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The effect of academic achievement on aggression and violent behavior: A meta-analysis

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1. Introduction

In this paper we present a meta-analysis of studies on the association between academic achievement and physically aggressive or violent behavior. The paper builds on the literature by testing the differential etiology of violence hypothesis proposed by Savage and Wozniak (2016), whereby academic achievement is among their "good prospects" for predicting violent, as opposed to nonviolent, criminal behavior. The current paper also provides estimates of effect sizes. An interest in the association between academic achievement and antisocial behavior has been seen in the published literature for decades (e.g., Jensen, 1976). Academic achievement has been inversely associated with conduct problems in children (e.g., Murray & Farrington, 2010), delinquency in adolescents (e.g., Lipsey & Derzon, 1998; Murray & Farrington, 2010), and even criminal activity in adults (e.g., Le Blanc, 1994). Some findings have implied that academic achievement may be related to *violent* antisocial behavior in particular (e.g., Katsiyannis & Archwamety, 1997). Rebellon and Van Gundy (2005) report that educational success was negatively associated with violent offending but not property offending in the National Youth Survey (NYS) data set. Lewis and colleagues found that incarcerated boys rated as "more violent" (having committed serious violent crimes) had lower scores for almost all tests, with significantly lower arithmetic scores (Lewis, Shanok, Pincus, & Glaser, 1979).

The differential etiology thesis suggests that the etiology of violent behavior is likely to be distinguishable from the etiology of nonviolent but antisocial behavior, an idea that is largely ignored in modern criminological theory. Savage and Wozniak (2016) include in their review of studies of violence those that measure violent criminal offenses, but also those that employ indicators of interpersonal violence where physical harm and intent to harm are present (such as studies of early physical aggression or self-report physical fighting). Academic achievement is equivalent to academic performance, and is usually operationalized with grades in U.S. studies, but might also be reflected in other indicators of academic success such as ratings of school performance (as was done in a study of Icelandic children, Bernburg & Thorlindsson, 1999), direct tests of reading or arithmetic, or given a "low secondary allocation" (as was used in a British study, Farrington, 1989).

Savage and Wozniak (2016) emphasize two main reasons that negative correlations between academic achievement and violence are theoretically sound, which also point to a stronger association between academic achievement and violence than between academic achievement and nonviolent offending (differential etiology). First, there is a close association between intelligence and academic achievement (Steinmayr, Ziegler, & Träuble, 2010; p. 14) and intelligence deficits have been associated with violent behavior in adolescents and adults in many studies (e.g., Ayduk, Rodriguez, Mischel, Shoda, & Wright, 2007; Barker et al., 2007; Cohen et al., 2003; Giancola, 2000). To the extent that low academic achievement is indicative of low intelligence, or specific cognitive deficits for some children, it may be associated with violence. Importantly, cognitive deficits and poor executive functioning in children have been associated with physically aggressive and antisocial behavior in many studies (see Savage & Wozniak's, 2016 chapter on intelligence and executive functioning). In several studies of offenders, violent offenders had significantly lower IQ scores than nonviolent ones, with recidivistic violent offenders having the lowest IQ of all (e.g., Holland, Beckett, & Levi, 1981; Kennedy, 2006; Lewis et al., 1979; Loeber et al., 2005).

Some of the reasons that intelligence is thought to be negatively associated with violence include the following. Those with low intelligence may also have weaker skills in complex problem solving (e.g., Stadler, Becker, Godker, Leutner, & Greiff, 2015), making it difficult to choose nonviolent options in a complex encounter. Verbal impairments (intelligence scores are often partly comprised of verbal tests) have been cited as among the "most well-established neurocognitive impairments associated with conduct behavior problems" (Barker et al., 2007, p. 593). In some studies, those with low intelligence have been less able to understand the inner workings of other persons (theory of mind) (e.g., Ibanez et al., 2013; Qualter, Barlow, & Stylianou, 2011) which is associated with empathy (Qualter et al., 2011). Some authors have reported significant correlations between intelligence and cognitive empathy (e.g., Schwenck et al., 2014). None of these in-and-of-themselves is inherent in academic underachievement, but low

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academic achievement may be indicative of hidden intellectual and cognitive deficits of this nature making intelligence an important potential confound in the association between academic achievement and violence.

Further, though the association between violence and intelligence is consistent across many studies, a review of the body of literature calls into question the association between intelligence and *nonviolent* criminality. Barker and colleagues have reported that a series of executive functions and verbal ability are negatively associated with physical aggression trajectories in their sample, but not with theft trajectories. In fact, controlling for violent offending, they report that measures of executive function and verbal intelligence were *positively* associated with frequent theft (Barker et al., 2007). Walsh (1987) found a negative association between IQ and violence, but a positive association between property crime and IQ. Bernat, Hall, Steffen, and Patrick (2007) report a negative correlation between WAIS-R scores and violence but not between WAIS-R scores and nonviolent offenses.

There is more to the theoretical foundation for the hypothesis that academic achievement is associated with violent behavior. School problems of many types are likely to engender frustration and negative emotionality (strain) and negative emotions clearly play a role in much aggressive and violent behavior. Although criminologists have largely ignored its role, the role of emotion has been featured in psychological theory and empirical research on aggression for a long time (Loeber & Hay, 1997). Seminal work on frustration and aggression (Dollard, Doob, Miller, Mowrer, & Sears, 1939) has been credited with initiating modern empirical work in this area (Baumeister & Bushman, 2007). Many authors in recent decades continue to emphasize the role of emotions in the etiology of aggression (e.g., Baumeister & Bushman, 2007; Beck, 1999; Bernard, 1990; Huesmann & Eron, 1992). When psychologists discuss causes of aggression, foremost on their lists of factors are items such as “unpleasant events,” (e.g., Bushman & Huesmann, 2010), frustration (e.g., Dollard et al., 1939), and anger (e.g., Scheff & Retzinger, 1991). Savage and Wozniak (2016) posit that negative emotionality is likely to have a special relationship with physically aggressive externalizing behaviors in young children and violence in adolescents because it is associated with “lashing out” (e.g., Dutton, 2011; Dutton, Starzomski, & Ryan, 1996; Eisenberg et al., 2001). While negative emotionality is associated with numerous forms of offending (e.g., Ferguson, 2011), some forms of negativity are likely to have a special relationship with physically aggressive behavior in children and violent behavior in adolescents and adults (e.g., Dutton, 2011; Dutton et al., 1996; Eisenberg et al., 2001).

The two emotions featured in studies of and theories about aggression are anger and shame (Baumeister & Bushman, 2007). In work by Kaplan (e.g., Kaplan et al., 1982), deviance springs from negative feelings that arise from self-derogation. According to Kaplan, self-derogation commonly occurs in the course of “normative participation” in various activities. While Kaplan did not emphasize school, it is clear that school experiences would fit neatly into his framework for deviance, and in one paper he reports that being “afraid of getting a bad report card” is associated with self-derogation (Kaplan & Pokorny, 1970). Doing poorly in school is highly likely to cause shame, frustration, and anger in many students.

In addition, low academic achievement may be caused by or correlated with other school problems, which are also likely to engender negative emotionality. Some authors have proposed that bonding to school (also referred to as school climate) might be protective against individual experiences of violence and community (e.g., Brookmeyer, Fanti, & Henrich, 2006). Other school problems include suspension, expulsion and, ultimately, low school attainment. Magnuson, Duncan, and Kalil (2006) point out that a sense of school connectedness and relationships with teachers play a particularly important role in the emotional and academic adjustment of middle school children. Thornberry draws our attention to the reciprocal nature of the relationship between academics and delinquency. Adolescents who become involved in delinquency “tend to have lower subsequent grades, develop weaker school bonds, and are less likely to graduate from high school or attend college” (Hoffmann, Erickson, & Spence, 2013, p. 631). Savage and Wozniak (2016) included school attachment in their review and concluded that measures of school attachment were consistently, negatively associated with violent behavior, and these relationships tended to withstand controls for academic achievement, in the few studies where such controls were applied.

Finally, Savage and Wozniak (2016) submit that the intensity of the experience, the grind of all-day every day attendance requirements “enhances the potential for dramatic positive impacts for those who benefit, and dramatic adverse impacts for those whose school experience is unhappy” (p. 41). To them, the “cumulative continuity” of negative interactions at school may snowball into larger problems (see also Payne & Welch, 2013). Their larger point is that the quantity and intense quality of the school experience magnifies whatever effect they exact on the developing child.

Emerging evidence suggests that, like intelligence, academic achievement may also be differentially related to violent over nonviolent offending. As an example, Hart, O'Toole, Price-Sharps, and Shaffer (2007) found that GPAs were significantly lower in violent compared to nonviolent delinquent adolescents. Loeber et al. (2005) reported that low academic achievement was significantly more common among violent compared to nonviolent offenders. There is no consensus on this point, however; other studies have reported significant negative associations between property delinquency and grades (e.g., Bernburg & Thorlindsson, 1999; Owens-Sabir, 2007; Rebellon & Van Gundy, 2005) and the difference may depend on the measure of academic achievement (e.g., reading vs. math; Marcus & Gray, 1998). Savage and Wozniak (2016) concluded from their comprehensive review of the evidence that academic achievement is a “good prospect” as a differential predictor of violence.

Thus, we examine what is known about empirical associations between academic achievement and violent behavior. Our summary will address the general research question about the consistency and size of that association. We will also examine the evidence to formally test whether the extant literature suggests that academic achievement is differentially associated with violent, compared to nonviolent antisocial behavior.

2. Method

2.1. Selection of studies

The studies included in this review were derived from those acquired for a larger project on academics, intelligence, and executive functioning. An attempt was made to acquire all published studies relevant for understanding the effect of academic achievement on aggression and violent behavior. To that end, we conducted extensive searches of Criminal Justice Abstracts and PsycINFO, supplemented by a search in ERIC. We combined search terms related to potential independent variables with a list of outcome terms. These databases were chosen because, together, they are comprised of the most comprehensive set of studies in the fields of developmental psychology and criminology. In Criminal Justice Abstracts, the outcome terms were: aggression, delinquency, crime, violence, violent, property, theft, status, nonviolent and non-violent. In PsycINFO, additional outcome terms were added due to different terminology used for aggression in the developmental literature. The terms were: aggression, delinquency, crime, violence, violent, property, theft, status, aggression, conduct disorder, conduct problems, externalizing, behavioral problems, antisocial, nonviolent, and non-violent. To capture the educational and learning measures, we used the following search terms: education, educational, attainment, academic, school, grades, learning disability, dyslexia, and reading, in addition to a long list of terms related to intelligence and executive function which were used for a larger project. Because ERIC was used to discover remaining items in education journals not included in PsychInfo or Criminal Justice Abstracts, a simpler search was conducted combining the terms "academic achievement" and "violen*" appearing in the abstract. In addition, we added items that came to our attention through other means. For example, added any item to the master list if the association between GPA or another indicator of academic achievement and physically aggressive or violent behavior was reported (or nonviolent-only offending), even if only as a control variable in a study of another topic.

We ran searches for every combination of search term reflecting violent or nonviolent antisocial behavior with every search term reflecting academic achievement. Initial vetting included a simple assessment of all titles returned in the search, to include those that could be publications reporting quantitative findings on this topic. From this a master bibliography of 160 pages was created and put to further scrutiny.

2.1.1. Inclusion/exclusion criteria

Studies selected for inclusion were limited to those with a dependent variable that operationalized physical aggression, criminal violence, or nonviolent criminal behavior, and an independent variable that operationalized academic achievement. Thus, we excluded the many studies where indicators of "externalizing" or "delinquency" combine violent with nonviolent antisocial behavior. Studies employing the Achenbach CBCL externalizing scale were excluded because most of the items on that scale do not reflect *physical* aggression. We did include studies that used the aggression subscale of the CBCL, as this largely reflects physical aggression. If the study also used grades, teacher ratings of academic achievement, other achievement scores, or scores on reading or math, it was included in our tables and estimates.

The final set of studies included a) those that reported associations between academic achievement and physically aggressive or violent behavior and b) those that reported associations between academic achievement and nonviolent antisocial behavior. These included studies with samples from the general population, as well as those that compared violent to nonviolent offenders.

2.2. Special problems for meta-analysis

Because procedures and purposes for meta-analysis have been widely described elsewhere (e.g., Hedges & Olkin, 1985; Hunter, Schmidt, & Jackson, 1982; Lipsey & Wilson, 2001; Rosenthal, 1991), we discuss only special issues related to this study.

2.2.1. Publication bias

When a meta-analysis is restricted to published reports, it is likely that average effect sizes will be larger than if unpublished ones are included because there is a tendency to publish studies with large or statistically significant effect sizes. This may result in a summary effect size that is larger than it would be if all studies were included. Some authors recommend including unpublished studies. This creates logistic problems, in particular in an area where research has been produced for many decades, by a great many authors. Ferguson and Brannick (2012) have also pointed out that no repository of unpublished data exists to ensure that any particular set of unpublished data is not, itself, a biased sample. Previous examinations of meta-analyses have indicated that attempts to include unpublished data tend to be haphazard, often increasing rather than decreasing bias (Ferguson & Brannick, 2012). Nonetheless, limiting ourselves to the published literature places significant limitations on potential conclusions from this research. A meta-analysis of this group of studies is unlikely to provide an unbiased estimate of the "true" effect size, but it can shed light on what *the published literature* has reported and help us establish whether or not firm conclusions on this matter are justified.

2.2.2. Mixed quality

Lipsey and Wilson (2001) discuss the problem of including studies of mixed quality as equal contributors to a meta-analysis. If a study with very "poor" methodology has an effect size that is much larger or smaller than the other studies, the estimate of the overall effect size will be biased. This was addressed in the present analysis by a) including only published studies (peer review being one indicator of quality); b) summarizing studies by analysis type so that studies with certain design features, thought to be higher in quality, are kept together and reported separately; c) weighting averages by sample size; d) testing and addressing heterogeneity and e) isolating comparisons that used appropriate control variables in statistical models and estimating separate effect sizes for them.

2.2.3. Statistical reporting, missing comparisons

Unfortunately, we were not able to derive summary effect sizes for some of the studies. In some cases, the authors report the finding–-statistically significant or not so–but do not report the coefficient. The same was true if the authors provided a coefficient that we were unable to convert to the common metric. In a few cases, the authors report a pattern of comparisons, but provide coefficients for the statistically significant ones only. We tried to retain as much information as possible, without estimating a biased effect. For example, if the authors found that a particular variable was significant for females and reported it, but not significant for males and did not report it, we eliminated that set of coefficients from effect size estimates.

In some cases, the methodology and pattern of analysis suggested that a comparison might have been estimated, but it was not reported. Hollin and Wheeler (1982) reported that in a model controlling for age, the simple correlation relationship was "still the same." We elected to use the provided statistic only for the effect size estimate. In the case of Ellickson and McGuigan (2000), the authors report for certain comparisons only that the association was "not statistically significant," with no coefficient. In these cases we substituted zero as the effect size, though this is likely to be very conservative as the association between "poor grades" and violence in all of their other models were positive and significant. We expect that the summary coefficient for this particular study is less than the actual value would be if we had complete information. We did compute a weighted average without the zeroes, and the new value was not very high, either, so we do not believe that the adverse effect of this adjustment is strong enough to exclude this important study.

2.2.4. Post hoc comparisons

Some authors have pointed to problems related to the use of post hoc comparisons. When coefficients estimated in post hoc analyses are included in meta-analysis, upward bias may be introduced. For example, Gorman (2004) examined the effectiveness of prevention programs and compared average effects of comparisons that were consistent with the original design of a set of studies and average effects for post hoc comparisons – follow-up analyses that followed from the first set of findings. He found that post hoc comparisons had higher average effect sizes than comparisons that were built into the original design of the studies and, in some cases, there was little evidence that programs that worked well when judged on the basis of their post hoc results, worked at all when evaluated based on the original models. Savage and Yancey (2008) emphasized this problem in a meta-analysis of studies on the impact of exposure to media violence on violent behavior. In that area of research, authors modified the computation of variables after initial null findings to come to a different result and different conclusion.

In the present study, post hoc comparisons were uncommon, but in a small number of cases we eliminated findings from multivariate models because it appeared that the authors had used information from the simple correlations to develop "best" predictors models. In this approach, usually accomplished with a stepwise method, the authors seek the "best model" by dropping and keeping variables in a model based on their associations with the dependent variable. In the present case, this might mean that the indicator for "academic achievement" is found in the model, in which case a coefficient is provided, or has been removed, with no further information. The net result of this problem is almost certainly an upward bias in average effect sizes, because null findings are not included in the computation of the mean unless the authors provided the associated statistical information. Therefore, multivariate coefficients provided by Farrington (1989) were excluded because the models were developed in a quest to discover "best predictors," and we elected to use only the simple comparisons provided in that study.

2.3. Procedure

2.3.1. Coding

In the initial phase, for each study a line of data was created for each comparison reported in tables or text that was relevant to the present research question, whether or not an effect size or even statistical significance was reported. Variables included, for example, study and comparison identifiers, subject ages (to separate child samples, adolescent samples, and adult samples, gender of the sample, sample size, independent variable (grades, reading, math), dependent variable (violent or nonviolent antisocial behavior), presence or absence of control variables (parent education, SES or neighborhood economic factors, a general measure of offending), and the effect statistic. All studies were coded by the principal investigator. The principal investigator double-checked the coding for every study and proofread after data entry. For a random sample of approximately 20% of the studies, a trained research assistant also coded the studies. There were no discrepancies between the two independent coders.

2.3.2. Summarization

The second phase involved reducing multiple effect sizes into one effect size for each study and for each analysis sub-grouping. In some cases, among the correlational studies and prospective longitudinal studies, more than one published study reported findings for the same group of subjects. This violates the assumption of independence and gives too much weight to those studies. Our solution was to create one line of data for each study of separate subjects so that no two records would be based on the same subjects. Thus, the coefficients from studies reporting findings for the following data sets were combined into one effect size each: the Pittsburgh Youth Study (Farrington, Loeber, & Berg, 2012; Fite, Wynn, & Pardini, 2009; Loeber et al., 2005), the Seattle Social Development Project (Herrenkohl et al., 2001, 2003), and Add Health (Bellair & McNulty, 2005; Choi, 2007; Resnick, Ireland, & Borowsky, 2004). There were other studies which used these data sets (e.g., Kosterman, Graham, Hawkins, Catalano, & Herrenkohl, 2001; McNulty & Bellair, 2003) but they did not provide coefficients that we could convert to the common metric so their findings were not included in the quantitative estimate.

The effect size estimate for each study consisted of the average of all of the effect sizes. We computed one mean effect size, treating all reported coefficients equally. We also computed separate mean effect sizes for each sub-analyses reported in our tables (e.g., simple correlations; multivariate coefficients; multivariate coefficient with control for parent education, SES or general delinquent behavior; whether the independent variable was GPA, reading, or math; whether the sample consisted of children, adolescents, or adults; whether the sample consisted of offenders or the general population; whether the sample consisted of a males, females, or a combination; whether the model was deemed to be "overspecified" or not [see below]).

Once we had achieved one summary effect size per study, effect sizes were combined into a weighted average for the subset being estimated. Pearson's r, a flexible and easily interpreted index of effect size, was used as the effect size (ES) estimate in this study. Effects reported in individual studies were first converted into the effect size r as per formulas provided by Rosnow and Rosenthal (2003) and then were transformed to Fisher's z, weighted, averaged and transformed back to a pooled r. In some cases, authors reported indirect information from which we were able to calculate effect sizes (e.g., means and standard deviations for compared groups).

Standardized β was treated as if it were equivalent to r (Pratt & Cullen, 2000). When discussing use of multivariate analyses, Lipsey and Wilson (2001) argue that because the standardized regression coefficient from each analysis is assumed to be estimating a different population parameter and the standard error of each regression coefficient usually cannot be computed from data reported, one cannot compute the inverse variance weight necessary for a proper meta-analysis. They argue that because of this, there really is no way at this time to accurately include multivariate findings from multiple regression, discriminant analysis, or structural equations models in a meta-analysis (see also Hunter & Schmidt, 2004). Most modern studies report multivariate findings, however, and the most important studies are likely to be the ones that have attempted to remove biases through multivariate modeling. So we combine multivariate standardized β into average effect sizes, as has been done by Pratt and Cullen (2000) and others, alerting the reader to the fact that a) these coefficients come from models with a variety of control variables, b) our estimate of weights, standard errors and confidence intervals may be imperfect and that c) some meta-analysts do not agree with this procedure (e.g., Cooper & Hedges, 1994; Hunter & Schmidt, 2004). For those who disagree with this usage, we provide a separate estimate for simple correlations. Further, we provide separate summaries for studies where parent education, for example, has been controlled, which means that the models we are comparing have similar (but not exactly the same) model specification.

2.3.3. Fisher's r to z transformation

As has become customary, each summary effect size originally reported as a function of r was converted to a z, because "the sampling distribution of z(r)-scores is assumed to approach normality, whereas the sampling distribution for r is skewed for all values other than zero" (Pratt & Cullen, 2000, p. 940) (also see explanations by Hall, 1995; Pearson, Lipton, Cleland, & Yee, 2002; Rosenthal, 1991).

2.3.4. Weighting

Because estimates of larger samples are thought to be more representative of the general population, it is also customary to use weighted averages in meta-analysis. We compute weights (w) as provided by Lipsey and Wilson (2001).

For $z(r)$:

$$w = n - 3$$

2.3.5. Windsorization

Some sample sizes in the correlational studies are very large, and it is undesirable to allow the largest studies to overwhelm the small ones in their impact on weighted average effect estimates. For example, if we weight a coefficient from a study with > 10,000 subjects in a similar way to the weight for a study with < 100, it will count > 100 times as much as the other study and we do not believe this would be appropriate. In fact, it would essentially nullify the influence of the small studies on the estimate. In most sub-analyses, we set an arbitrary cap on the sample size, for the purposes of weighting, at $n = 300$. In some of the sub-analyses with smaller numbers of cases, it made better sense to Windsorize the sample sizes to the highest n among the main group. For example, in the estimate for studies of females, the disparity in sample sizes made it sensible to Windsorize the samples to $n = 58$. Windsorization applied to weighting but not to the estimate of the standard error or confidence intervals.

2.3.6. Heterogeneity

Ideally, in a meta-analysis we seek a homogeneous distribution where the individual effect sizes are all measuring the same underlying relationship and all differ from the population mean only by sampling error (Lipsey & Wilson, 2001). The Q statistic is used to test homogeneity and is computed as follows; it is distributed as a chi-square:

$$Q = \Sigma w_i (ES_i - \overline{ES})^2$$

where w_i and ES_i are the weight and effect sizes for each case (in this case Fisher's z_r).

The summary effect sizes for associations between academic achievement and violence here were tested for homogeneity and were found to be heterogenous ($Q = 61.77$, $p < 0.0001$). There is no consensus in the literature for addressing heterogeneity. One recommendation is to identify outliers using frequency histograms (Lipsey & Wilson, 2001). We did so and removed outliers on a casewise basis. We were not able to use a traditional method due to small numbers of studies in most categories. Outliers deleted from estimates are noted as footnotes to our tables. We also provide Appendix B which displays the estimates computed with all available cases.

The second way that we addressed the heterogeneity problem was to report a summary statistic for subsets of cases. We divided the cases into groupings by analysis type (simple correlations, multivariate coefficients, multivariate coefficients from analyses with controls for parent education, SES, or general delinquency) and by sample (male, female, child, adolescent, adult). Early reviewers of this manuscript recommended a moderation analysis, but this is not statistically defensible when the number of effect sizes becomes small. Instead, we provide separate effect sizes for various types of analysis and allow the reader to interpret variations in effect sizes across study quality on this basis.

We provide the unweighted and weighted average effect sizes, after correcting for heterogeneity in Table 1. We provide the uncorrected estimates (including the outlier effect sizes) in Appendix B.

2.4. Publication bias

Publication bias was assessed through use of the Tandem Procedure (Ferguson & Brannick, 2012). Most approaches to examining publication bias look for associations between sample size and effect size (based on the fact that smaller effect sizes are required to achieve statistical significance for larger samples). However, due to low power of such analyses, both Type I and Type II error are common problems in publication bias analyses. The Tandem Procedure was developed as a decision matrix, combining multiple approaches to examining for publication bias. Thus, the Tandem Procedure is particularly effective in reducing Type I error in publication bias assessment. Ferguson and Brannick (2012) provide a full discussion of the Tandem Procedure and provide its decision matrix. The Tandem Procedure should be regarded as a fairly conservative test for publication bias, and the potential exists that certain types of publication bias may not be detected through this type of analysis.

3. Results

3.1. Overview

Forty-three studies met the criteria outlined in our introduction and also reported adequate information to code a summary measure of significance (See Appendix A). We were able to convert coefficients to a common effect size for 36 studies. In some cases, the authors did not provide coefficients or adequate information for us to estimate them; in other cases the authors provided coefficients for which we do not have a conversion formula.

Table 1
Summary of effect size estimates: The association between academic achievement and offending.

Grouping	# studies k	Σ summary n	Unweighted effect size estimate r	Weighted effect estimate r	95% confidence interval of ES_{zr}	
					Lower bound ES_{zr}	Upper bound ES_{zr}
Overall violence	28[a]	43,057	− 0.157	− 0.143*	− 0.154	− 0.135
Nonviolent offending	7	12,659	− 0.126	− 0.113*	− 0.1309	− 0.096
Violent compared to nonviolent offenders	7[b]	1681	− 0.117	− 0.106*	− 0.155	− 0.059
Offender samples	8[c]	1935	− 0.266	− 0.258*	− 0.309	− 0.219
General population samples	22	42,382	− 0.152	− 0.132*	− 0.155	− 0.107
Females	7	10,900	− 0.152	− 0.138*	− 0.158	− 0.120
Males	13[d]	12,801	–0.132	–0.127*	–0.145	–0.111
Child samples	4	2790	− 0.105	− 0.106*	− 0.144	− 0.070
Adolescent Samples	23[e]	37,377	− 0.160	− 0.151*	− 0.162	− 0.142
Adult samples	6[f]	6184	− 0.137	− 0.137*	− 0.164	− 0.113
Simple associations	21[g]	38,090	− 0.202	− 0.198*	− 0.210	− 0.190
Multivariate associations	17[h]	30,992	− 0.128	− 0.127*	− 0.138	− 0.116
Models with control for parent education	4	11,962	− 0.125	− 0.125*	− 0.144	− 0.108
Models with control for other forms of offending	3[i]	17,778	− 0.138	− 0.089*	− 0.104	− 0.075
Models with control for economic factors	6[j]	9391	− 0.105	− 0.083*	− 0.103	− 0.063
Overspecified models	7[k]	6675	− 0.055	− 0.044*	− 0.067	− 0.019
Models with control for prior violence	3[i]	5971	0.074	0.074	0.049	0.070
Multivariate models, not overspecified	12	20,917	− 0.162	− 0.159*	− 0.173	− 0.145

[a] Outliers removed: Bryant et al., 1984; Hollin & Wheeler, 1982; Mutschler, 1997; Tarter et al., 1983.
[b] Outliers removed: Bryant et al., 1984; Hollin & Wheeler, 1982.
[c] Outliers removed: Bryant et al., 1984; Hollin & Wheeler, 1982; Mutschler, 1997; Tarter et al., 1983.
[d] Outliers removed: Hollin & Wheeler, 1982; Mutschler, 1997.
[e] Outliers removed: Hollin & Wheeler, 1982.
[f] Outliers removed: Bryant et al., 1984; Mutschler, 1997.
[g] Outliers removed: Hollin & Wheeler, 1982; Mutschler, 1997.
[h] Outliers removed: Hyeunsook; Taylor et al., 2007.
[i] The number of cases was too few to consider removing any outliers.
[j] Outliers removed: Taylor et al., 2007.
[k] Outliers removed: Taylor et al., 2007.
* $p \leq 0.05$.

3.2. Overall effect of academic achievement on aggression and violence and the differential etiology of violence thesis

The weighted overall estimate of the association between academic achievement and violence, based on 43,057 cases from 28 data sets, was $r = -0.143$ and this was statistically significant (CI: -0.154, -0.135). This suggests that there is consistency across many studies but the effect size is not large. The data also support Savage and Wozniak's (2016) differential etiology of violence thesis. In studies comparing violent to nonviolent offenders, violent offenders consistently had lower scores on academic achievement ($r = -0.117$; k = 7). In offender samples, academic achievement was negatively associated with violent acts ($r = -0.258$) and this effect was larger in magnitude than the effect estimated from general population samples ($r = -0.132$). In a few studies, the authors controlled for other forms of offending and the negative association between academic achievement and violence remains—thus academic achievement does not appear to be associated with violence through its association with antisocial behavior more generally. Though it appears that academic achievement has a stronger association with violent antisocial behavior than nonviolent antisocial behavior, the association with nonviolent antisocial behavior is negative and statistically significant ($r = -0.113$).

3.3. Summary of simple correlation coefficients vs. multivariate partial coefficients

We have argued that simple correlations are likely to bias the effects. In these studies, the average effect size among the simple correlations reported was larger in magnitude ($r = -0.198$) than the average we computed based on partial coefficients in multivariate models ($r = -0.127$), though both estimates are statistically significant.

We were particularly interested in model specification, however, because partial coefficients can be biased if important variables are missing from the regression model, and severely attenuated when models are overspecified. We hypothesized that some of the variability in violence accounted for by academic achievement might be due to SES or parent education. The effect size estimate among studies where parent education was controlled was similar to those aforementioned ($r = -0.125$), but we should note there were only four studies included in the estimate. There was one important study which was not included. McNulty and Bellair (2003) reported a logit coefficient which we could not convert to the common metric. They presented four conservative models, controlling for "parents college graduates," and in all four, the coefficient for school grades is strongly, negatively associated with self-reported serious adolescent violence.

The estimate of the association between academic achievement and violence in studies controlling for socioeconomic status or neighborhood economic conditions (k = 6) appears to be somewhat attenuated ($r = -0.083$), though it remains statistically significant due to the large sample size used to estimate the standard error for the confidence interval. Two comparatively large positive coefficients were reported by Taylor, Davis-Kean, and Malanchuk (2007) and Hyeonsook (2010). These were removed from the estimates of effect sizes among the multivariate comparisons in Table 1 as outliers (but see Appendix B for estimate with these included). There were four other studies that reported important multivariate estimates including a control for SES for which we were unable to convert to a common metric. These provide further support for a robust association between academic achievement and violence. Bellair, Roscigno, and McNulty (2003) reported strongly negative and significant associations between school achievement and violent delinquency in their report. They were studying labor market opportunity, so a series of variables operationalizing this construct were included in the models as well as family income. Also using Add Health data, McNulty and Bellair (2003) report that school grades are still associated with violence in models controlling for family income and concentrated disadvantage. Using National Youth Survey data, Rebellon and Van Gundy (2005) also report a negative and significant coefficient for educational success and violence, controlling for the Hollingshead index. In the Seattle Social Development Study, Kosterman et al. (2001) found that school achievement was no longer statistically significantly associated with violent behavior in their multivariate model which added a simple control for poverty, but also controls for sex, race, ethnicity, childhood fighting, early individual characteristics, early prosocial development, early antisocial influences. On the whole, though the average effect size for this group is small, we conclude that the association between academic achievement and violence withstands a control for income or other economic factors.

A very conservative test of whether the association between academic achievement and violence is specific to violence, and not due to the association between academic achievement and general offending will come from studies where the authors controlled for other forms of delinquent behavior (see Savage & Wozniak, 2016). Only five studies did so, and only three of them reported coefficients that we could convert to a common metric. This group is heterogenous: Ellickson and McGuigan (2000) controlled for "problem behavior" explained as deviance and drug use frequency, Feshbach and Price (1984) controlled for same-wave delinquency in their models of parent-rated aggression, Herrenkohl et al. (2001) controlled for gang membership. Nonetheless, the summary coefficients for all three of those (Ellickson & McGuigan, 2000; Feshbach & Price, 1984; Herrenkohl et al., 2001) were negative and the weighted average for them was also statistically significant ($r = -0.089$, 95% confidence interval -0.104 to -0.075).

In addition to these three, two important multivariate studies have also controlled for general delinquency. McNulty and Bellair (2003) reported a logit coefficient and Piquero (2000) reported a Wald coefficient so we were unable to convert them to the common metric and include them in our average. Piquero (2000) controlled for number of police contacts to control for frequency of offending. Piquero (2000) employed a conservative model which also included controls for SES, maternal education, maternal smoking during pregnancy and pregnancy complications, low birth weight, neuropsychological factors of the child and the mother, school discipline problems and also WISC intelligence scores. Given the sample size and the conservative model specification, it is not surprising that Piquero reports coefficients estimating the association between academic achievement and violent behavior that are in the predicted direction but not statistically significant. It is notable that he reports almost no significant associations between individual variables and violence in these models. McNulty and Bellair (2003) report four conservative models, controlling for gang membership, and in all four, the coefficient for school grades is strongly, negatively associated with self-reported serious adolescent violence. Thus, resting on this body of evidence, we conclude that the findings point to a consistent association between academic achievement and violent behavior, even in models controlling for general antisocial behavior.

3.4. Model overspecification

Piquero's finding raises the question about redundancy. Some models include control variables that substantially remove variance that might reasonably be attributable to academic achievement, exerting a downward bias on our estimates of effect size. Statistics text books generally refer to this as "redundancy" among predictors, and we will use the term "model overspecification" as has been used by some others (e.g., Savage & Wozniak, 2016). We estimated an effect size for "overspecified" models and it was predictably small, but still statistically significant due to the large sample sizes upon which the estimate was based ($r = -0.044$). We point to five studies where we see the possibility of overspecification (for the purposes of our own research question). Bellair and colleagues (Bellair & McNulty, 2005;

McNulty & Bellair, 2003) controlled for violence just one year earlier than the date of the dependent variable measure of violence. Nonetheless, school grades were still significantly, negatively associated with violence. Kosterman et al. (2001), reported that school achievement was significantly correlated with violence (ages 13–21), but found that it was no longer significantly associated in their multivariate models where they controlled for childhood fighting, among other things. Rebellon and Van Gundy (2005) include "importance of education" and "time studying" in their model, which might be redundant with the construct of academic achievement and attenuate the association between academic achievement and delinquency, but they still find a significant coefficient. Taylor et al. (2007), whose positive coefficient turns up as an outlier in the heterogeneity test, controlled for previous school aggression in their model. Hyeonsook (2010), another outlier, controlled for previous wave aggression, measured just 6 months earlier.

By contrast, we compare the average effect among studies which we believe were not threatened by overspecification and that effect size was ($r = -0.159$).

3.5. Other disaggregated analyses

Turning our attention now to Table 2, we computed effect size estimates broken down by independent variable. All three indicators of academic achievement produced statistically significant effect size estimates. The overall weighted effect size estimates for GPA ($r = -0.131$), reading ($r = -0.207$) and math ($r = -0.254$) were all statistically significant (see Table 2). This supports the association across measures that are more generalizable to other countries than GPA would be. In two related studies, the authors looked at the association between being left back and violent behavior. Farrington et al. (2012) reported that homicide offenders in the Pittsburgh Youth Study were far more likely than control subjects to be older than expected for their grade in school. Resnick et al. (2004) self-reported violence among girls in the Add Health data was significantly associated with repeating a grade.

We also estimated effect sizes for children (k = 4), adolescents (k = 23) and adults (k = 6). The estimated effect sizes were statistically significant in all three groups ($r = -0.151$) (see Table 1).

3.6. Publication bias analyses

Effect sizes were analyzed for the presence of publication bias using the Tandem Procedure as described earlier. Results did not indicate evidence for the presence of publication bias. The Tandem Procedure is fairly conservative and it is possible that some forms of publication bias may be present without being detected by the Tandem Procedure. However, this analysis increases our confidence that results cannot easily be ascribed to the fact that we relied on published studies.

4. Discussion

In the present paper we have summarized findings from a body of literature on the effect of academic achievement on physical aggression and violence. While some studies report nonsignificant findings, our estimates are very consistent; in all of the weighted estimates, the negative association between indicators of violence and academic achievement was statistically significant. This was true for males and females, for children adolescents and adults, for simple and multivariate analyses. This was true for the three main indicators of academic achievement (GPA, reading and mathematics). It was also true in conservative analyses, controlling for parent education and SES.

Importantly, the findings also support the differential etiology of violence hypothesis proposed by Savage and Wozniak (2016). In studies comparing offenders, the violent offenders had significantly lower scores on indices of academic achievement. In studies controlling for other forms of offending, the association between violence and academic achievement was still statistically significant in almost all cases. In studies where variability of violent offending was examined in a group of offenders, violent behavior was predicted by lower grades. This provides supplementary evidence a) that a differential etiology of violence exists and b) that academic achievement may be an important contributor to violent, as opposed to nonviolent antisocial behavior.

One of the important implications of this finding is that it confirms the potential of remedial achievement programs in addressing violent behavior. Most in-school crime prevention programs focus on other issues entirely, including security measures and school management practices. Few explicitly make use of what we know about violent juveniles: that they are often failing in school. Because of the impact of violent behavior by juveniles today, including consequences for the victim but also very severe consequences for students accused of violence in schools, targeting academic remediation programs should be considered in any anti-violence initiatives.

One limitation of our findings is that much of the research relies on an indicator of academic achievement that is not used internationally: GPA. This limits the generalizability of effect size estimates for correlations between GPA and violence, though our findings were consistent for direct measures of reading and mathematics.

The data also suggest that while the association between low academic achievement and violent behavior is consistent, it is not particularly strong. The effect size estimate we look to as being likely to best represent the actual effect size, that for the multivariate models which we believe are not overspecified, was $r = -0.159$, which falls in the range of a "small" effect based on Cohen's (1988) assessment. This effect is also small if compared to other factors that we believe to influence violence. For example, effects for measures of economic well-being such as resource deprivation or income on aggregate measures of violent crime are often > 0.35 (Hannon & DeFronzo, 1998; Kubrin & Wadsworth, 2003; LaFree & Drass, 1996) and are sometimes as high as 0.62 and 0.77

Table 2
Disaggregated effect size estimates between academic achievement and violence.

Grouping	# studies *k*	Σ summary n	Unweighted effect size estimate *r*	Weighted effect estimate *r*	95% confidence interval of ES_{zr}	
					Lower bound ES_{zr}	Upper bound ES_{zr}
Outliers removed						
GPA	15	40,208	− 0.128	− 0.131*	− 0.144	− 0.124
Reading	9	746	− 0.200	− 0.207*	− 0.283	− 0.137
Mathematics[a]	6	491	− 0.251	− 0.254*	− 0.344	− 0.164
Full sample including outliers						
GPA	16	40,747	− 0.144	− 0.147*	− 0.153	− 0.139
Reading	10	766	− 0.099	− 0.184*	− 0.258	− 0.114
Mathematics	6	491	− 0.251	− 0.254*	− 0.344	− 0.164

[a] There were no outliers among the coefficients estimating association between math ability and violence.
* $p \leq 0.05$.

(DeFronzo & Hannon, 1998; Wooldredge & Thistlethwaite, 2003). If we look at the literature on inequality and violent crime, we see effect sizes in the range of 0.32 to 0.68 (Fajnzylber, Lederman, & Loayza, 2002; Hsieh & Pugh, 1993). Authors have reported effect sizes of 0.34 (Patterson, 1991) to 0.58 (Parker & Pruitt, 2000) for racial residential segregation. Pratt and Cullen (2000) computed an average effect of self-control on behavioral measures of 0.28, and effects of peer delinquency on offending are usually very high as well–Piquero and Sealock (2004) report 0.42 and Mears, Ploeger, and Warr (1998) report 0.32 (in a very large sample). Even moral commitment, a topic which has not received a great deal of attention, receives effect sizes on the order of −0.22 (Hannon, DeFronzo, & Prochnow, 2001), and −0.32 (Mears et al., 1998).

Our findings leave us with three important questions, then. First, what are the mechanisms through which academic achievement may be affecting violent behavior, and which mechanisms are responsible for violent as opposed to nonviolent offending? Savage and Wozniak (2016) propose that one potential mediator is the negative emotionality–frustration, anger, and shame of doing poorly in school–but we did not find adequate literature on such mechanisms to make any conclusions about these possible mechanisms. One potential confound is intelligence, and too few studies include indicators of intelligence along with indicators of academic GPA to estimate a summary effect size. Further research examining the mediating effects of intelligence and negative emotionality is recommended.

Second, what is the total effect of academic achievement on violence? In most of the studies examined here, cross-sectional or longitudinal correlations are examined, in some cases with control variables. But it is possible and, indeed, likely, that academic achievement affects other factors that influence violence. For example, if early academic achievement influences whether or not adolescents associate with violent friends or join gangs, that indirect effect should be counted as part of the total effect. Thus, longitudinal structural approaches to examining indirect effects on association with peers, gang joining, frustration, economic poverty or unemployment, and the like should be used. At this time, too few such studies are available to include in a systematic review.

Third, because the research question is decidedly developmental, further research on the timing of academic problems and their association with the onset of physically aggressive and violent behavior might be useful in order to make this line of research more helpful in developing prevention policy.

Appendix A. Summary information for studies included in the meta-analysis

Study	Sample size[a]	Sex	Sample age group	Offender/ general pop.	Simple correlation and/ or multivariate	Control for parent educ.	Control for economic factors	Control for nonviolent offending	Multivariate, not overspecified
Andrew, 1979	120	Mixed	Adol	Offender	Multi				✓
Bellair & McNulty, 2005	4803[b]	Mixed	Adol	Gen pop	Simple Multi				✓
Bellair et al., 2003[c]	4803[b]	Mixed	Adol	Gen pop	Multi		✓		✓
Bernburg & Thorlindsson, 1999[d]	3810	Mixed	Adol	Gen POP	Multi	✓			✓
Brownlie et al., 2004[d]	58 110	Fem Male	Adult	Gen pop	Simple				
Bryant, Scott, Golden, & Tori, 1984	110	Mixed	Adult	Offender	Simple				
Campbell, Spieker, Vandergrift, Belsky, & Burchinal, 2010[c]	543 548	Fem Male	Child	Gen pop	Simple				
Choi, 2007[d]	4803[b]	Mixed	Adol	Gen pop	Multi	✓			✓
Cohen, Rosenbaum, Kane, Warnken, & Benjamin, 1999[c]	102	Male	Adult	Gen pop	Simple				
Ellickson & McGuigan, 2000	2342 2021	Fem Male	Adult	Gen pop	Simple Multi	✓	✓	✓	✓
Farrington, 1989	411	Male	Adol Adult	Gen pop	Simple				
Farrington et al., 2012	1443	Male	Adult	Offender	Simple				
Feshbach & Price, 1984	297	Mixed	Child	Gen pop	Multi			✓	
Fite et al., 2009[d]	481	Male	Adol	Gen pop	Simple Multi		✓		
Harmon-Jones, Barratt, & Wigg, 1997	34	Mixed	Adol	Gen pop	Simple				
Hart et al., 2007	107	Mixed	Adol	Gen pop Offender	Simple				
Herrenkohl et al., 2001	808	Mixed	Adult	Gen pop	Simple Multi			✓	✓
Herrenkohl et al., 2003	808	Mixed	Adult	Gen pop	Simple				
Hollin & Wheeler, 1982	20	Male	Adol	Offender	Simple				
Hyeonsook, 2010	326	Mixed	Child	Gen pop	Simple Multi				
Johnson, 1979[d]	207	Male	Adol	Gen pop	Multi		✓		✓

Kennedy, 2006 DISS	30 65	Fem Male	Adol	Offender	Simple Multi				
Kosterman et al., 2001[c] Childhood risk factors for persistence.	808	Mixed	Adol	Gen pop	Multi		✓		
Lewis et al., 1979	97	Male	Adol	Offender	Simple				
Loeber et al., 2005 The prediction of violence …	1224	Male	Adult	Offender	Simple				
Loveland, Lounsbury, Welsh, & Buboltz, 2007	470 417	Fem Male	Adol	Gen pop	Simple Multi				✓
Marcus & Gray, 1998	101	Male	Adol	Offender	Simple				
McNulty & Bellair, 2003[c] Criminology	13,460	Mixed	Adol	Gen pop	Multi	✓	✓	✓	
Mutschler, 1997 DISS	61	Male	Adult	offender	Simple				
Owens-Sabir, 2007[d]	1675	Mixed	Adol	Gen pop	Multi				✓
Piquero, 2000	91	Mixed	Adult	Offender	Multi	✓	✓	✓	
Rainone, Schmeidler, Frank, et al., 2006	14,977	Mixed	Adol	Gen pop	Simple				
Rebellon & Van Gundy, 2005[d]	1118	Mixed	Adol	Gen pop	Simple Multi		✓		
Resnick et al., 2004	7419 6913	Fem Male	Adol	Gen pop	Multi				
Ribeaud & Eisner, 2010	532 573	Fem Male	Child	Gen pop	Simple				
Salts, Lindholm, Goddard, & Duncan, 1995	1192	Male	Adol	Gen pop	Multi				✓
Tarter, Hegedus, Alterman, & Katz-Garris, 1983	73	Male	Adol	Offender	Simple				
Taylor et al., 2007	842	Mixed	Adol	Gen pop	Multi		✓		
Wegner, Garcia-Santiago, Nishimura, & Hishinuma, 2010	307	Mixed	Adol	Gen pop	Simple Multi				✓
Wright & Fitzpatrick, 2006	4834	Mixed	Adol	Gen pop	Multi		✓		✓
Ybarra et al., 2008	1588	Mixed	Child	Gen pop	Simple				
Yeomans, 1996 DISS	49	Fem	Adol	Offender	Multi		✓		✓
Zentall & Smith, 1993[c]	92	Male	Child	Gen pop	Simple				

[a] Sample sizes listed here reflect the overall sample for the study and may not reflect the actual weight used in sub-analyses.
[b] All Add Health studies report in text that they use Wave 2 measures of violence and nonviolent antisocial behavior, so the sample size was adjusted to the maximum available in Wave 2 of Add Health.
[c] These studies were not included in estimates, only vote counts, because a coefficient was not reported or we were unable to convert the coefficients reported into a common metric.
[d] Authors provide estimates for nonviolent offending.

Appendix B. Summary of effect size estimates including outliers

Grouping	# Studies k	Σ Summary n	Unweighted effect size estimate r	Weighted effect estimate r	95% Confidence Interval of ES_{zr}	
					Lower bound ES_{zr}	Upper bound ES_{zr}
Overall violence	32	43,321	− 0.142	− 0.147*	− 0.158	− 0.139
Nonviolent offending	No outliers					
Violent compared to nonviolent offenders	9	1811	− 0.048	− 0.087*	− 0.133	− 0.040
Offender samples	12	2199	− 0.170	− 0.242*	− 0.299	− 0.205
General population samples	No outliers					
Females	No outliers					
Males	15	12,801	− 0.100	− 0.119*	− 0.137	− 0.102
Child samples	No outliers					
Adolescent samples	24	37,397	− 0.120	− 0.160*	− 0.158	− 0.138

Adult samples	8	6355	− 0.230	− 0.216*	− 0.245	− 0.195
Simple associations	23	38,171	− 0.175	− 0.199*	− 0.211	− 0.191
Multivariate associations	19	32,160	− 0.099	− 0.091*	− 0.102	− 0.081
Models with control for parent education	No outliers					
Models with control for other forms of offending	No outliers					
Models with control for economic factors	7	10,233	− 0.058	− 0.022*	− 0.042	− 0.003
Overspecified models	8	7517	− 0.020	− 0.002	− 0.019	0.022
Models with control for prior violence	No outliers					
Multivariate models, not overspecified	No outliers					

* $p \leq 0.05$

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