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A Demonstration of the Generalizability of Twin-based Research on Antisocial Behavior

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Abstract

Researchers typically analyze samples of twin pairs in order to decompose trait variance into genetic and environmental components. This methodological technique, referred to as twin-based research, rests on several assumptions that must be satisfied in order to produce unbiased results. While research has analyzed the tenability of certain assumptions such as equal environments, less attention has been given to whether results gleaned from samples of twins generalize to the broader population of non-twins. The current study analyzed data drawn from the National Longitudinal Study of Adolescent Health and findings suggested twins do not systematically differ from the general population of non-twins on many measures of behavior and development. Furthermore, the *effects* of specific covariates on measures of antisocial behavior did not appear to differ across twin status. In sum, evidence concerning the etiology of antisocial behavior (e.g., heritability estimates) gleaned from twin-based research is likely to generalize to the non-twin population.

Keywords

twin research; assumptions; limitations; generalizability; external validity

Over the past century, one of the primary goals of science has been to answer the question of why humans vary so widely on most phenotypes. Not surprisingly, a wealth of studies cutting across multiple disciplines has underscored the complex nature of human development. What research in this area has unequivocally demonstrated is that variation in nearly every measurable phenotype—ranging from disease (Rutter, 2006), psychopathology (Raine, 1993), personality traits (Bouchard and Loehlin, 2001), and violent behaviors (Burt, 2009; Ferguson and Beaver, 2009; Guo et al., 2008)—is accounted for by a complex architecture of both genetic and environmental factors (Carey, 2003).

Twin (and sibling) studies, by a considerable margin, represent the most commonly utilized strategy for scholars interested in examining the different sources of variation in human phenotypes (Cleveland et al., 2011). Like all analytical techniques, twin research rests on certain assumptions that when left unsatisfied may challenge the validity of the results (Plomin et al., 2013). Many of the fundamental assumptions of twin research (i.e., no assortative mating and equal environments) have been scrutinized relatively closely

(Borkenau et al., 2002; Boutwell et al., 2012; Cronk et al., 2002; Derks et al., 2006; Eaves et al., 2003; Klump et al., 2000; Krueger et al., 1998; Plomin and Bergeman, 1991; Raine, 2002). What remains a concern is the extent to which findings from twin research can (or should) be generalized to a broader population of singletons (e.g., Petersen et al., 2011).

At least some existing evidence suggests certain outcomes such as parental attachment and peer affiliation (Beaver, 2008), cognitive abilities (Christensen et al., 2006; Tsou et al., 2008), and personality traits (Johnson et al., 2002) are similar across twins and singletons. While these findings are important, they do not fully address the question of whether twins differ from the general population of non-twins on traits that are of interest to researchers studying the correlates of antisocial behavior. Less research, for instance, has considered whether twins differ on behavioral outcomes such as involvement in delinquency, drug use, and victimization likelihood (Levy et al., 1996; Pulkkinen et al., 2003; Robbers et al., 2010; van den Oord et al., 1995). Moreover, relatively nothing is known regarding differences between twins and non-twins for outcomes such as peer affiliation, a variable known to correlate consistently with delinquent and antisocial behaviors (Haynie, 2001).

Though limited evidence speaks directly to twin-singleton similarity on delinquent behavior, drug-using behaviors, and victimization, a body of research has observed twin-singleton comparability on various instruments intended to tap psychopathology (Rutter and Redshaw, 1991). Scholars have long noted certain biological/neurological developmental delays (particularly in language development) are associated with twin status (e.g., Shinwell et al., 2009; Voracek and Haubner, 2008). In their review, Rutter and Redshaw (1991) concluded that socio-emotional adjustment levels were similar across twins and non-twins. van den Oord and colleagues (1995) reported that problem behavior in twin children was comparable to behaviors observed in singletons. Pulkkinen et al. (2003) reported twin differences for certain personality traits but not for externalizing problem behaviors nor for internalizing problem behaviors. More recently, Robbers et al. (2010) suggested that the trajectories of externalizing behavioral problems of twins were similar to those of singletons. Differences did emerge, however, when trajectories of internalizing behavioral problems were analyzed (see also Kendler et al., 1995). Despite some attention from scholars, research has yet to fully unpack whether twins differ systematically from non-twin members of the population for a variety of variables central to research in disciplines including psychology, sociology, and criminology.

THE CURRENT STUDY

In the current study, we extend the available literature concerning the generalizability of twin research to the broader population of non-twins (i.e., singletons) by examining the subsample of twins contained in the Add Health data (Beaver, 2008; Jacobsen and Rowe, 1998). Our analyses are intended to explore the comparability of twins and non-twins on a host of developmental, cognitive, and behavioral phenotypes that are of interest to a wide range of scientists. It is important to point out that two prior studies have used the Add Health data to explore twin/non-twin similarity. Beaver (2008) analyzed the twin subsample to determine whether MZ twins were similar to the general population of non-twins on some of the measures utilized in the current study. Note, however, that Beaver only analyzed MZ twins and did not include DZ twins in the twin sample. The current study will extend Beaver's work by including both MZ and DZ twins in the twin sample. Jacobsen and Rowe (1998) also presented an analysis of twin/non-twin differences. Their analysis was limited, however, in that they only examined demographic characteristics. Last, neither Beaver (2008) nor Jacobsen and Rowe (1998) analyzed the possibility that, while mean differences might not emerge across twins and non-twins, the *effects* of the covariates might vary across

twin status (i.e., an interaction effect with twin status acting as the moderator variable). These possibilities are considered in the current study.

METHODS

DATA

The data for this study were drawn from the restricted use wave 1 sample of the National Longitudinal Study of Adolescent Health (Add Health; Harris, 2009). The Add Health data and sampling strategies have been described at length elsewhere (see Harris et al., 2011; Kelley and Peterson, 1997). Briefly, the Add Health is a longitudinal and nationally representative sample of American adolescents who were enrolled in middle school or high school during the mid 1990s. Sampling began at the school-level, wherein approximately 130 schools across the nation were identified and all students attending these schools (90,000) were administered a self-report questionnaire during a designated class session. This original cohort (i.e., the in-school survey) was utilized as the sampling frame from which a sample of roughly 21,000 youth were targeted and asked to complete a more extensive in-home interview. The participant's primary caregiver was also asked to complete an interview ($n \approx 17,000$). Importantly, youth were asked a range of questions tapping their social behaviors, their health behaviors, their attitudes, their personality characteristics, and their involvement in crime, drugs, and other antisocial behavior. Respondents ranged between 11 and 21 years of age at wave 1.

One important feature of the Add Health data is that a portion of the in-home interviews were completed with a subsample of twins. To be specific, 1,479 individuals were identified and confirmed to be part of a twin pair during wave 1 interviews.¹ Twins were administered the exact same questionnaire as the other participants, allowing for a comparison between twins and non-twins on a range of outcome variables. In order to make these comparisons, however, it was important to ensure that the two groups (i.e., twins and non-twins) were mutually exclusive, meaning that the sampling strategy followed by the Add Health design team was critical. As noted by Kelley and Peterson (1997), twins entered into the sample with certainty. Thus, any respondent who self-identified him/herself as a twin during the in-school survey was automatically added to the in-home sample. This design fact allows for a reliable comparison between twins and non-twins in the Add Health data. Specifically, the risk that twins actually appear in the non-twin sample (therefore biasing any comparison across groups) was controlled as part of the sample design. As will be discussed below, the appropriate sampling weights and cluster variables were utilized in the analysis. Some respondents were missing information for one or more of the weight/cluster variables ($n = 266$ twins; $n = 1,597$ non-twins). After eliminating cases with missing sampling weight or cluster information, the analytic sample size was 17,711 non-twins and 1,213 twins.²

¹There are at least three ways to identify twins in the Add Health data: 1) use the "twin sample flag"; 2) use information gleaned from question 23 in the school interview ("Are you a twin?"); or 3) use the "pairs" data. We elected to utilize the pairs data to identify twins because these respondents are confirmed to have been part of a twin pair by the Add Health researchers. As a sensitivity analysis, however, we combined information from the twin sample flag with information gleaned from the pairs data. Specifically, we used the twin sample flag variable as the primary twin identifier but anyone who was uniquely identified as a twin in the pairs data was also coded as a twin in the analysis. This coding scheme identified a larger number of twins than any one measure alone and a cross-tab between the twin sample flag and question 23 from the school survey resulted in only 14 cases appearing in the off-diagonals (suggesting a reporting error on part of the respondent during the in-school surveys). When this alternative twin identification scheme was utilized in the analysis, the overall pattern of findings was substantively similar to those presented in the text and tables. Any difference in findings that emerged when using this alternative twin identification scheme is noted in the paper.

²Due to the oversampling of twins, the weighted results are reported here. Differences that emerged when analyzing unweighted cases (i.e., including cases with missing weights/cluster variables) are noted in the paper.

MEASURES

This section presents an overview of each of the variables/scales utilized in the analysis. All variables/scales were generated using wave 1 data.

Behavioral Outcomes

Delinquency: Respondents were asked whether and how often they had engaged in 19 different delinquent activities over the past 12 months. Specifically, respondents were asked whether they had painted graffiti, damaged property, lied to their parents, stolen from a store, gotten into a serious fight, hurt someone badly enough to require medical attention, run away from home, stolen a car, stolen something worth more than \$50, broken into a house, committed an armed robbery, sold drugs, stolen something worth less than \$50, taken part in a group fight, acted loud or unruly in a public place, carried a weapon to school, used a weapon in a fight, pulled a knife or gun on someone, and shot or stabbed someone. Most items were coded so that 0 = never, 1 = 1 or 2 times, 2 = 3 or 4 times, and 3 = 5 or more times. Two items (i.e., carried a weapon to school and used a weapon in a fight) were coded so that 0 = no and 1 = yes. Two items (i.e., pulled a knife or gun on someone and shot or stabbed someone) were coded so that 0 = never, 1 = once, and 2 = more than once. Respondents scores on each of the 19 items were summed together to generate the delinquency scale ($\alpha = .85$).

Polydrug use: During wave 1 interviews participants were asked to indicate whether (0 = no, 1 = yes) they had used six different drugs. Specifically, respondents were asked whether they had tried cigarettes, alcohol, marijuana, cocaine, inhalants (such as glue or solvents), and any other type of drug without a doctor's prescription (e.g., LSD, PCP, ecstasy, mushrooms, speed, ice, heroin, or pills). Summing across the six items generated the polydrug use scale ($\alpha = .68$).

Victimization: Respondents were asked to indicate whether any of the following six things had happened to them over the past 12 months: they had seen someone shot or stabbed; someone pulled a knife or gun on them; someone shot them; someone cut or stabbed them; they got into a physical fight; they were jumped. Each response was coded so that 0 = never, 1 = once, and 2 = more than once. Summing across the six items generated the victimization scale ($\alpha = .70$).

Key Covariates

Low self-control: Prior scholars have identified a 23 item self-control scale available at wave 1 (Miller et al., 2011). Individual items tapped a range of concepts indicative of low self-control. For instance, respondents were asked to report on their bad temper, their ability to get along with others, whether they generally used a systematic method for judging and comparing alternatives, and whether they try to think through multiple solutions to a problem. Each item was coded so that higher values reflected lower levels of self-control when combined into a scale. Factor analysis indicated a one-factor solution best fit the data. To generate the scale, answers to each of the 23 items were summed together ($\alpha = .76$).

Depression: During wave 1 interviews respondents were asked 18 items taken from the CES-D depression scale (Radloff, 1977). Each item was coded so that higher values reflected more depression symptoms when the 18 items are combined. To generate the scale, therefore, respondents' answers to each of the 18 items were summed together ($\alpha = .86$).

Drug-using peers (self-report): Respondents' exposure to drug-using peers was tapped via the respondent's reports about his/her peers' behavior. In particular, respondents were asked

to indicate how many of their three best friends (0 = none, 1 = one, 2 = two, and 3 = three) smoked at least one cigarette per day, drank alcohol at least once per month, and used marijuana at least once per month. Each participant's answers to these three questions were combined by summing the scores so that higher values indicated more exposure to drug-using peers ($\alpha = .76$). This scale has been used previously (Beaver, 2008).

Delinquent peers (network sent and network sent & received): Prior scholars have utilized the social networking feature of the Add Health data to link the target respondent's information with his/her peers' information (Haynie, 2001; Morris and Johnson, 2011). We followed the lead of these researchers by generating two measures of peer delinquency taken directly from the peers' self reports. The first measure captured the delinquency of respondents' peers who were nominated as a friend by the target respondent. This format is referred to as the *network sent* measure. The second peer measure captured the delinquency of respondents' peers who were nominated by the respondent and those respondents reciprocated this nomination. This alternative version of the peers measure is referred to as the *network sent & received* measure. In other words, both measures utilized the same variables. The difference between the two measures lies in the peers who provided the information for these variables.

Peers were asked to report on the frequency with which they did the following over the past 12 months: smoked cigarettes; drank beer, wine, or liquor; got drunk; did something dangerous because they were dared; lied to their parents; skipped school without an excuse; and, got into a physical fight (responses to the first six items were coded so that 0 = never, 1 = once or twice, 2 = once a month or less, 3 = 2 or 3 days a month, 4 = once or twice a week, 5 = 3 to 5 days a week, and 6 = nearly every day; responses to the last item were coded so that 0 = never, 1 = one or two times, 2 = three to five times, 3 = six or seven times, and 4 = more than seven times). To generate the *network sent* scale, the responses given by peers' nominated by the target respondent were averaged together so that higher values reflected greater delinquency among the peer group ($\alpha = .80$). To generate the *network sent & received* scale, the responses given by peers' who were both nominated by the target respondent and who reciprocated this nomination were averaged together so that higher values reflected greater delinquency among the respondent's peers ($\alpha = .81$).

Network centrality: Network centrality was measured via the Bonacich centrality method (Bonacich, 1987). In short, centrality is based on the number of peers nominated by the target respondent and on the number of peers nominated by those respondents. Higher scores indicate that the respondent occupies a more central position in the peer network.

Network density: Density is a measure of the degree to which a peer network is closely tied together. In short, density reflects the number of ties in a friendship network, divided by the number of possible ties in that network. Higher scores reflected a more dense peer network (sent and received scores were analyzed).

Network popularity: Popularity was measured as the total number of peer friendship nominations the respondent received from other students in the school. Higher values reflected more friendship nominations.

Time with peers: Respondents were asked to indicate how often, within the past week, they hung out with their friends. Responses were coded so that 0 = not at all, 1 = one or two times, 2 = three or four times, and 3 = five or more times.

Attractiveness: Immediately after each interview was completed, the *interviewer* was asked several questions about the interview process and the respondent. One of these questions

asked the interviewer to rate the respondent's level of physical attractiveness on a scale of 1 (very unattractive) to 5 (very attractive).

Number of sex partners: Respondents were asked to indicate the total number of people with which they had ever had a sexual relationship. Respondents answered on a scale from 0 to 900. A clear break in the distribution occurred around 30. Thus, to limit the influence of outliers on the analytic results, all respondents reporting 30 or more sex partners were coded as 30.

Verbal IQ: At the beginning of wave 1 interviews, respondents were administered an abridged version of the Peabody Picture Vocabulary Test (PVT). The PVT is designed to tap the respondent's hearing vocabulary and was standardized by age. Higher scores reflected higher verbal IQ.

GPA: Respondents were asked to self-report their grades in four core courses (English/language arts, mathematics, history/social studies, and science). Grades were assigned the following values: A = 4, B = 3, C = 2, and D (or lower) = 1. Next, participants' grades were averaged to generate their overall GPA. Higher values (values at or close to 4.0) indicate a higher overall GPA.

Biological maturity: Questions designed to tap each respondent's (both male and female) biological maturity were included at wave 1. Males were asked four questions that tapped their physical development: underarm hair, facial hair thickness, lowering of voice, and physical development compared to other boys his age. All four questions were coded so that higher values reflected more biological maturity. Females were asked four questions that tapped their physical development: breast development, body curviness, ever menstruated, and physical development compared to other girls her age. All four questions were coded so that higher values reflected more biological maturity (Barnes and Beaver, 2010).

Three steps were followed in generating the biological maturity measure from the above items. First, responses to each of the items were standardized within sex. Second, the z-scores were summed together into a single scale. This resulted in a male biological maturity scale and female biological maturity scale. Finally, a single biological maturity scale was created by standardizing the male biological maturity scale ($\alpha = .67$) and the female biological maturity scale ($\alpha = .62$) and then combining them into one measure tapping biological maturity for both sexes.

Birth weight: During wave 1 interviews, the respondent's primary caregiver was asked to report on the target respondent's (i.e., the youth) birth weight. Reports were given in pounds and ounces where higher values reflected a higher birth weight. For the analysis, ounces were divided by 16 and then added with pounds so that the final measure reflected pounds (in whole numbers) and ounces were measured as a fraction of a pound.

Maternal age childbirth: Maternal age at childbirth was calculated by subtracting the respondent's age from his/her mother's reported age. Mothers reported on their own age in the primary caregiver interview. Caregivers who were identified as someone other than the respondent's mother were assigned a missing value resulting in a missing score for the maternal age at childbirth variable.

Parental permissiveness: Participants reported on the level of autonomy granted to them by their parents. Specifically, respondents were asked whether they were allowed to make their own decisions about curfew, about their peer group, about their clothes, about their bedtime, about their diet, about what they watched on television, and about how much television they

watched (0 = no, 1 = yes). To generate the scale, each of the items were summed together so that higher values reflected more parental permissiveness ($\alpha = .63$). This scale has been used previously (Beaver, 2008).

Maternal attachment: Maternal attachment was measured via two items. First, respondents were asked to indicate the level of closeness they felt toward their mothers (scores ranged from 1 = not at all to 5 = very much). Second, each participant was asked to report on how much they thought their mothers cared about them (scores ranged from 1 = not at all to 5 = very much). Answers to these two questions were combined by summing the two items so that higher values reflected more maternal attachment ($\alpha = .64$). This scale has been used previously (Beaver, 2008).

Maternal involvement: Respondents were asked to report whether, in the past four weeks, they had participated in any of the following 10 activities with their mother (0 = no, 1 = yes): shopped, played a sport, went to a religious service, talked about dating relationships, went to a movie, talked about personal problems, argued about the respondent's behavior, talked about school work, worked on a school project, and talked about school activities. To generate the scale, responses to the 10 items were summed so that higher values reflected more maternal involvement ($\alpha = .55$). This scale has been used previously (Beaver, 2008).

Maternal disengagement: Maternal disengagement was measured via five items asked to the respondent. The questions tapped the level of warmth expressed by the mother, the level of encouragement offered by the mother, the degree to which the mother talked to the respondent when he or she was in the wrong, the respondent's level of satisfaction with communication between him/her and mother, and the respondent's level of satisfaction with his/her relationship with mother. Each item was coded so that, when combined, higher scores reflected more maternal disengagement. To generate the scale, each of the five items were summed ($\alpha = .84$). This scale has been used previously (Beaver, 2008).

Demographics

Age: A count variable indexed the respondent's age (ranged from 11 to 21).

Sex: A dichotomous variable indexed the respondent's sex (0 = female, 1 = male).

Race: A dichotomous variable indexed the respondent's race (0 = non-Black, 1 = Black).

Urbanicity: A dichotomous variable identified respondents as living in a rural (or partly rural; coded as 0) or urban area (coded as 1). Respondents were only coded as living in an urban area if *all* citizens living in that particular Census block group were classified as living in an urban area (see Billy et al., 1998). This variable was created by the Add Health data collection team and is based on Census data from 1990.

ANALYSIS PLAN

The analysis proceeded in two steps. The first step informed the question of whether there are *mean differences* between twins and non-twins on a range of factors/characteristics. In order to address this question, mean scores on each of the variables/scales (percentages are reported for dichotomous variables) were observed across twin status (i.e., twin or non-twin). Next, a *t*-test was carried out to determine whether any of the observed mean differences were statistically significant at the $p < .05$ (two-tailed) level or the $p < .01$ (two-tailed) level (a bivariate logistic regression model provided the statistical test for differences across twin status on dichotomous variables). The .01 level is perhaps most appropriate due

to the large sample size (between 10,000 and 19,000; see sample sizes presented in the tables). Mean differences were analyzed in two ways. First, mean differences were estimated for the unadjusted variables. Second, mean differences were estimated after adjusting for the respondent's age, race, sex, and urbanicity. The unadjusted mean differences are reported in the table and text because no substantive differences emerged after controlling for respondent age, race, sex, and urbanicity.

The second step to the analysis informed the question of whether the *effects* of the different covariates on the behavioral outcomes differed between twins and non-twins. While it may be the case that twins report mean differences on some of the variables/scales, whether the *impact* of a variable on behavior is different for twins as compared to non-twins is a separate issue. In order to address this question, negative binomial regression models were estimated and the behavioral outcome variables were utilized as the dependent variables. Each model included three predictors of interest: 1) the twin status variable (0 = non-twin, 1 = twin), 2) the covariate of focus (i.e., the Key Covariate), and 3) a multiplicative interaction term between twin status and the covariate of focus (twin status*Key Covariate). It is also important to point out that the regression equations included controls for age, race, sex, and urbanicity but the coefficient estimates for these variables are not presented. Evidence to support a differential effect of a covariate on a behavioral outcome will be garnered any time the interaction term reaches statistical significance.

One final point deserves attention. Specifically, the Add Health sampling design included stratification and unequal sampling across strata. In particular, twins were included in the sample at a disproportionate rate as compared to non-twins. As such, it was important to adjust the analyses to account for these sampling procedures by utilizing the sampling weights and cluster variables suggested by Chantala and Tabor (1999). Specifically, all analyses were carried out by weighting the cases according to their value on the wave 1 grand sample weight, by post-stratifying cases according to the region of the country in which the respondent lived, and by accounting for clustering of respondents within schools.³ Each of the survey design variables was created by the Add Health staff (for more information, see Chantala and Tabor, 1999) and by including them, the results can be generalized to the general population of American adolescents.

RESULTS

To begin, we examined whether twins exhibited mean differences in scores on a range of developmental and behavioral indicators. The findings from this portion of the analysis are contained in Table 1. As can be seen, twins did not differ from non-twins on most variables/scales. Indeed, 27 variables were analyzed and only 4 statistically significant differences emerged after making a Bonferroni correction (to account for multiple tests) to the critical value ($.05/27 = .0019$). The first was for the victimization scale; twins tended to be victimized less often as compared to non-twins. In addition, twins were found to differ significantly from non-twins on total number of sexual partners (twins reported fewer sex partners), birth weight (twins had lower birth weights), and maternal age at childbirth (twins were born to older mothers). The latter two differences were expected based on prior literature (e.g., Martin et al., 2011, 2012). Two of the differences were unexpected: twins experienced victimization less frequently and twins had fewer sex partners than non-twins.

³Ideally, standard errors would also be adjusted to account for twins being clustered within families. Based on Add Health recommendations (Chantala & Tabor, 1999), however, the sample selection was assumed to be conducted "with replacement." Chantala and Tabor (1999: 6) note, "The information needed to make finite population corrections for analyzing the dataset as a 'without replacement design' is not available." Because the sample was assumed to be conducted with replacement, the "survey design" package in *Stata* 12.0 (i.e., *svyset*) does not take into consideration any secondary sampling unit information (i.e., a request to adjust standard errors based on a family ID variable). More on this point in the Discussion section.

We are, however, hesitant to draw conclusions based on these results because the findings were not robust to a sensitivity analysis using the alternative twin classification scheme (see description of the alternative measure in footnote 1).⁴ Substantive findings were identical when the means/proportions were adjusted for respondent age, race, sex, and urbanicity.⁵

Thus far, our results indicate that twins exhibit relatively few *mean* differences across a range of important developmental indicators. When differences did emerge, they tended to be small—raising the question of the substantive significance—and two of them were expected based on prior literature. The second step in the plan of analysis was to further test whether twin (or non-twin) status conditions the effect of certain key covariates on the behavioral outcomes. Table 2 begins by presenting the results of a series of negative binomial regression models where the delinquency scale was utilized as the dependent variable. Recall that this step of the analysis included the respondent's age, race, sex, and urbanicity as control variables (results for these covariates are not reported in the tables). As shown in the table, three of the interactions emerged as statistically significant but only one (twin status * attractiveness, $b = -.27$, $t = 3.52$, $p = .001$) remained significant after a Bonferroni correction ($.05/24 = .0021$) was carried out to correct for the total number of tests. Thus, the findings presented in Table 2 suggest that few of the interactions between twin status and the key covariates were substantively important for predicting involvement in delinquency. It is important to note that none of the interaction terms emerged as statistically significant when the alternative twin identification strategy was utilized (see footnote 1) or when the sampling weights and cluster variables were omitted.

Table 3 contains the findings of 24 negative binomial regression models where the polydrug use scale was utilized as the dependent variable. Twin status conditioned the influence of two variables, but when a Bonferroni correction ($.05/24 = .0021$) was carried out only one of the interactions (twin status * sex partners, $b = .04$, $t = 3.71$, $p < .001$) remained statistically significant. For the remaining covariates, twin status failed to significantly moderate the influence of the key predictor variables (i.e., the interaction terms failed to reach statistical significance). None of the interaction terms emerged as statistically significant when the alternative twin identification strategy was utilized (see footnote 1) or when the sampling weights and cluster variables were omitted.

The final set of negative binomial regression models—where the victimization scale is the dependent variable—is presented in Table 4. Two interaction terms emerged as statistically significant yet a Bonferroni correction ($.05/24 = .0021$) washed out these results. Thus, there was no evidence that being a twin moderated the influence of the key covariates on the outcome of victimization. None of the interaction terms emerged as statistically significant when the alternative twin identification strategy was utilized (see footnote 1) or when the sampling weights and cluster variables were omitted.

⁴When the alternative twin identification strategy was employed (see footnote 1) two additional differences emerged between twins and non-twins (only those which passed the Bonferroni correction [$.05/27 = .0019$] are reported): twins displayed slightly lower verbal IQ scores ($p = .0005$) and twins were more likely to be Black ($p < .001$) than non-twins. The following variables/scales did not differ across twins and non-twins when the alternative twin identification scheme was used: victimization ($p = .77$), number of sexual partners ($p = .005$), and maternal age at childbirth ($p = .006$).

⁵When the sampling weight and cluster variables were omitted, the following variables were significantly different across twin status after a Bonferroni correction ($.05/27 = .0019$): victimization (twins reported less victimization), network popularity (twins were more popular), number of sexual partners (twins reported fewer sexual partners), biological maturity (twins reported less biological maturity), birth weight (twins had lower birth weight), maternal age at childbirth (twins were born to older mothers), parental permissiveness (twins reported less permissive parents), maternal attachment (twins reported more attachment).

DISCUSSION

The current study was intended to test the assumption that findings generated from a sample of twins are reflective of a larger population of non-twins. Our analysis of data gleaned from the Add Health revealed two broad sets of findings that are of note. First, twins did not differ significantly from non-twins in their involvement in delinquency or in their polydrug use. Twins did appear to report victimization experiences at a slightly lower rate as compared to non-twins (though this difference was not robust to the sensitivity analysis using an alternative twin classification scheme [see footnote 1 and footnote 4]). Also of interest was that twins did not differ systematically from non-twins on the majority of covariates included in the study. When differences did appear (e.g., differences in birth weight) they often were in line with prior research on twins and non-twins (e.g., Christensen, et al., 2006; Martin et al., 2011). Thus, the Add Health data appear to be comparable with other extant data sources meaning that there is nothing “unique” about the Add Health twin data that should produce systematic differences when compared to other samples.

The second important set of findings in the current study concerned the role that twin status played in moderating the relationship between correlates of antisocial behavior and the behavioral outcomes themselves. Virtually none of the covariates was conditioned by twin status when predicting behavioral problems. Only two statistically significant interactions emerged (after performing a Bonferroni correction) across the 72 different tests and these effects were neither consistent across the different behavioral outcomes nor were they consistent in their influence. As such, we are reluctant to conclude that these interactions reflect a “true” finding. It may be more likely that these findings were the result of random chance (i.e., a statistical artifact).

In short, the relationship between variables such as self-control and delinquency did not differ between twins and non-twins. This point is worth additional emphasis because it suggests findings from studies using twin samples will not differ systematically from findings gleaned from non-twin samples. This becomes all the more important given that numerous researchers have employed Add Health data (both the full sample as well as the twin sample) in order to examine the correlates and potential causes of antisocial behavior. Previously, little was known concerning the comparability of findings from the full sample compared to that of the twin sample in the Add Health. Our results, however, indicate that findings gleaned from the twin subsample may indeed generalize to the broader adolescent population.

The current study was not without limitation and it is important to mention possible shortcomings prior to concluding. Currently, four waves of data have been collected by the Add Health research team. This study, however, only analyzed data drawn from the first wave of data collection. Whether the pattern of results differs in later waves of the data is an open empirical question that we encourage scholars to consider. If prior research is any indication, however, it is likely that the small and infrequent differences identified here will become less meaningful as the respondents age (Webbink et al., 2008). A second limitation worth considering is that the current study was unable to examine more pathological outcomes including, for example, the influence of twin status on mental illness (e.g., bipolar disorder). While our findings suggest that the etiology of behavioral problems is likely similar for both twins and non-twins, this issue remains an empirical question that future studies can address. A third limitation is that we were unable to adjust standard errors for the non-independence of twin data (see footnote 3). We were unable to account for the non-independence of twin data due to the available information regarding the sampling strategy taken by the Add Health researchers (see footnote 3). This limitation would have downwardly biased standard errors, meaning that they are *smaller* than they otherwise would

be (if non-independence was accounted for). Because this limitation would have made it more likely to find a difference between twins and non-twins, and given the over-arching conclusion of no differences between twins and non-twins, the effects of this limitation are likely negligible.

Finally, a fourth limitation is that certain scales exhibited lower-than-desired reliability scores. Though a majority of the scales evinced a reliability score above .70, certain scales may have exhibited an attenuated reliability score due to the dichotomous/ordinal scaling of the constituent variables (i.e., the polydrug use scale, the biological maturity scales, the parental permissiveness scale, the maternal attachment scale, and the maternal involvement scale). Indeed, limited variance in the original items reduces alpha reliability scores (DeVellis, 2011). Importantly, each of the scales utilized in the current study were constructed based on the work of prior Add Health researchers. Future work should replicate the current study with scales known to exhibit higher reliability scores.

Analysis of twin data has been a cornerstone of behavior genetic research for decades (Rhee and Waldman, 2002). With the progression of time, twin research is also becoming an integral part of criminological (Barnes et al., 2011), psychological (Ferguson, 2010; Rhee and Waldman, 2002), and sociological (Guo et al., 2008) research. Given the increased use of twin methodologies, it is important to empirically examine whether findings gleaned from twin data reflect, in some way, the more general population. Our results suggest that in large part, they do. This finding underscores the relevance of twin research for the population as a whole, suggesting that twins are not unique and that findings gleaned from behavior genetic research, such as heritability estimates, apply broadly to the non-twin population.

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Table 1

Survey-Adjusted Summary Statistics by Twin Status

	Non-Twin Mean/%	Non-twin N	Twin Mean/%	Twin N	Absolute Mean Difference	t-stat	p-value
<i>Portion of Sample</i>	.94	17,711	.06	1,213	--	--	--
<i>Behavioral Outcomes</i>							
Delinquency Scale	4.42	17,399	4.13	1,201	.29	.97	.33
Polydrug Use Scale	1.61	17,263	1.45	1,186	.16	1.88	.06
Victimization Scale	.51	17,540	.33	1,206	.18	3.47	.0007
<i>Key Covariates</i>							
Low Self-control	30.41	14,400	29.60	1,011	.81	1.57	.12
Depression	10.19	17,594	9.81	1,203	.37	.89	.38
Drug-using Peers (Self-report)	2.60	17,113	2.49	1,168	.11	.59	.56
Delinquent Peers (Network Sent)	1.07	10,442	1.00	701	.07	1.12	.26
Delinquent Peers (Network Sent & Received)	1.04	12,062	1.00	828	.04	.93	.36
Network Centrality	.82	12,596	.82	870	.004	.10	.92
Network Density	.30	12,127	.31	832	.02	.82	.41
Network Popularity	4.59	12,596	5.67	870	1.08	1.90	.06
Time with Peers	1.99	17,693	2.04	1,213	.05	.97	.34
Attractiveness	3.56	17,644	3.62	1,213	.06	.93	.35
Number of Sex Partners	1.37	17,348	.86	1,194	.51	3.61	.0004
Verbal IQ	100.75	16,837	98.80	1,164	1.96	2.45	.02
GPA	2.83	13,032	2.84	934	.01	.27	.79
Biological Maturity	.01	17,519	-.18	1,201	.19	2.83	.005
Birth Weight	7.46	13,435	5.92	900	1.54	20.17	<.0001
Maternal Age Childbirth	25.50	12,728	26.85	916	1.35	3.79	.0002
Parental Permissiveness	5.12	17,260	5.00	1,181	.13	1.23	.22
Maternal Attachment	9.39	16,630	9.54	1,133	.15	2.45	.016
Maternal Involvement	3.91	16,629	3.85	1,136	.06	.58	.56
Maternal Disengagement	8.98	16,587	8.49	1,133	.50	1.85	.07
<i>Demographics</i>							
Age	15.50	17,697	15.29	1,213	.22	1.37	.17
Sex (1 = Male)	51%	17,709	51%	1,213	0%	.06	.95

	Non-Twin Mean/%	Non-twin N	Twin Mean/%	Twin N	Absolute Mean Difference	t-stat	p-value
Race (1 = Black)	16%	17,676	20%	1,212	4%	1.68	.095
Urbanicity (1 = Urban)	53%	17,550	47%	1,204	6%	1.24	.22

Note: All *p*-values are two-tailed; All estimates were weighted to account for the survey design effects of the Add Health study; Standard errors were corrected for clustering of participants within schools.

Table 2

Searching for Differential *Effects* of the Covariates: Survey-Corrected Negative Binomial Regression of Delinquency on Twin Status, Covariates, and their Interaction

	Dependent Variable: Delinquency			
	<i>N</i>	<i>b</i> _{Twin Status}	<i>b</i> _{Covariate}	<i>b</i> _{Interaction}
<i>Key Covariates</i>				
Low Self-control	15,097	.20	.06 [*]	-.01
Depression	18,356	-.04	.05 [*]	-.001
Drug-using Peers (Self-report)	17,981	-.11	.19 [*]	.01
Delinquent Peers (Network Sent)	10,937	-.17	.40 [*]	.09
Delinquent Peers (Network Sent & Received)	12,642	-.17	.45 [*]	.05
Network Centrality	13,189	-.27	-.09 [*]	.15
Network Density	12,705	-.31	-.18	.53
Network Popularity	13,189	.05	.01 [*]	-.04 ^a
Time with Peers	18,409	-.01	.19 [*]	-.04
Attractiveness	18,356	.87 [*]	-.03	-.27 [*]
Number of Sex Partners	18,181	-.10	.07 [*]	.05 ^a
Verbal IQ	17,540	.38	.0002	-.005
GPA	13,649	.13	-.36 [*]	-.05
Biological Maturity	18,363	-.05	.20 [*]	.02
Birth Weight	14,000	.76	.02	-.13
Maternal Age Childbirth	13,330	-.38	-.004	.01
Parental Permissiveness	18,004	.03	.04 [*]	-.02
Maternal Attachment	17,340	.38	-.20 [*]	-.04
Maternal Involvement	17,341	.05	-.004	-.03
Maternal Disengagement	17,316	.08	.08 [*]	-.01
<i>Demographics</i>				
Age	18,410	.22	-.001	-.02
Sex (1 = Male)	18,410	-.10	.38 [*]	.06
Race (1 = Black)	18,410	-.12	.04	.23
Urbanicity (1 = Urban)	18,410	-.15	.19 [*]	.15

^a $p < .05$,

^{*} $p < .01$ (two-tailed)

Note: Each row presents estimates from a separate negative binomial equation; Estimates were adjusted for covariance with age, race, sex, urbanicity (covariates not shown); Estimates were weighted to account for the survey design effects of the Add Health study; Standard errors were corrected for clustering of participants within schools.

Table 3

Searching for Differential *Effects* of the Covariates: Survey-Corrected Negative Binomial Regression of Polydrug Use on Twin Status, Covariates, and their Interaction

	Dependent Variable: Polydrug Use			
	<i>N</i>	<i>b</i> _{Twin Status}	<i>b</i> _{Covariate}	<i>b</i> _{Interaction}
<i>Key Covariates</i>				
Low Self-control	14,982	-.16	.04 [*]	.004
Depression	18,203	-.17	.03 [*]	.01
Drug-using Peers (Self-report)	17,936	-.10	.17 [*]	.01
Delinquent Peers (Network Sent)	10,880	-.11	.37 [*]	-.002
Delinquent Peers (Network Sent & Received)	12,565	-.06	.44 [*]	-.06
Network Centrality	13,102	-.19 ^a	-.05 ^a	.05
Network Density	12,627	-.19	-.25 [*]	.13
Network Popularity	13,102	-.12	.02 [*]	-.01
Time with Peers	18,261	-.07	.17 [*]	-.01
Attractiveness	18,209	.32	.02	-.11
Number of Sex Partners	18,059	-.12 ^a	.04 [*]	.04 [*]
Verbal IQ	17,415	.25	.003 ^a	-.003
GPA	13,535	-.02	-.30 [*]	-.01
Biological Maturity	18,215	-.06	.18 [*]	-.03
Birth Weight	13,897	-.25	.02 ^a	.04
Maternal Age Childbirth	13,249	-.70 ^a	-.004 ^a	.02 ^a
Parental Permissiveness	17,862	-.09	.07 [*]	.003
Maternal Attachment	17,204	.41	-.10 [*]	-.05
Maternal Involvement	17,208	-.02	-.004	-.02
Maternal Disengagement	17,183	.002	.05 [*]	-.01
<i>Demographics</i>				
Age	18,262	-.08	.13 [*]	-.0003
Sex (1 = Male)	18,262	-.15	.02	.12
Race (1 = Black)	18,262	-.10	-.27 [*]	.07
Urbanicity (1 = Urban)	18,262	-.08	.02	-.01

^a $p < .05$,

^{*} $p < .01$ (two-tailed)

Note: Each row presents estimates from a separate negative binomial equation; Estimates were adjusted for covariance with age, race, sex, urbanicity (covariates not shown); Estimates were weighted to account for the survey design effects of the Add Health study; Standard errors were corrected for clustering of participants within schools.

Table 4

Searching for Differential *Effects* of the Covariates: Survey-Corrected Negative Binomial Regression of Victimization on Twin Status, Covariates, and their Interaction

	Dependent Variable: Victimization			
	<i>N</i>	<i>b</i> _{Twin Status}	<i>b</i> _{Covariate}	<i>b</i> _{Interaction}
<i>Key Covariates</i>				
Low Self-control	15,192	-.77	.05 *	.01
Depression	18,484	-.51 ^a	.06 *	.01
Drug-using Peers (Self-report)	18,070	-.53 ^a	.22 *	.02
Delinquent Peers (Network Sent)	10,998	-.51	.40 *	.02
Delinquent Peers (Network Sent & Received)	12,722	-.62	.42 *	.08
Network Centrality	13,271	-.60 ^a	-.24 *	.09
Network Density	12,786	-.72 ^a	-.15	.55
Network Popularity	13,271	-.36	-.01	-.03
Time with Peers	18,548	-.57 ^a	.16 *	.06
Attractiveness	18,495	.97	-.06 ^a	-.41 ^a
Number of Sex Partners	18,297	-.41 *	.09 *	.05
Verbal IQ	17,674	.90	-.01 *	-.01
GPA	13,739	-.38	-.51 *	.01
Biological Maturity	18,493	-.44 *	.23 *	.20
Birth Weight	14,091	1.22	.01	-.27 ^a
Maternal Age Childbirth	13,424	-.06	-.02 *	-.01
Parental Permissiveness	18,132	-.37	.02	-.01
Maternal Attachment	17,464	1.21	-.12 *	-.17
Maternal Involvement	17,467	-.56	.002	.04
Maternal Disengagement	17,439	-.59	.05 *	.02
<i>Demographics</i>				
Age	18,549	.03	.06 *	-.03
Sex (1 = Male)	18,549	-.39	.95 *	-.06
Race (1 = Black)	18,549	-.49 *	.54 *	.26
Urbanicity (1 = Urban)	18,549	-.63 *	.42 *	.35

^a $p < .05$,

* $p < .01$ (two-tailed)

Note: Each row presents estimates from a separate negative binomial equation; Estimates were adjusted for covariance with age, race, sex, urbanicity (covariates not shown); Estimates were weighted to account for the survey design effects of the Add Health study; Standard errors were corrected for clustering of participants within schools.