation	.09	.29	—	.17	.38	—	—
Multiple arrests	.12	.33	—	.19	.39	—	—
<b>Control variables</b>							
Age	16.12	1.69	2.86	16.24	1.45	2.72	.75
Gender	.48	.50	—	.53	.50	—	—
Caucasian	.61	.49	—	.66	.47	—	—
African-American	.24	.42	—	.19	.39	—	—
Other race	.15	.36	—	.15	.36	—	—
Neighborhood disadvantage	4.61	1.54	2.37	4.48	1.48	2.18	2.29

*Note.* Reported *F* statistics are the results of Levene's tests of the equality of variances.  
\*\*  $p \leq .01$ .

### *Mean, Standard Deviation, and Variance for Study Measures*

A total of eight parenting measures, all drawn from Wave-1 data, were included in the analyses. The first four parenting scales—maternal disengagement, maternal attachment, maternal involvement, and maternal education—pertain directly to the mothers of the subjects. Maternal disengagement is a five-

item scale that consists of responses to questions pertaining to the interaction between each respondent and his or her mother. Higher values represented greater levels of maternal disengagement ( $\alpha = .72$ ). It is important to note, this same scale had been used previously (Beaver, 2008). Maternal attachment is a two-item scale that consists of adolescent responses to two questions tapping how close they feel to their mothers and how much they think their mothers care about them. This scale had been used previously (Schreck, Fisher, & Miller, 2004); higher values represent greater levels of maternal attachment ( $\alpha = .70$ ). The maternal involvement index, which is similar to previously used indexes (Crosnoe & Elder, 2004), was created from 10 questions asked to the youth. Higher scores on this index represented greater maternal involvement ( $\alpha = .60$ ). The last maternal socialization item was created from a single-item measure that tapped the mother's highest level of education. Higher values on this item represented higher levels of education.

The next three parenting scales—paternal attachment, paternal involvement, and paternal education—pertain directly to the fathers of the subjects. The paternal attachment ( $\alpha = .77$ ) and paternal involvement ( $\alpha = .65$ ) scales were measured the same way the maternal attachment and involvement scales were measured. Higher values on both scales indicated greater levels of paternal attachment and involvement. The third paternal socialization item measured the father's highest level of education. Higher values indexed greater levels of paternal education.

Last, a parental permissiveness scale that applied to both mothers and fathers was included in the analyses. This scale, which had been used previously (Barnes & Morris, 2012), taps the amount of monitoring, supervision, and autonomy provided to the respondents. At Wave 1, participants were asked seven questions related to whether their parents allowed them to make their own decisions about watching TV, their choices of friends, and their bedtimes. All responses were coded dichotomously (0 = *no*, 1 = *yes*) and this summated index is coded such that higher values represented greater levels of parental permissiveness ( $\alpha = .58$ ).

Four control variables were included in the analyses: age, gender, race, and neighborhood disadvantage. Age was measured in years (at Wave 1) and gender was coded as a dichotomous dummy variable (0 = *female*, 1 = *male*). Race was entered into the analyses as a series of dummy variables, wherein African American, other race, and Caucasian were coded dichotomously. A three-item neighborhood-disadvantage scale created from three questions asked to the primary caregivers during Wave 1 interviews was also included in the analyses. Responses to these items were summed together where higher values represented greater levels of neighborhood disadvantage ( $\alpha = .65$ ).

## Results

Prior to estimating the statistical models examining the association between each of the parenting measures and the included outcomes, a series of diagnostics were estimated for the parenting measures included in the current study. First, in an effort to ensure that an adequate amount of variation existed within each parenting measure, the variance of each measure was estimated for the sample of nonadoptees and the adoptee sample. Second, in an effort to determine whether the observed variances in each of the two subsamples were equivalent, Levene's tests for the equality of variances were estimated. The resulting *F* statistics are reported in Table 1, along with the variances for each of the parenting measures. The results revealed that many of the parenting measures possessed

significantly different variances between the two examined groups. However, for all measures with significantly different variances between the two examined groups, the adoptee sample possessed significantly greater variances, suggesting greater levels of dispersion within the adoptee subsample than within the nonadoptee sample. This increased variance would lead to a situation in which it is more likely to detect significant parenting influences on the adoptee sample, as opposed to on the nonadoptee sample.

Table 2 presents the results from a series of binary logistic regression models in which parenting measures were used to predict criminal justice outcomes at Wave 4 for the nonadoptee sample. As can be seen, several of the parenting measures were significantly associated with odds of being arrested, being incarcerated, being on probation, or being arrested multiple times. Maternal disengagement was positively associated with the probability for all of the outcome measures, whereas paternal attachment was negatively associated with the probability for all of the outcome measures. Moreover, maternal education was significantly and negatively associated with the probability of ever being arrested, incarcerated, and placed on probation. Lower levels of paternal education predicted the odds of being arrested multiple times.

Table 2  
*Parenting Predicting Criminal Justice Outcomes at Wave IV for the Nonadoptee Sample*

	Ever arrested		Ever incarcerated		Ever on probation		Multiple arrests	
	OR	SE	OR	SE	OR	SE	OR	SE
<b>Parenting variables</b>								
Maternal disengagement	1.04**	.01	1.05**	.02	1.04*	.02	1.05**	.02
Maternal attachment	.94	.04	.97	.05	1.00	.06	.94	.05
Maternal involvement	.99	.02	.92*	.03	.97	.03	.98	.03
Maternal education	.94**	.02	.94**	.02	.94**	.02	.97	.02
Paternal attachment	.93**	.03	.92*	.03	.89**	.04	.93*	.04
Paternal involvement	.98	.02	1.02	.03	1.04	.03	.99	.03
Paternal education	.98	.02	.97	.02	.97	.02	.95*	.02
Paternal permissiveness	1.02	.02	.96	.03	.98	.03	.97	.03
<b>Control variables</b>								
Age	.89**	.02	.94*	.03	.95	.03	.90**	.03
Gender	4.11**	.29	4.38**	.43	4.05**	.45	5.85**	.64
African-American	1.33**	.12	1.29*	.15	1.18	.16	1.39**	.17
Other race	.72**	.07	.82	.11	.78	.12	.80	.11
Neighborhood disadvantage	1.07**	.03	1.13**	.03	1.06 <sup>†</sup>	.04	1.09**	.03
<i>N</i>	5816		5831		5703		5836	

<sup>†</sup>  $p \leq .10$ . \*  $p \leq .05$ . \*\*  $p \leq .01$ .

*Parenting Predicting Criminal Justice Outcomes at Wave IV for the Nonadoptee Sample*

Table 3 shows the association between parenting and the criminal justice outcomes for the adoption sample, which we were able to control for genetic confounding. In these equations, none of the parenting measures were significantly associated with being arrested, being incarcerated, being on probation, or being arrested more than once.

Table 3  
*Parenting Predicting Criminal Justice Outcomes at Wave IV for the Adoptee Sample*

	Ever arrested		Ever incarcerated		Ever on probation		Multiple arrests	
	<i>OR</i>	<i>SE</i>	<i>OR</i>	<i>SE</i>	<i>OR</i>	<i>SE</i>	<i>OR</i>	<i>SE</i>
Parenting variables								
Maternal disengagement	1.04	.05	1.03	.06	.92	.06	1.00	.06
Maternal attachment	1.04	.21	1.03	.24	.77	.19	.83	.20
Maternal involvement	1.13	.12	.93	.11	.99	.12	1.21	.16
Maternal education	.93	.07	.96	.08	.99	.09	.91	.08
Paternal attachment	.95	.12	1.03	.17	.82	.12	.84	.12
Paternal involvement	.83	.08	.98	.10	.89	.10	.86	.10
Paternal education	1.05	.07	.96	.08	1.12	.10	.95	.08
Paternal permissiveness	1.06	.12	.88	.11	.93	.13	.93	.13
Control variables								
Age	.96	.10	1.08	.13	1.05	.13	1.03	.13
Gender	3.45**	1.08	2.92**	1.03	3.70**	1.43	3.56**	1.39
African-American	2.70*	1.14	1.64	.76	2.36	1.11	1.84	.92
Other race	.81	.35	1.01	.50	1.24	.64	1.69	.82
Neighborhood disadvantage	1.17	.13	1.08	.14	1.20	.15	1.07	.14
<i>N</i>	240		240		234		240	

\*  $p \leq .05$ . \*\*  $p \leq .01$ .

*Parenting Predicting Criminal Justice Outcomes at Wave IV for the Adoptee Sample*

The slightly different pattern of significant effects for the parenting measures between the nonadoptee and adoptee samples (Tables 2 and 3) is likely driven, at least in part, by sample sizes and thus statistical power. The sample sizes for the nonadoptee sample, for example, ranged between  $N = 5703$  and  $5836$ , whereas the sample sizes for the adoptee sample ranged between  $N = 234$  and  $240$ . As a result, it was not unexpected from a statistical perspective to observe a greater number of statistically significant effects for the nonadoptee sample. To more fully address this issue and to determine whether the effect sizes were statistically different, the nonadoptee and adoptee samples were combined and interaction terms were created between each of the parenting measures and a dichotomous variable indicating adoptee status ( $0 = nonadoptee$  and  $1 = adoptee$ ). The interaction term was then entered into separate equations (i.e., one interaction term at a time for each parenting measure and for each outcome separately) to determine whether the effect sizes were different. The results of these analyses revealed only two significant differences (which is about what would be expected by chance alone): one for the effect of maternal education on probation and one for the effect of paternal education on probation.

## Discussion

Analysis of adoptees from the Add Health data in the current study revealed that, once genetic confounds were appropriately controlled, the effects of parenting on the criminal justice outcome measures were nonsignificant. If these findings are to be believed, they pose a significant challenge to research that estimates parenting effects. To begin with—and in line with previous arguments (Wright & Beaver, 2005)—parenting effects may be upwardly biased due to genetic confounding. For the current study, we were not able to fully address this possibility, however, because the parenting items from the nonadoptee sample (i.e., the sample for which genetic confounding could not be controlled for) had small and largely nonsignificant influences on the outcome measures.



This latter result of nonsignificant parenting effects on criminal involvement in the nongenetically informative analyses was largely unexpected. Most previous research using a similar methodological design has reported statistically significant associations between a wide array of parenting measures and an assortment of antisocial outcomes, including delinquent involvement (e.g., Farrington, 1995; Farrington & Welsh, 2007; Haapasalo & Tremblay, 1994; Hawkins et al., 1998; Huesmann, Dubow, Eron, & Boxer, 2006; Loeber & Stouthamer-Loeber, 1986). As a result, we are left to speculate as to why the findings of our study are somewhat disparate with those previously published. Although not exhaustive, we offer three potential explanations. First, although studies consistently find statistically significant associations between parenting and delinquency/antisocial behaviors, these effects tend to be relatively small in magnitude, accounting, on average, for only about 11% of the overall variance in the examined outcomes (Hoeve et al., 2009). What this necessarily means is that these relatively small effect sizes may be at risk for falling from statistical significance based on sample-specific characteristics or differences in measurement. For example, previous researchers have often analyzed data drawn from high-risk samples (e.g., Cambridge Study in Delinquent Development; Farrington & West, 1990), whereas, for this study, we analyzed data from a nationally representative sample of youth. These differences, of course, result in quite different distributional properties for parenting measures and for criminal justice outcome measures, which, in turn, can result in differences in the ability to detect statistically significant associations (Wright & Beaver, 2012). Though there are any number of differences that might account for the different pattern of findings for the parenting measures, whatever the reason, the fact that we did not detect consistent parenting effects on any of the criminal involvement measures should not be completely unexpected, given the relatively small effects of parenting measures on delinquency.

Second, the current study focused on the effects of parenting during adolescence on later life crime and delinquency. Previous research has revealed that parenting likely has its strongest influence early in the life course. By adolescence, any influence of parents likely is replaced by other social institutions, particularly peers (Harris, 1998). Given that the Add Health data did not include parenting data collected during childhood, it remains impossible to determine whether early life parenting would have had a detectable effect on the crime measures employed in the current analysis. Third, to assess the potential association between parenting and criminal involvement, we used a longitudinal research design, spanning approximately 14 years. Stated differently, the parenting measures were assessed when the youth were in middle or high school and the criminal involvement measures were drawn from interviews with the respondents when they were between the ages of 24 and 32 years of age. Given that associations tend to become attenuated as the time lag between waves increases (Wooldridge, 2010), perhaps this long time lag was responsible for producing nonsignificant associations between parenting and criminal involvement. Future research would benefit from a closer examination of these (and other) explanations of the inconsistent effect of parenting on criminal involvement in the current study.

Nonetheless, we urge researchers who focus on estimating the impact of parenting on child behaviors and traits to employ a research design that is capable of controlling for genetic influences. Fortunately, there are an array of samples and analytic techniques available that can be used to control for genetic confounding when the influence of parenting is estimated. What is particularly advantageous about these genetically sensitive research designs is that, if genetic confounding is not an issue, such designs will not result in biased findings, and if genetic confounding is an issue, then these research designs will result in more accurate findings. The widely used research designs to test parenting effects, however,

will only produce unbiased results when genetic confounding is not an issue, a highly untenable possibility based on the existing literature (Cohen, 1999; Harris, 1995, 1998; Rowe, 1994; Pinker, 2002; Wright & Beaver, 2005).

Caution should be exercised when reviewing the findings of the current study as a result of a number of limitations that need to be addressed in future studies. First, the behavioral outcome measures and the parenting variables were based on self-reports. As a result, the predictor variables and the outcome measures were affected by shared-methods variance that could have biased the results. Typically, though, shared-methods variance is viewed as a problem that upwardly biases covariances among the variables under investigation. In the current study, there were so few significant associations between the parenting variables and outcome variables that this problem is not likely affecting the parameter estimates in typical ways. Second, because adoptees were the focus of the current study, the extent to which these findings might be applicable to nonadoptees remains unknown. It is important to note, though, that children from other types of behavior–genetic research designs, such as twins, have been shown not to be significantly different from children from the larger population (Barnes & Boutwell, 2013). Future researchers would benefit by more fully addressing this possibility with adoptees. Third, whether the parent–child dynamics within adopted families are similar to those within nonadopted families could not be explored in the current study. In addition, due to limitations of the Add Health study, we were unable to examine whether the timing of adoption (i.e., what age the child was adopted) had an impact on the link between parenting and criminal involvement. For now, all that our study was able to reveal was that, in a sample of adoptees, widely used parenting variables that have previously been found to influence antisocial outcomes had no consistent effects on variation in criminal involvement. Whether this general pattern of results would be different after addressing these limitations remains an open empirical question.

#### Footnotes

<sup>1</sup> As a sensitivity check on the robustness of these results, we also employed the monozygotic (MZ) difference-scores method to examine the potential association between the parenting items and the criminal justice outcome measures. This approach allowed us to hold all genetic effects constant (as well as shared environmental influences), and thus any significant effect of the parenting items would not be attributable to genetic confounding. The results of these models revealed results that were directly in line with those of the adoptee sample—that is, none of the parenting effects were statistically significant predictors of any of the criminal justice outcome measures.

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