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Criminogenic or Criminalized? Testing an Assumption for Expanding Criminogenic Risk Assessment

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Abstract

Objectives: Proponents of criminogenic risk assessment have called for its widespread expansion throughout the criminal justice system. Its success in predicting recidivism is taken as evidence that criminogenic risks tap into the causes of criminal behavior, and that targeting these factors can reduce correctional supervision rates and even prevent crime. This study challenges these assertions, by testing the implicit assumption that populations in which recidivism risk factors were identified are interchangeable with populations experiencing the onset/duration of exposure to the criminal justice system.

Hypotheses: Exposure to the criminal justice system increases some of the risk factors used to predict recidivism and re-arrest; therefore, risk factors for recidivism and onset/duration of exposure to the criminal justice system are not interchangeable.

Method: Secondary analysis of data from 503 boys followed prior to first arrest through early adulthood. Inverse-probability-of-exposure-weighted marginal structural models and fixed effects models were employed to test whether arrests and convictions increase antisocial attitudes, behaviors, and peers.

Results: Being arrested or convicted resulted in subsequently higher levels of antisocial attitudes, behaviors, and peers. Risks for recidivism, which include the effect of exposure to the criminal justice system, are not identical to the risks of exposure to the criminal justice system.

Conclusions: Results caution against the uncritical expansion of criminogenic risk assessment from community corrections to policing, pretrial, and sentencing. Researchers and policymakers should engage with the social conditions that put people at risk of criminogenic risks and more cautiously communicate the scope of reform that criminogenic risk assessment can deliver.

Keywords

criminogenic risk assessment; criminal justice; recidivism; methodology; theory; algorithmic accountability

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Introduction

Over the past several decades, criminogenic risk assessment has become an integral, "evidence-based" component of the criminal justice system (National Institute of Corrections, 2010). Currently, in the "fourth generation" of criminogenic risk assessment, there has been a shift in focus to not only *assess* risk, but also to *reduce* it by intervening on criminogenic risk factors that are dynamic and manipulable (Andrews, Bonta, & Wormith, 2006). This strategy focuses on identifying people at the highest risk of recidivism and targeting them for supervision and treatment. The apparent success of this approach for recidivism reduction has been interpreted as evidence that criminogenic risk assessment taps into the causes of criminal behavior more broadly, and that targeting these factors can therefore also reduce criminal behavior and correctional supervision rates overall. Indeed, an explanatory framework has emerged around criminogenic risk factors as fundamental to the origins of criminal behavior and the roots of crime itself, and a model for organizing and applying this knowledge, the Risk-Need-Responsivity model of correctional assessment and rehabilitative programming, is widely accepted and promoted (Andrews & Bonta, 2010). Proponents suggest that criminogenic risk assessment can improve sentencing procedures, facilitate jail diversion, reduce prison populations, help scale down mass incarceration, improve policing, reduce violence and corrections spending, increase resources for community development, and ultimately, prevent crime altogether (Clement, Schwarzfeld, & Thompson, 2011; Jouvenal, 2016; Monahan & Skeem, 2016).

Yet, confidence in expanding risk assessment in criminal justice policy and practice may be outpacing the theory and evidence to support it (Desmarais, Johnson, & Singh, 2016; Hannah-Moffat, 2013; Lowenkamp & Whetzel, 2009). For example, Andrews and Bonta (2010, p. 299) go so far as to argue that "the prediction of criminal behavior is perhaps one of the most central activities of the criminal justice system [because] from it stems community safety, prevention, treatment, ethics, and justice." They advocate that criminogenic risk assessment's "…theoretical and empirical base…should be disseminated widely for purposes of enhanced crime prevention throughout the justice system and beyond…." (Andrews, Bonta, & Wormith, 2011, p. 738). This is indeed happening: the use of criminogenic risk assessment is expanding from the back-end of the criminal justice system to the front, in pre-trial processing, sentencing, and even policing (Lowenkamp & Whetzel, 2009; Monahan & Skeem, 2016; Storey, Kropp, Hart, Belfrage, & Strand, 2014).

But statements such as Andrews and Bonta's raise concerns about potential conceptual and empirical overreach. For example, should we transport prediction instruments designed for tertiary prevention (i.e., recidivism reduction) for use in primary or secondary prevention (i.e., first arrest, pre-trial detention, sentencing)? Such an expansion assumes that the populations in which criminogenic risk factors for recidivism were identified are interchangeable with populations facing the onset and duration of exposure to the criminal justice system. Moreover, such statements imply that interventions successful at reducing recidivism will also be successful at reducing the onset and duration of criminal behavior and exposure to the criminal justice system (Prins & Reich, 2018). This potential overreach matters for legal and correctional professionals and policymakers, who frequently do not understand the actuarial technologies upon which they base their decisions, such as mistaking probabilities for administrative certainties (Hannah-Moffat, 2013). As concerns about prediction and risk assessment in the criminal justice system enter the popular discourse (Angwin, Larson, & Kirchner, 2016; Barry-Jester, Casselman, & Goldstein, 2015; O'Neil, 2016), it is crucial that researchers, policymakers, and advocates are clear on not only the ethical, but also the methodological and empirical dimensions of the debate.

This paper tests the question: Is it possible that exposure to the criminal justice system increases criminogenic risk levels? If the answer is yes, and the propensities and dispositions our risk instruments are intended to assess are in fact influenced by the criminal justice system itself, what does that mean for expanding the use of criminogenic risk assessment beyond recidivism prediction? I will provide new empirical evidence for an old claim—that criminogenic risk assessments do not fully distinguish between individual-level propensities for criminal behavior and the fact that assessed individuals have already come into contact with the criminal justice system, i.e., that they occupy already-criminalized social locations. Ultimately, I will argue that the results of this analysis caution against the uncritical expansion—beyond recidivism prediction—of risk assessment based on individual-level criminogenic risk factors.

What Is Criminogenic Risk and Where Does it Come From?

Criminogenic risk assessment is the empirical foundation of the Risk-Need-Responsivity model of correctional assessment and rehabilitative programming. The Risk-Need-Responsivity Model has been described extensively elsewhere, but briefly, the model comprises (a) empirical evidence for predictors of recidivism; (b) a theory of criminal behavior inductively derived from those predictors (Ward, Melser, & Yates, 2007); and (c) a set of normative principles to guide effective practice (Andrews & Bonta, 2010; Andrews et al., 1990). This paper is primarily concerned with the first component of the Risk-Need-Responsivity model, and thus distinguishes criminogenic risk assessment from the other components, which are implicated here only insofar as they rely on the validity of criminogenic risk factors.

Criminogenic risk assessment is based on research that identifies strong individual-level predictors of recidivism among those under correctional supervision (Andrews & Bonta, 2010), and then uses individuals' scores on these predictors to categorize them into various risk groups. A history of antisocial behavior, antisocial personality pattern, antisocial cognitions, and antisocial associates are the "Big Four" factors that are consistently associated with recidivism in justice-involved samples; many dozens of meta-analyses support the predictive utility of instruments that include these factors (e.g., Gendreau, Little, & Goggin, 2006; Olver, Stockdale, & Wormith, 2014). Often, supervision and treatment strategies that target criminogenic risk factors modestly reduce recidivism (Andrews & Dowden, 2006; Lowenkamp, Latessa, Holsinger, Latessa, & Holsinger, 2006). Criminogenic risk assessment is thus seen as a key component of criminal justice reform: by matching the intensity of supervision and treatment to individuals' corresponding levels of risk, we can be smarter about policy, resource, supervision, and treatment allocation throughout the criminal justice system (Andrews & Bonta, 2010; Lowenkamp, Latessa, & Smith, 2006).

Since criminogenic risk assessment became an evidence-based practice, there have been two important developments. First, the most recent iteration of the Risk-Need-Responsivity model de-emphasizes prior distinctions between the Big Four and other risk factors (e.g., family/marital circumstances, school/work, leisure/recreation, and substance use problems), as the primacy of the Big 4 no longer appears to be empirically supported (Bonta & Andrews, 2017). Second, more recent meta-analyses have raised serious questions about criminogenic risk assessment's empirical status (Desmarais et al., 2016; Fazel, Singh, Doll, & Grann, 2012; Singh & Fazel, 2010). Nonetheless, given how influential the personality-based components of criminogenic risk assessment have become in both theory and practice, they remain the focus of this investigation.

According to the explanatory framework that emerged from (Andrews & Bonta, 2010; Ward et al., 2007) criminogenic risk assessment, the causes of crime are to be found within individuals and their social learning environments (Andrews & Dowden, 2008; Bonta & Andrews, 2017; Bonta, Blais, & Wilson, 2014). The Big Four "underpin a general personality and cognitive social learning theory of criminal behavior that provides an explanatory model of the origin and continuation of criminal conduct" (Olver et al., 2014, p. 157). Criminogenic risk assessment has informed the development of at least three theoretical models (the Psychology of Criminal Conduct, the General Personality and Social Psychological Perspective, and the Personal Interpersonal Community-Reinforcement perspective), in what Ward and colleagues (2007, p. 219) describe as a "bottom-up process" and what Andrews and Bonta (2010, p. 132) describe as a "radical empirical approach to building theoretical understanding."

A complete discussion of these theories is beyond the scope of the present paper, but ultimately, the Big Four criminogenic risk factors are informed by theory and evidence that certain personality traits distinguish individuals who engage in delinquent or criminal behaviors from those who do not, such as low constraint, negative emotionality, and cognitive impulsivity (Caspi et al., 1994; Leschied, Chiodo, Nowicki, & Rodger, 2008; Loeber et al., 2012). In addition to these personality characteristics, there is considerable overlap between the Big Four criminogenic risk factors and Diagnostic and Statistical Manual of Mental Disorders-5 Section II diagnostic criteria for antisocial personality disorder and conduct disorder (American Psychiatric Association, 2013), which have lifetime prevalences of roughly 2% - 5% and 1%, respectively, in the adult general population in the United States (Black & Blum, 2015; Compton, Conway, Stinson, Colliver, & Grant, 2005; Goldstein et al., 2007).

Yet, the psychological versus social origins of antisocial constructs are unclear. By definition, antisocial personality and conduct disorder involve violating the rights of others, repeatedly performing acts that are grounds for arrest, and repeatedly failing to sustain consistent work behavior or honor financial obligations. Antisocial cognitions involve attitudes, values, beliefs, and rationalizations supportive of crime and cognitive-emotional states of anger, resentment, and defiance. Antisocial constructs are thus necessarily relational: they are beholden to changes in social and legal norms about what constitutes criminal versus legal behavior, and to political-economic conditions that structure educational, employment, and other material circumstances. Indeed, both disorders are structured by social disadvantage: they are more prevalent among those with low income and education levels, among those who report more stressful life events, among those whose parents received welfare when they were children, among people undergoing residential drug treatment, and among people who experience homelessness (Black & Blum, 2015; Horwitz, Widom, McLaughlin, & White, 2001).

Furthermore, as predictors of illegal behavior, antisocial constructs are circular—they contain the outcome for which they are risk factors. One would thus expect people with antisocial personality disorder to be overrepresented in jails and prisons, and for this proportion to remain stable over changes in incarceration rates. Before mass incarceration, this seemed to be the case: 80% of incarcerated men and 65% of incarcerated women met proto-DSM-III criteria (the “Feighner criteria”) (Guze, Goodwin, & Crane, 1969). However, more recent estimates suggest the figure has dropped to 35% (Black et al., 2010). The decline in prevalence is attributed to dramatic increases in incarceration rates overall (Black & Blum, 2015; Black et al., 2010). Because the social policy of mass incarceration was not a response to increases in the incidence of criminal behavior (Gilmore, 2007; Wacquant, 2009; Western, 2006), more people *without* antisocial personality disorder have come into contact with the criminal justice system. In other words, despite the circularity of antisocial constructs, there are clearly powerful risk factors for exposure to the criminal justice system that are not mediated through individual-level propensities at all.

Conceptual and Methodological Dimensions of the Problem

Criminogenic risk assessment is built upon an immense foundation of studies examining why one person recidivates and another does not, but what these individuals share in common is that they have all been in contact with the criminal justice system; therefore, any causal effects of the criminal justice system on their behaviors are hidden (Schwartz & Carpenter, 1999). But if contact with the criminal justice system changes individual risk profiles, then the risk profiles of those with prior justice system contact may not be the same as those who have not yet come into contact with the criminal justice system. Expanding criminogenic risk assessment to predict the incidence of criminal behavior, i.e., moving criminogenic risk assessment to the front-end of the justice system, prior to first contact, may not capture these different risks.

Because virtually all research on criminogenic risk factors has been conducted with samples that are already involved in the criminal justice system, the causal contrast is unavailable for questions regarding criminogenic risk factors as causes of criminal behavior more broadly. Further, because criminogenic risks are typically measured at a single time point during or after incarceration, the causal contrast is unavailable for questions regarding these risk factors (or trajectories of risk profiles) as causes of initial or ongoing contact with the justice system. This includes rare inquiries into changes in criminogenic risk scores over time (Vose, Smith, & Cullen, 2013). If one were interested, for example, in the effect of antisocial attitudes on the onset or duration of criminal behavior or first contact with the criminal justice system, “exposed” and “unexposed” groups should be free of criminal behavior at baseline, and should have had the possibility of not being involved in the criminal justice system.

There is evidence that lends credence to these concerns. For example, evidence is mixed that criminogenic risk factors can predict distinct offending trajectories over the life course (Sampson & Laub, 2003). Furthermore, in a meta-analysis of childhood predictors of adult criminality, Lescheid and colleagues (2008) found that risk factors measured before age 7 had no predictive utility, and those measured before age 12 had only slight predictive utility, for adult offending. And criminological theory has long hypothesized that contact with the justice system might cause future deviance: labeling theory suggests that crime may be heightened by criminal sanction, so that contact with the justice system works only to criminalize people further (Cullen & Agnew, 2010; Plummer, 2001). Indeed, proponents of criminogenic risk assessment have long argued that the criminal sanction has no deterrent effect, and if anything, increases risk of reoffending (Andrews et al., 1990; Cottle, Lee, & Heilbrun, 2001; Gendreau & Smith, 2011). Numerous studies have found that exposure to the criminal justice system increases subsequent antisocial behaviors and peers, and some research has found similar effects on antisocial attitudes (Huizinga & Henry, 2008; Mowen, Brent, & Bares, 2018; Wiley, Slocum, & Esbensen, 2013).

If exposure to the criminal justice system has an effect on antisocial constructs, and antisocial constructs have an effect on recidivism, the effect of antisocial constructs thus includes the effect of exposure to the justice system. Therefore, the effect of criminal justice exposure may only be detectable in a sample of individuals observed prior to first contact with the criminal justice system and thereafter. However, isolation of such effects has typically been beleaguered with the problem of time-varying confounding, as illustrated in Figure 1. The secondary data analysis presented below leverages longitudinal data and advanced methods for control of confounding to triangulate causal inference for complex causal models where reciprocal relationships are likely. This analysis demonstrates empirically why the conceptual and methodological issues discussed above raise concerns about the wholesale expansion of criminogenic risk assessment. This paper's hypothesis is that exposure to the criminal justice system increases some of the risk factors used to predict recidivism and re-arrest; therefore, risk factors for recidivism and onset/duration of exposure to the criminal justice system are not interchangeable.

Method

Sample and Design

Data are from the youngest cohort of the Pittsburgh Youth Study, a prospective cohort study established in 1986 under the Office of Juvenile Justice and Delinquency Programs' Program of Research on the Causes and Correlates of Delinquency (Loeber, Farrington, Stouthamer-Loeber, & Raskin, 2008; Loeber et al., 2012; Pardini, Loeber, Farrington, & Stouthamer–Loeber, 2012). The study's design and sample have been described extensively elsewhere (Loeber et al., 2008, 2012; Pardini et al., 2012). Boys attending the first grade in virtually all public schools (N=31) in downtown Pittsburgh in 1987–1988 were recruited. Roughly 85% agreed to participate, and a random sample of this pool was selected for initial screening for antisocial behavior. This screening used a combination of parent, teacher, and self-report instruments. Boys with composite conduct problems scores in the upper 30% on this screening instrument (approximately 250 boys) in addition to a random selection of boys from the remaining 70% of the cohort (approximately another 250), were selected for follow-up (*N* = 503). The sample is predominantly Black (56%) and White (41%) with 3% Asian, Hispanic, and mixed-race/ethnicity, reflecting the racial/ethnic composition of Pittsburgh public schools at the time. As of 2012, the cohort has been assessed a total of 19 times: nine 6-month assessments from age six onward, yearly assessments from age 10 to 20, and assessments at age 25 and 28. Interviews were conducted with boys and their primary adult caretakers (until age 16). Prior to the assessment, caretakers and teachers provided written informed consent, and adolescents provided assent until age 17, and consent thereafter. This secondary analysis was exempted by the institutional review board at Columbia University.

Measures

Outcomes: antisocial attitudes, behaviors, and peers.—The present study uses constructed variables in PYS data that summarize antisocial attitudes, behaviors, and peers, which map onto three of the "Big Four" risk factors. Regarding antisocial attitudes, adolescents' responses to three scales were summed for each assessment interval to produce composite "total attitudes" scores. Scales included the Attitude Toward Delinquent Behavior Scale, which gauges youths' attitudes on a 5-point scale about the acceptability of 15 delinquent and substance-using acts (reliability = .73 – .83, internal consistency = .91) (Pardini et al., 2012; Zhang, Loeber, & Stouthamer-Loeber, 1997); The Likelihood of Getting Caught Scale, an 11-item scale that measures youths' perceptions of how likely it is that they would be caught by the police if they committed specific delinquent acts, and their perception of what would happen if they were caught (internal consistency = .90) (Loeber et al., 2008; Pardini et al., 2012); and a Perception of Problem Behavior scale, which measures youths' perception of the acceptability of engaging in a variety of delinquent behaviors (reliability = .77 – .80, internal consistency = .91) (Pardini et al., 2012; Zhang et al., 1997).

Regarding antisocial behaviors, variables include the frequency of very minor, minor, moderate, and serious delinquency (e.g., theft, violence, and drug selling). These constructs were summed for each assessment interval to produce composite "total behaviors" scores. These measures were constructed from the following scales: A 40-item Self-Reported Delinquency Scale, based on the National Youth Survey, which has been evaluated extensively (Elliott, Huizinga, & Ageton, 1985); the Self-Reported Antisocial Behavior Scale, which includes 27 items of delinquent behaviors appropriate to younger children and is easier for them to understand (Loeber, Stouthamer-Loeber, Van Kammen, & Farrington, 1989); and the Youth Self-Report (YSR), which measures youth behavior problems, as well as social and academic competence, such as prosocial behavior (Achenbach & Edelbrock, 1987).

Regarding antisocial peers, variables were measured by the Peer Delinquency Scale, which contains 15 items corresponding to a number of items on the Self-Reported Delinquency Scale and the Substance Use Scale (Loeber, Farrington, Stouthamer-Loeber, Moffitt, & Caspi, 1998). It asked whether *all, most, half, few,* or *none* of the youth's peers engaged in delinquent acts or used substances. Items were summed to create a total score. The internal consistency for this scale was α=.92 (Pardini et al., 2012).

Independent variables: Constructed variables that measure the count, per assessment interval, of adolescents' total arrests and convictions were obtained from official records.

Potential confounding variables.—Potential confounding variables include internalizing psychopathology, substance use, institutionalization, academic achievement, parenting factors, parental criminal history, neighborhood factors, and sociodemographic factors, in addition to prior values of antisocial attitudes, behaviors, and peers.

Internalizing problems.: Internalizing problems were measured with the Childhood Behavioral Checklist (CBCL) (Achenbach, 1991a, 1991b, 1991c; Youngstrom, Loeber, & Stouthamer-Loeber, 2000), which was administered to youths' primary caretakers. The CBCL is one of the most widely used instruments in both research and clinical practice with children (Youngstrom et al., 2000), The internalizing scale represents the sum of 32 items that loaded onto *withdrawn*, *somatic complaints*, and *anxious/depressed* clinical syndrome scales. The one-week test-retest stability coefficient for internalizing problems was .89 (Achenbach, 1991c; Youngstrom et al., 2000).

Substance use.: A 16-item Substance Use Scale based on the National Youth Survey (Elliott et al., 1985) was used to ascertain whether participants had ever used alcohol or marijuana in the period prior to assessment.

Youth institutionalization.: Youth institutionalization for a variety of psychopathological or behavioral problems was assessed with the Family Health Questionnaire (Loeber et al., 2008), measured as the number of occurrences in the past year.

Academic achievement.: Performance in school was measured through youths,' caretakers,' and teachers,' evaluations of achievement in reading, math, writing, and spelling; caretakers and youths also evaluated youths' achievement in up to three other academic subjects, such as history, science, or geography. Poor academic performance was rated on a four-point scale from 1 (above average) to 4 (far below average). The construct was created by taking the mean of all ratings across informants (internal consistency α = 0.81) (Pardini et al., 2012).

Parental stress.: Parental stress was measured by the Perceived Stress Scale, a 14-item scale that measures parents' perceived stress levels and abilities to cope with stress in the previous month (Loeber et al., 2008). Poor parental supervision was measured by the Supervision/Involvement Scale, a 43-question scale administered to both parents and youth, which assesses parents' supervision style, with values ranging from closely supervised to poorly supervised (Loeber & Stouthamer-Loeber, 1986).

Parents' conviction history.: Lifetime data on parents' history of conviction were collected via caretaker self-report (Loeber et al., 2008).

Neighborhood crime.: Neighborhood crime was assessed by the Neighborhood Scale (Loeber et al., 1998) which contained 17 items covering the perceived presence of prostitution, assaults, burglaries, and similar problems in the neighborhood, with higher scores reflecting more crime.

Socioeconomic status.: Socioeconomic status (SES) was assessed yearly by applying the Hollingshead Index of Social Status to data provided by the primary caretaker or youth no longer living with family beginning at age 16 (Miller & Miller, 1997).

Race/ethnicity.: Participant race/ethnicity was ascertained from adolescents' caretakers at screening. This variable was coded as "person of color" (POC) and white, due to the low frequency of Hispanic, Asian, and other categories.

Analytic Approach

All analyses were conducted in R version 3.5.2.

Missing data.—A small amount of missing data (Table 1) were imputed with multivariate imputation by chained equations, an implementation of fully conditional specified models for imputation. After imputation, for phases in which particular measures were not assessed, the last observation was carried forward. Estimates presented below represent results from analyses run on 10 imputed datasets combined with Rubin's Rules (Rubin, 1996).

Inverse-probability-weighted marginal structural models.—Marginal structural models (Hernán, Brumback, & Robins, 2000) were constructed for antisocial attitudes, behaviors, and peers, estimated with inverse probability weights for arrests and convictions, respectively. Inverse-probability-weighted marginal structural models control for all measured (i.e., observed) time-varying confounding—the confounding that can arise when variables act as confounding and mediating variables at different time points (Figure 1). Inverse probability weighting achieves this control of confounding by creating a pseudo-population in which each individual is weighted by the inverse probability of their own exposure history (in this case arrest and conviction history), essentially balancing measured covariates within the pseudo-population and making the exposure independent of measured confounding variables (Cole & Hernán, 2008). When the assumptions of consistency, exchangeability, and positivity are met, the exposure parameter estimates the average causal effect of the exposure in the original sample (Cole & Hernán, 2008; Hernán, Brumback, & Robins, 2002; Hernán et al., 2000). For a detailed exposition of inverse-probability-weighted marginal structural modeling in criminological research, see Sampson, Laub, and Wimer (2006).

The first step in constructing stabilized inverse probability weights is to determine the predicted probability of exposure status. I fit negative binomial models to estimate the predicted counts of arrest and conviction exposure history over the study period. To develop a robust model of arrest history, for example, I regressed arrest on one-year-lagged and lagged-cumulative versions of all time-varying confounds described above, in addition to study phase. That is, I used both the raw value of the confounding variable from the prior assessment interval, and their cumulative sum up to the prior assessment interval, to predict arrest counts in the subsequent assessment interval (Sampson et al., 2006). I also included the time-invariant race/ethnicity variable (Cole & Hernán, 2008). Next, I used this model to create a vector of model-predicted values for arrest, and input this vector into R's negative binomial probability mass function (Hernán & Robins, 2019). The resultant vector of values represents the probability that individuals were arrested the number of times they were actually arrested in each assessment interval. This vector is the denominator of the stabilized weights.

The second step is to create a model for the numerator of the weights. To ensure positivity (Hernán & Robins, 2006) and correct model specification, inverse-probability weights are typically stabilized by modeling the probability of exposure in the numerator, less time-varying covariates (Cole & Hernán, 2008). Often, baseline levels of time-varying covariates are also included in the numerator model, for further stabilization (Cole & Hernán, 2008). Weight stabilization ensures that the mean of the weights is approximately 1, the range of weights is not extreme (which would indicate nonpositivity or model misspecification), and that confidence intervals around effect estimates are narrow (Cole & Hernán, 2008). I regressed arrest counts on lagged arrest, lagged cumulative arrest, and race/ethnicity, and obtained a vector of model-predicted values as above. I then input the model-predicted values into the negative binomial probability mass function. The resultant vector of values is the numerator of the stabilized weights. As a result of stabilizing weights, the exposure is randomized within levels of the numerator model covariates, and so these covariates must be included in the final structural model. I repeated these steps to create stabilized weights for convictions.

Examination of the stabilized weights for arrests and convictions suggested positivity violations, as the probability of these events for certain participants was nearly zero. This made their inverse probabilities extremely large, and skewed the mean and range of the weights. I thus truncated the weights for further stabilization, by setting the 99th percentile of the weights as the maximum, meaning that the outliers' weights were changed to this maximum (Cole & Hernán, 2008).

Finally, I fit inverse-probability-weighted marginal structural models with linear Generalized Estimating Equations (Liang & Zeger, 1986). These models employed the robust sandwich variance estimator (Fitzmaurice, Laird, & Ware, 2004) to account for dependence of observations within individuals, and an exchangeable correlation structure. I regressed total antisocial attitudes, behaviors, and peers respectively, on total arrest and total convictions, respectively, while controlling for race/ethnicity and the lagged cumulative exposure variable.

Fixed effects models.—While inverse-probability-weighted marginal structural models control for all measured (observed) time-varying confounding, unmeasured confounding is still a threat to valid causal inference. As a complementary approach, fixed effects models control for all stable characteristics of study participants, whether or not they are measured (Allison, 2009; Curran & Bauer, 2011). This type of analysis reduces the possibility that time-stable individual differences such as genotype and family history can explain the association between criminal justice system contact and antisocial attitudes, behaviors, and peers.

A series of linear models were fit separately to test the effects of one-year-lagged arrests and convictions on total antisocial attitudes, behaviors, and peers. Following Allison's "dummy variable method" for fixed effects (Allison, 2009), models included a dummy variable for *n-1* respondents. I thus regressed total antisocial attitudes, behaviors, and peers, respectively, on lagged arrests and *n-1* respondents. I then added two-year-lagged versions of the potential confounding variables discussed above, as well as two-year-lagged antisocial characteristics (to control for the effect of prior values of the outcome on subsequent values). I repeated this procedure for the effects of convictions on antisocial attitudes, behaviors, and peers. Two-year lags were used for potential confounding variables so that they would be modeled prior to the measurement of the independent variable. This ensured that the estimated total effect of change in arrests and convictions on change in antisocial attitudes, behaviors, and peers included effects mediated through the covariates that occurred contemporaneously with changes in arrests and convictions.

In both the inverse-probability weighted marginal structural models and the fixed effects models, the outcome variables, which were constructed from measures using different scales, were standardized in order to aid in the interpretation of the independent variable coefficients.

Results

Figure 2 visually summarizes the longitudinal characteristics of the sample. The minimum and maximum values are labeled. For alcohol and marijuana use, columns represent the proportion of respondents who used alcohol or marijuana in the assessment interval. The total number of arrests and convictions and the proportion of individuals who were arrested or convicted are displayed in separate panels. Table 1 presents the medians, means, standard deviations, minimums, maximums, and percent missing for each variable. Of the 503 participants, 288 (57%) experienced arrests, 234 (47%) experienced convictions, and 196 (39%) experienced arrests and no convictions. Among those who experienced arrest, the ratio of convictions to arrests was approximately 1.4, which suggests that some arrests resulted in multiple convictions.

Inverse-Probability-Weighted Marginal Structural Models

Table 2 summarizes the results of the inverse-probability-weighted marginal structural models for the effects of contact with criminal justice system on antisocial attitudes, behaviors, and peers. All coefficients can be interpreted as the cumulative effect of the independent variable on the outcome, controlling for all measured time-varying and stable confounding variables, including baseline antisocial characteristics. The cumulative effect of arrest on antisocial attitudes was a 0.22, $p < .001$, 95% CI [0.13, 0.30] standard deviation increase in antisocial attitudes. Results were more pronounced for antisocial behaviors: the cumulative effect of arrest on antisocial behaviors was 0.33, $p = .003$, 95% CI [0.11, 0.55] standard deviations. The cumulative effect of arrest on antisocial peers was a 0.15, $p = .059$, 95% CI [−0.01, 0.31] standard deviation increase in antisocial peers.

Convictions had a more modest effect on antisocial attitudes, behaviors, and peers. The cumulative effect of conviction history on antisocial attitudes was a 0.07, $p < .001$, 95% CI

[0.03, 0.10] standard deviation increase in antisocial attitudes. The cumulative effect of convictions on antisocial peers was a 0.05, $p = .029$, 95% CI [0.00, 0.09] standard deviation increase in antisocial peers. There was no effect of convictions on antisocial behaviors.

Fixed Effects Models

Table 3 summarizes the results of fixed effects models for the effect of a one-year-lagged change in arrests on change in antisocial characteristics, controlling for all measured time-varying confounds and all unmeasured time-invariant confounds. Each additional arrest a respondent experienced in the prior year increased his antisocial attitudes in the subsequent year by 0.11 standard deviations ($p < .001$, 95% CI [0.07, 0.14]). Results were similar for antisocial behaviors (0.10 standard deviations, $p < .001$, 95% CI [0.05, 0.15]) and antisocial peers (0.12 standard deviations, $p < .001$, 95% CI [0.07, 0.18])

Table 4 summarizes the results of fixed effects models for the effect of a one-year-lagged change in arrests on change in antisocial attitudes, behaviors, and peers. Convictions again had a more modest effect than arrests on antisocial attitudes and peers. Adjusting for confounds, each additional conviction a respondent experienced in the prior year increased his antisocial attitudes by 0.03 standard deviations ($p = .001$, 95% CI [0.01, 0.05]). Results were similar for antisocial peers (0.03 standard deviations, $p = .042$, 95% CI [0.00, 0.06]). There was no effect of convictions on antisocial behaviors.

Discussion

In a community-based sample of 503 boys followed from childhood into early adulthood, exposure to the criminal justice system increased subsequent criminogenic risk factors. Each arrest, and to a lesser extent conviction, an individual experienced increased their subsequent antisocial characteristics. Arrests likely showed greater effects than convictions because arrest is a more visceral experience than conviction. Furthermore, because not all arrests resulted in conviction, and single arrests sometimes resulted in multiple convictions, the latter may lack precision.

This analysis verifies one methodological and conceptual concern with transporting risk assessment instruments developed to manage community corrections populations and reduce recidivism (tertiary prevention) to earlier contact points in the criminal justice system such as policing, pre-trial detention, and sentencing (primary and secondary prevention). The methodological problem is that in research examining individual-level risk factors for recidivism, samples are ubiquitously exposed to the criminal justice system; therefore, any causal effects of the justice system on their attitudes, behaviors, and peers are hidden. In other words, the populations on which criminogenic risk assessments are validated are systematically different from populations on which they might be applied at the front end of the criminal justice system. The closely related conceptual problem is with the suggestion that risk factors for individual differences in recidivism are the causes of crime more broadly. This cannot be the case if the risk instruments used to support this claim do not fully distinguish between individual-level propensities for criminal behavior and the criminalizing effects of the criminal justice system itself. Moving further upstream, this conceptual problem can be re-articulated: in the era of mass incarceration, the suggestion that risk factors for individual differences in recidivism are the causes of crime more broadly ignores structural causes of exposure to the criminal justice system that are not fully mediated by individual-level factors.

Risk, Criminal Behavior, Crime, and Recidivism

The general personality and cognitive social learning theory that emerged from criminogenic risk assessment can begin to explain the present study's findings, insofar as individuals' experiences with law enforcement, courts, and corrections (a) reinforce their ambivalence about prosocial norms or negative feelings about the criminal justice system, (b) increase exposure to other crime-involved peers, or (c) reduce the perceived costs relative to benefits of engaging in criminal behavior. These mechanisms operate at the individual level in the immediate situation preceding a criminal act (Andrews & Bonta, 2010). This perspective's strength lies in its elaboration of proximate risk factors such as these, which directly inform cognitive-behavioral treatments that target attitudes, feelings, self-control, etc. However, from etiologic and prevention perspectives, it is inadequate to explain the findings from this analysis merely in terms of individuals' psychological predispositions. Prioritizing the immediate situation preceding criminal behaviors—the end of a causal process—masks the antecedents of that process and any feedback loops therein (and ignores social factors that do not operate through individual-level risk factors). To understand the various causes of proximate criminogenic risks, we must locate people, their psychologies, and their behaviors in a wider causal context.

Taking seriously the social antecedents of criminogenic risks, and not ignoring them as fixed background characteristics, may begin to remediate conceptual slippage that arises from the methodological individualism of criminogenic risk assessment. Conceptually, the Big Four criminogenic risk factors are located within the discourse of psychopathology, in which crime and criminal behavior are roughly the same constructs, and both reside within or emerge from deviant or abnormal individuals (e.g., Andrews & Bonta, 2010). But if crime is a psychologically reductionist behavioral phenomenon, then *why* it occurs, *by whom* it occurs, and *how much* it occurs become the same question. This perspective leads to conceptual slippage because crime is in fact a complex, multi-level construct that denotes social deviance and norm violations, activities prohibited by the state and codified in law, and various dynamic subsets and intersections therein. Crime can thus be both a specific action/behavior and a social process, the latter in terms of dynamic interactions among people, institutions, norms, and laws, all of which can differ over time and place. The psychopathological conceptualization of crime either ignores these social processes and contingencies, or assumes they are fixed, thus conflating crime (and exposure to the criminal justice system) with criminal behavior. In other words, it sidesteps the question of what puts people at risk of criminogenic risks.

Further conceptual slippage occurs when recidivism is also conflated with crime and criminal behavior, because incident criminal behavior is sufficient but not necessary for certain definitions of recidivism. Indeed, the definition of recidivism influences the performance of particular instruments (Desmarais et al., 2016; Desmarais & Singh, 2013). For example, Vose and colleagues (2008) found that when recidivism was defined as reincarceration, 100% of validation studies of one instrument deemed it a valid predictor of recidivism, but when recidivism was defined as re-arrest, only 54% deemed it a valid predictor of recidivism. The explanatory power of criminogenic risk assessment for the onset of justice system contact is thus further obscured, if proximate risk factors fine-tuned for certain definitions of recidivism are used to predict or explain more distal phenomena. Such issues should be resolved before any risk assessment instrument designed for use at one location in the criminal justice process is employed in another.

A more robust understanding of how the criminal justice system increases individuals' antisocial characteristics would seem to require a shift in theoretical perspective. For this there are numerous intellectual strains, beyond labeling theory, that engage seriously with the wider context in which dynamic systems, processes, and individuals' encounters with them take on causal significance. For example, scholars have cautioned that in the era of mass incarceration, the therapeutic, rehabilitative origins of criminogenic risk assessment have been "supplanted by a managerialist approach centered on the cost-drive administration of carceral stocks and flows…" (Wacquant, 2009, p. 2). This shift has likely not gone unnoticed by individuals navigating the system. In his in-depth interviews with over 50 residents of a juvenile detention facility and its staff, teachers, and administrators, Reich (2010) shows how the young men there defined success in strategic rather than moral terms —as staying out of the detention facility, but also improving their material conditions, i.e., engaging in crime without getting caught. Reich (2010), drawing on Feeley and Simon's (1992) foundational analysis, suggests that

> …this strategic orientation toward prison among young men might be understood as the flip side of a…justice system that has increasingly abandoned any pretense of treatment *or* punishment, where the impersonal and actuarial management of a criminal population takes precedence over moral and personal responses to criminals, whether rehabilitative or punitive (p. 77).

Appreciating the systematic community disinvestment, bleak and racialized educational and employment opportunities, and the erosion of unions and other political and civic organizations, Reich's framework does not find it surprising that young men involved in criminal behavior would experience their relation to the criminal justice system as "a game in which the goal is to profit as much as possible without getting caught" (Reich, 2010, p. 77) – with more exposure to the system potentially reinforcing this outlook.

The Right Answer for the Wrong Question?

This analysis shows that it may be inappropriate to apply evidence for the predictive and intervention utility of risk factors for recidivism to broader questions about the onset and duration of criminal activity. This is because exposure to the criminal justice system causes some portion of the risk used to predict subsequent involvement in the criminal justice system. These findings suggest that criminogenic risks identified in samples under correctional supervision may be different than more distal risks that occur prior to first exposure to the criminal justice system.

For example, post-hoc analysis revealed that the strongest predictors of first arrest in this sample, controlling for age, were marijuana use, race, and alcohol use (see Table 5). That said, overlapping confidence intervals suggest that many of the top 10 factors may be statistically indistinguishable. Many of the factors in Table 5 may be measured, and appropriately treated, at the individual level (e.g., substance use disorders). Indeed, when operationalized at the individual level, many are considered criminogenic risk factors in the Risk-Need-Responsivity framework. However, many can also be conceptualized as social factors that are not fully mediated by individual-level characteristics. In some cases, these social factors may be more appropriate targets for primary prevention, e.g., the criminalization of all forms of drug use, racial disparities in arrests, and institutional racism in policing (Human Rights Watch, 2016; Rovner, 2016; Swaine, Laughland, & Lartley, 2015).

The issue of "reverse causation" uncovered in this analysis also raises important questions about the efficiency and effectiveness of criminogenic risk assessment. If exposure to the criminal justice system increases the very risk factors used to predict recidivism or future criminal activity, intervening on these risk factors will not reduce overall levels of criminal justice system involvement unless we also reduce the absolute number of people flowing into criminogenic risk. Population *prevention* strategies, versus population *management* strategies, would aim to reduce first exposure to the criminal justice system, not merely deploy criminogenic risk assessments during or after first exposure. True prevention would focus on shifting the risk distribution's mean, not merely truncating its right tail (McMichael, 1999; Rose, 1985). Indeed, the originators of criminogenic risk assessment, and many of its proponents, have been among the first to advise against conflating recidivism reduction with primary prevention (e.g., Andrews & Bonta, 2010). But they may be failing to heed their own warning in their calls for the sweeping expansion of criminogenic risk assessment throughout the criminal justice system.

Limitations

The present study's findings should be understood in light of the following limitations. First, all participants in the Pittsburgh Youth Study are male, which limits generalizability. Nonetheless, contact with the criminal justice system is a predominantly male phenomenon, as is antisociality (Black & Blum, 2015; Durose, Cooper, & Snyder, 2014; Glaze & Parks, 2012). Second, all participants were selected from Pittsburgh public schools, which potentially limits generalizability to other areas if there were any secular trends regarding criminal justice policy or antisociality. Third, half of the sample comprised high-risk boys, which limits generalizability, but potentially makes the findings more conservative, as there was less baseline variation in antisocial characteristics than one might find in a representative sample. Fourth, while measures were available for the constructs that underlie three of the Big Four criminogenic risk factors, it was not possible to test a particular risk assessment instrument directly. However, as Skeem and Cooke (2010) have noted, one measure does not a construct make, and it is difficult to imagine measures with greater convergent validity. Fifth, data on arrests, charges, and convictions were not linked, so it was not possible to follow participants through the criminal justice process.

Conclusion

The analysis presented here shows that arrests and convictions result in subsequently higher levels of antisocial attitudes, behaviors, and peers among boys followed into young adulthood. Results caution against the wholesale expansion of criminogenic risk assessment from community corrections to policing, pretrial decision-making, and sentencing, as the causes of recidivism may not be the same as the causes of the onset and duration of exposure to the criminal justice system. Future research should engage with the social conditions that put people at risk of criminogenic risks and consider the criminalizing effect of contact with the criminal justice system. Researchers and policymakers should more cautiously communicate the scope of reform that criminogenic risk assessment can deliver, and be more open to population approaches to prevention that account for structural influences on the risk distributions of crime and exposure to the criminal justice system.

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Public Significance

This study found that exposure to the criminal justice system itself increases some of the risk factors used to predict recidivism and re-arrest. This raises concerns about transporting risk assessments validated for recidivism prediction to the front-end of the criminal justice system. Populations in which risk factors for recidivism were identified are not interchangeable with populations facing the onset of exposure to the criminal justice system, and a failure to recognize this may lead to inappropriate prevention targets.

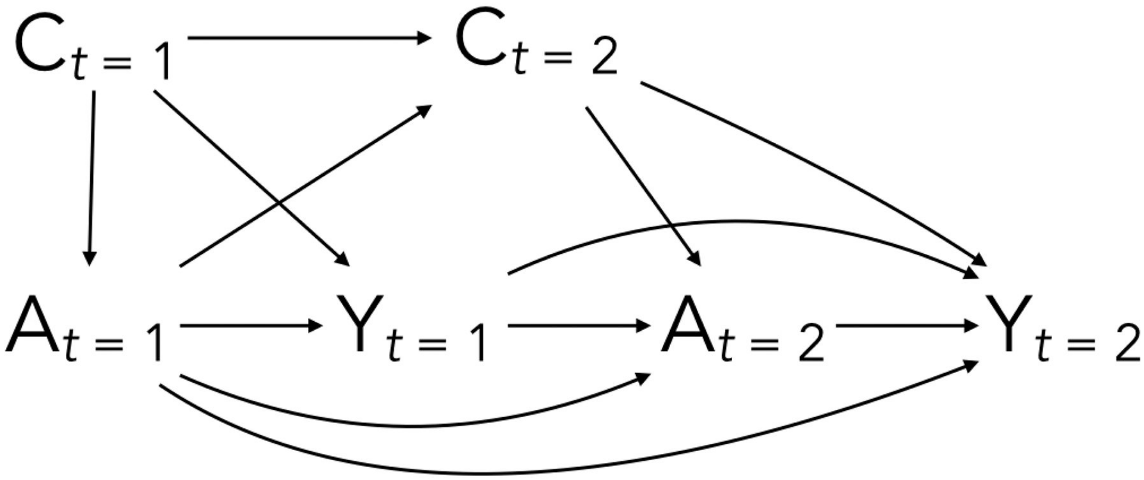

Figure 1.
Directed acyclic graph illustrating the problem of time-varying confounding.
Note. A = arrest or conviction, Y = criminogenic risk, C = confounding variables, e.g., parenting, academic achievement, neighborhood crime, mental illness, substance use, SES, etc. *t* = time.

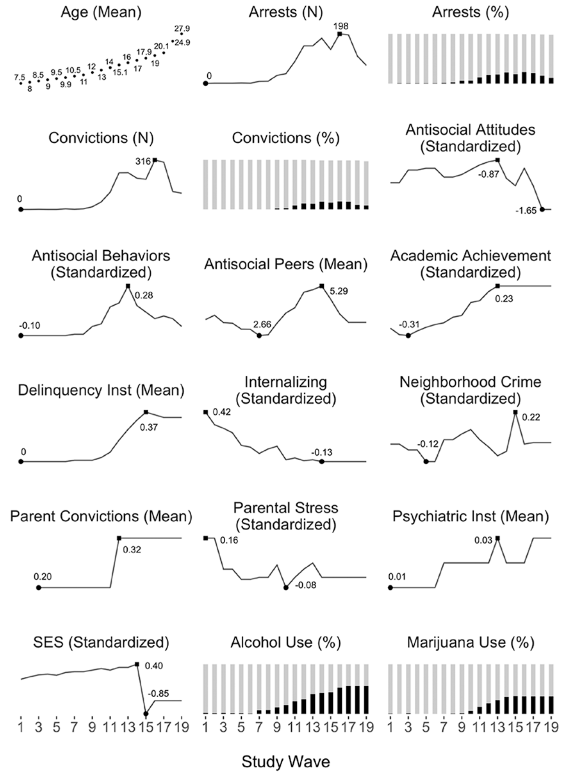

Figure 2.

Variable values over time. Note. Circles represent minimums, squares represent maximums. For alcohol, marijuana use, arrests, and convictions, columns represent the proportion “yes” (black) and “no” (gray).

Table 1.

Description of study variables

Variable	Construct	Reliability α	Median	Mean	SD	Min	Max	% Missing
Total Antisocial Attitudes	Attitudes toward delinquent behavior, perceptions of problem behavior, perceptions of likelihood of getting caught	.77 – .83	44.5	44.4	18.5	0.0	140.0	0.2
Total antisocial behaviors	Very minor, minor, moderate, serious	0.77 – 0.92	0	3.9	24.8	0.0	1,002.0	0.2
Total antisocial peers	Number of youth's peers who engaged in activities described above under "delinquent behaviors"	0.79 – 0.96	2	3.9	5.1	0.0	40.0	0.21
Total Arrests (*n* = 1,371)	Frequency of total arrests	NA	0	0.1	0.6	0.0	7.0	0.3
Total Convictions (*n* = 1,883)	Frequency of total convictions	NA	0	0.2	1.1	0.0	29.0	0.3
Internalizing t-score	Psychiatric symptoms, disorders	Depression: 0.87	48	48.3	10.4	24.8	88.0	0.21
Psychiatric institutionalization	Periods of psychiatric institutionalization	NA	0	0.0	0.2	0.0	4.0	0.24
Delinquency institutionalization	Periods of correctional institutionalization	NA	0	0.1	0.6	0.0	12.0	0.22
Academic performance	Achievement in reading, writing, math, spelling, and up to three other academic subjects	0.46 – 0.56	2.1	2.2	0.7	0.5	4.3	0.21
Parental stress	Caretaker perceptions of stress in past month	0.57 – 0.85	23	23.5	5.0	10.4	42.0	0.26
Parental supervision	Youth perceptions of parental discipline, supervision.		5.5	5.9	1.4	2.8	12.0	0.21
Parent convictions	Frequency of parental convictions	NA	0	0.3	0.7	0.0	4.0	14.1
Neighborhood crime	Presence of prostitution, assaults, burglaries, etc.	0.95	12	14.5	5.3	1.8	31.2	0.22
Socioeconomic status	Race, ethnicity, work, marital status, education of caretakers	NA	37	34.6	12.7	0.0	67.2	0.3
Alcohol use	Used alcohol in past assessment interval (Yes/No)	NA	% No: 73.9 % Yes: 26.1					0.21
Marijuana use	Used marijuana in past assessment interval (Yes/No)	NA	% No: 83.7 % Yes: 16.3					0.21

Table 2.

Inverse-probability-weighted marginal structural model estimates for the cumulative effects of exposure to the criminal justice system on antisocial attitudes, behaviors, and peers

	Antisocial Attitudes			Antisocial Behaviors			Antisocial Peers		
	β	*95% CI*	*p*	*β*	*95% CI*	*p*	*β*	*95% CI*	*p-*
Arrests									
Intercept	0.46	[−0.04, 0.97]	.072	0.59	[−0.57, 1.76]	.316	0.18	[−0.29, 0.66]	.445
Arrest	**0.22**	**[0.13, 0.30]**	**< .001**	**0.33**	**[0.11, 0.55]**	**.003**	**0.15**	**[−0.01, 0.31]**	**.059**
Race (Person of color)	−0.39	[−0.94, 0.16]	.161	−0.86	[−2.81, 1.09]	.385	0.23	[−0.36, 0.83]	.438
Lagged cumulative arrests	−0.13	[−0.17, −0.09]	< .001	−0.05	[−0.14, 0.05]	.358	−0.04	[−0.1, 0.02]	.182
Convictions									
Intercept	0.19	[0.00, 0.39]	.055	0.03	[−0.06, 0.11]	.536	0.03	[−0.20, 0.26]	.815
Conviction	**0.07**	**[0.03, 0.10]**	**< .001**	**0.21**	**[−0.52, 0.94]**	**.571**	**0.05**	**[0.00, 0.09]**	**.029**
Race (Person of color)	−0.05	[−0.30, 0.20]	.669	−0.13	[−0.32, 0.07]	.203	0.26	[−0.05, 0.58]	.098
Lagged cumulative convictions	−0.06	[−0.09, −0.04]	< .001	−0.03	[−0.28, 0.22]	.816	−0.01	[−0.04, 0.02]	.625

Note. N = 503. The number of observations in each model is 9,054. Main effects are bolded, as the coefficients for confounding variables are not interpretable (Westreich & Greenland, 2013).

Table 3.

Fixed effect model estimates for the effect of change in total arrests on change in antisocial characteristics

	Antisocial Attitudes			Antisocial Behaviors			Antisocial Peers		
	β	*95% CI*	*p*	*β*	*95% CI*	*p*	*β*	*95% CI*	*p*
Intercept	2.07	[1.33, 2.69]	< .001	−0.28	[−1.2, 0.64]	.532	−0.03	[−1.01, 0.95]	.955
1-year lagged total arrests	**0.11**	**[0.07, 0.14]**	**< .001**	**0.10**	**[0.05, 0.15]**	**< .001**	**0.12**	**[0.07, 0.18]**	**< .001**
Age	−0.12	[−0.12, −0.11]	< .001	0.00	[−0.01, 0.01]	.865	0.01	[−0.01, 0.02]	.340
Race	−0.09	[−1.05, 0.88]	.850	0.23	[−0.98, 1.44]	.698	0.24	[−1.02, 1.5]	.692
2-year lagged									
Alcohol use	0.10	[0.02, 0.19]	.012	0.05	[−0.04, 0.15]	.257	0.07	[−0.05, 0.18]	.225
Marijuana use	−0.02	[−0.10, 0.07]	.706	−0.01	[−0.13, 0.12]	.885	−0.03	[−0.14, 0.07]	.516
Academic achievement	0.06	[−0.02, 0.13]	.103	0.02	[−0.06, 0.10]	.559	−0.04	[−0.14, 0.06]	.412
Antisocial attitudes	0.00	[0.00, 0.00]	.142	0.00	[0.00, 0.01]	.005	0.00	[0.00, 0.01]	.008
Antisocial behaviors	0.00	[0.00, 0.00]	.060	0.00	[0.00, 0.00]	< .001	0.00	[0.00, 0.00]	.597
Antisocial peers	0.02	[0.01, 0.02]	< .001	0.01	[0.00, 0.02]	.091	0.03	[0.02, 0.04]	< .001
Internalizing	0.00	[0.00, 0.00]	.336	0.00	[0.00, 0.00]	.976	0.00	[0.00, 0.00]	.994
Neighborhood crime	0.00	[−0.01, 0.01]	.975	0.00	[−0.01, 0.00]	.433	0.00	[−0.01, 0.01]	.979
Parent convictions	−0.04	[−0.09, 0.00]	.067	−0.02	[−0.08, 0.04]	.572	−0.04	[−0.13, 0.05]	.344
Parental stress	0.00	[−0.01, 0.01]	.655	0.00	[−0.01, 0.01]	.617	0.00	[−0.01, 0.01]	.633
Socioeconomic status	0.01	[0.00, 0.01]	< .001	0.00	[0.00, 0.00]	.684	0.00	[−0.01, 0.01]	.933
Parental supervision	0.00	[−0.03, 0.02]	.872	0.01	[−0.04, 0.05]	.753	−0.03	[−0.07, 0.02]	.238
Psychiatric institutionalization	0.01	[−0.17, 0.19]	.907	0.15	[−0.20, 0.50]	.354	0.11	[−0.23, 0.45]	.484
Delinquency institutionalization	−0.03	[−0.07, 0.01]	.105	−0.06	[−0.11, −0.01]	.024	−0.06	[−0.20, 0.07]	.304

Note. N = 503. The number of observations is 8,551. Coefficients for the 503 respondent dummy variables are not shown. Main effects are bolded, as the coefficients for confounding variables are not interpretable (Westreich & Greenland, 2013). Unadjusted models available upon request.

Table 4.

Fixed effect model estimates for the effect of change in total convictions on change in antisocial characteristics

	Antisocial Attitudes			Antisocial Behavior			Antisocial Peers		
	β	*95% CI*	*p*	*β*	*95% CI*	*p*	*β*	*95% CI*	*p*
Intercept	2.04	[1.34, 2.74]	< .001	−0.36	[−1.28, 0.56]	.432	−0.08	[−1.06, 0.91]	.872
1-year lagged total convictions	**0.03**	**[0.01, 0.05]**	**.001**	**−0.01**	**[−0.03, 0.02]**	**.509**	**0.03**	**[0.00, 0.06]**	**.042**
Age	−0.12	[−0.12, −0.11]	< .001	0.00	[−0.01, 0.01]	.777	0.01	[−0.01, 0.02]	.242
Race	−0.09	[−1.06, 0.87]	.842	0.25	[−0.97, 1.46]	.677	0.24	[−1.01, 1.49]	.691
2-year lagged									
Alcohol use	0.10	[0.02, 0.18]	.015	0.05	[−0.04, 0.15]	.264	0.06	[−0.05, 0.18]	.251
Marijuana use	−0.01	[−0.09, 0.08]	.829	0.01	[−0.12, 0.14]	.867	−0.02	[−0.13, 0.08]	.668
Academic achievement	0.06	[−0.02, 0.13]	.095	0.03	[−0.05, 0.11]	.472	−0.04	[−0.14, 0.07]	.446
Antisocial attitudes	0.00	[0.00, 0.00]	.184	0.00	[0.00, 0.01]	.002	0.01	[0.00, 0.01]	.005
Antisocial behaviors	0.00	[0.00, 0.00]	.046	0.00	[0.00, 0.00]	< .001	0.00	[0.00, 0.00]	.529
Antisocial peers	0.02	[0.01, 0.02]	< .001	0.01	[0.00, 0.02]	.079	0.03	[0.02, 0.04]	< .001
Internalizing	0.00	[−0.01, 0.00]	.287	0.00	[0.00, 0.00]	.947	0.00	[0.00, 0.00]	.940
Neighborhood crime	0.00	[−0.01, 0.01]	.987	0.00	[−0.01, 0.00]	.441	0.00	[−0.01, 0.01]	.991
Parent convictions	−0.04	[−0.09, 0.01]	.086	−0.01	[−0.07, 0.05]	.703	−0.04	[−0.12, 0.05]	.388
Parental stress	0.00	[−0.01, 0.01]	.637	0.00	[−0.01, 0.01]	.643	0.00	[−0.01, 0.01]	.658
Socioeconomic status	0.01	[0.00, 0.01]	< .001	0.00	[0.00, 0.00]	.696	0.00	[−0.01, 0.01]	.933
Parental supervision	0.00	[−0.02, 0.02]	.960	0.01	[−0.03, 0.05]	.674	−0.02	[−0.07, 0.02]	.272
Psychiatric institutionalization	0.01	[−0.18, 0.2]	.909	0.14	[−0.21, 0.49]	.394	0.11	[−0.23, 0.45]	.498
Delinquency institutionalization	−0.03	[−0.07, 0.01]	.122	−0.05	[−0.10, 0.00]	.035	−0.06	[−0.20, 0.07]	.320

Note. N = 503. CI: Confidence interval. The number of observations is 8,551. Coefficients for the 503 respondent dummy variables are not shown. Main effects are bolded, as the coefficients for confounding variables are not interpretable (Westreich & Greenland, 2013). Unadjusted models available upon request.

Table 5.

Post-hoc analysis of the strongest 10 predictors of first arrest, controlling for age

Predictor	Hazard Ratio	95% CI	*p*
Marijuana use (yes vs. no)	3.43	[2.45, 4.82]	< .001
Race (youth of color vs. white)	2.51	[1.89, 3.34]	< .001
Alcohol use (yes vs. no)	1.86	[1.34, 2.59]	< .001
Antisocial attitudes (standardized unit increase)	1.75	[1.59, 1.92]	< .001
Academic achievement (standardized unit decrease)	1.67	[1.43, 1.95]	< .001
Peer delinquency (standardized unit increase)	1.38	[1.24, 1.55]	< .001
Parental supervision (standardized unit decrease)	1.38	[1.19, 1.60]	< .001
Neighborhood crime (standardized unit increase)	1.26	[1.12, 1.41]	< .001
Socioeconomic status (standardized unit decrease)	1.23	[1.42, 1.07]	.004
Parental stress (standardized unit increase)	1.22	[1.08, 1.38]	< .001

Note. Hazard ratios from Cox proportional hazards models with robust sandwich estimator for standard errors. Details available upon request.