Is criminogenic risk assessment a prisoner of the proximate?
Challenging the assumptions of an expanding paradigm

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Abstract

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Criminogenic risk assessment, which was developed to predict recidivism, has risen to the status of "evidence-based practice" in corrections systems. As a result of its apparent success, proponents now claim that it captures the origins of criminal behavior, and can thus be leveraged to reduce correctional supervision rates and prevent crime. This dissertation investigates the validity of the these claims, by identifying and testing three assumptions requisite for the framework's expansion: 1) the evidence base for the framework's predictive performance is being interpreted correctly and appropriately, 2) the best causal models of recidivism are also the best causal models of the onset and duration of criminal behavior (and by extension, that interventions successful at reducing recidivism will be successful at reducing the onset, duration, and rate of criminal behavior); and 3) the causes of individual variation in criminal behavior are the same as causes of the population distribution, or incidence rate, of crime. This dissertation proceeds in three parts: a meta-review and critical analysis of the literature addresses the first assumption, and two empirical studies test the second and third assumptions, respectively. The meta-review determined that findings for the framework's predictive performance are inconsistent, based on inadequate or insufficient statistical information, and often overstated. The first empirical study found that each arrest, and to a lesser extent conviction, an individual experienced increased their subsequent criminogenic risk levels, raising concerns about the framework's applicability for crime prevention and etiology. The second empirical study found that criminogenic risks do not explain group differences in arrest and conviction rates, underscoring that researchers and policymakers should more cautiously communicate the scope of reform that the framework can deliver.

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Chapter 1

Introduction

> "The prediction of criminal behavior is perhaps one of the most central activities of the criminal justice system. From it stems community safety, prevention, treatment, ethics, and justice."
>
> – Andrews & Bonta, 2010, p. 299

> "Like all habitual patterns of social action, the structures of modern punishment have created a sense of their own inevitability and of the necessary rightness of the status quo…. Thus we are led to discuss penal policy in ways which assume the current institutional framework, rather than question it…. In consequence…difficult and troublesome questions no longer arise. They are authoritatively settled, at least in principle, and only matters of detail need to be concluded—details which can be left to experts and administrators in specialist institutions set aside for that purpose."
>
> – Garland, 1990, p. 3-4

One of the current "evidence-based practices" in corrections systems is what the epidemiologist Geoffrey Rose (1985) described as a *high-risk strategy*. The focus of the strategy is on identifying people at the highest risk of recidivism and targeting supervision and other resources at these individuals. This is accomplished through the use of risk assessment instruments such as the Level of Services Inventory (Andrews, Bonta, & Wormith, 2004), which were developed to quantify individual characteristics thought to predict criminal behavior. Often, programs designed to intervene upon these individual-level, *criminogenic* risk factors reduce recidivism for those at the highest risk (Cullen & Gendreau, 2001; Gendreau, French, & Gionet, 2004; Lowenkamp, Latessa, & Smith, 2006; Skeem, Manchak, & Peterson, 2011). This apparent effect on recidivism is taken as evidence that the high-risk strategy has tapped into the causes of criminal behavior more broadly, and can thus prevent crime and reduce correctional supervision rates overall. Indeed, an explanatory framework has emerged around proximate criminogenic risk factors as fundamental to the origins of criminal behavior and the roots of crime itself (Andrews & Bonta, 2010), and 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 (Desmarais & Singh, 2013; Gottfredson & Moriarty, 2006; Lowenkamp & Whetzel, 2009; Storey, Kropp, Hart, Belfrage, & Strand, 2014; Summers & Willis, 2010; Trujillo & Ross, 2008; VanNostrand & Keebler, 2009).

Despite its apparent success, the use and expansion of criminogenic risk assessment is contested terrain. Proponents see the framework as a solution to the inefficiencies of mass incarceration (Sherman, 2007). They view it as an empirically driven implementation of a rehabilitative philosophy, which reserves the most intensive supervision and treatment resources for those with the highest risk profiles, and essentially leaves alone those with low risk profiles, for whom supervision may have no benefit or even worsen criminal justice outcomes (Andrews & Bonta, 2010; Lowenkamp et al., 2006). Scores of meta-analyses and systematic reviews conclude that the evidence supports these claims. Critics, however, see criminogenic risk assessment as at best unable to reduce crime even on its own terms, and at worst reproducing or exacerbating structural inequalities under a more "objective" guise (Feeley & Simon, 1992; Harcourt, 2007). Some argue that risk assessment is gendered, racialized, and constitutive of neoliberal morality (Hannah-Moffat, 1999; Hannah-Moffat & Shaw, 2001). Others contend that the rehabilitative origins of risk assessment have been "supplanted by a managerialist approach centered on the cost-driven administration of carceral stocks and flows…" (Wacquant, 2009). However, these critiques have not always engaged directly with the empirical evidence supporting the criminogenic risk framework, or the methodological issues that inform that evidence, but rather tend to reject the framework's premises outright.

This dissertation offers a different perspective. I approach the criminogenic risk framework on its own terms, while arguing that the validity of its expansion rests on three assumptions. The three assumptions are: 1) the evidence base for the predictive performance of criminogenic risk assessment is being interpreted correctly and appropriately, 2) the best causal models of recidivism are also the best causal models of the onset and duration of criminal behavior (and by extension, that interventions successful at reducing recidivism will be successful at reducing the onset, duration, and rate of criminal behavior); and 3) the causes of individual variation in criminal behavior are the same as causes of the population distribution, or incidence rate, of crime. If any of these assumptions are not met, I will argue, expanded policy and practice based on criminogenic risk assessment will likely not change the status quo of crime or incarceration rates. I make this argument by testing aspects of the assumptions introduced above, to determine whether prototypical risk factors for recidivism withstand scrutiny after being freed from the "prison of the proximate" (McMichael, 1999) in which a high-risk strategy resides.

To begin to test the assumptions of the criminogenic risk framework, one might scrutinize factors that have been taken as evidence for its validity. Numerous meta-analyses suggest that four proximate criminogenic risk factors consistently predict recidivism: a history of antisocial behavior, antisocial personality pattern, antisocial attitudes and cognitions, and antisocial associates (Andrews, Bonta, & Wormith, 2006; Andrews, 2011; Dowden & Andrews, 1999a; Gendreau, Little, & Goggin, 1996; Simourd & Andrews, 1994). However, this evidence conceals empirical and conceptual ambiguities. These ambiguities arise because the criminogenic risk framework focuses on the very end of a process—the point at which individuals have already moved through the criminal justice system and are at risk for recidivism—and assumes that causal factors proximate to recidivism apply to the distal beginning of the process. Implicitly, by suggesting it has uncovered the origins of crime, the framework also assumes that these individual-level risk factors proximate to criminal behavior can explain differences in crime rates between populations and over time.

Yet, lessons learned from debates about the success of risk factor epidemiology provide reasons to doubt this view (McMichael, 1999; Rose, 1985). The question of whether criminogenic predictors of individual variation in recidivism can be generalized as the predictors of the onset and duration of criminal behavior, and whether these, in turn, are the same as the origins of crime itself, mirrors debates dubbed "the Epidemiology Wars" (Poole & Rothman, 1998) at the turn of the century. These debates centered around whether the appropriate role for epidemiology was to identify high-risk, susceptible people so that interventions might reduce their individual risk, or to try to identify and influence the macro-determinants of incidence (the risk of risk) in the population as a whole. In an influential paper, McMichael (1999) argued that the mind-set and methods of modern epidemiology entailed certain constraints that limited engagement with wider context, including a preoccupation with proximate risk factors, a focus on individual-level versus population-level influences on health, a typically modular, time-windowed view of how individuals undergo changes in risk status, and a blindness to complex multi-directional causal relationships (e.g., feedback loops).

There are at least four ways in which this conceptual prison of the proximate has been expressed in prior research on the criminogenic risk framework. First, because criminogenic risk factors have been developed from research on recidivism, research samples are already in contact with the criminal justice system. Evidence for the predictive and intervention utility of criminogenic risk factors for recidivism, however, may be inappropriately generalized to broader questions about criminal behavior prior to and during initial contact with the criminal justice system, i.e., its onset, duration, and related social processes. This evidence base may be inappropriately invoked in discussions of crime prevention and crime reduction. Second, current evidence for the predictive and intervention utility of criminogenic risk factors comes from studies in which the outcome (recidivism) follows the exposure (criminogenic risk) in a proximate time window. Evidence based on such proximate exposure may again be inappropriately generalized to questions of onset and duration, by conflating criminogenic risks measured immediately before recidivism with more distal risks that occur long prior to recidivism.

Third, prior research has thus far been unable to determine whether contact with the criminal justice system itself has an effect on criminogenic risk. This potential causal feedback blurs the construct validity of criminogenic risk and criminal justice outcomes. A resultant assumption is that interventions for tertiary prevention (recidivism) are generalizable to primary prevention (onset) and secondary prevention (duration). Fourth, if criminogenic risk factors are fundamental to the origins of crime itself, i.e., if they explain changes in the incidence rate of crime in addition to individual differences in criminal behavior, criminogenic risk factors and crime rates over time and between populations should correspond in empirically predictable ways, yet this has been largely unexplored.

This dissertation sets out to test the above assumptions, with attention to the above methodological issues. The project does this in three parts: a meta-review and two empirical papers that test hypotheses about criminogenic risk factors in a prospective cohort. Chapter 2 is a meta-review that critically assesses the nearly 40 meta-analyses and systematic reviews that have been conducted on the criminogenic risk framework over the past three decades. It synthesizes not only what we know about the predictive performance of the criminogenic risk framework, but also whether we interpret the framework in a way that is theoretically and empirically appropriate. Chapter 3 tests a premise of the criminogenic risk framework, by determining whether contact with the criminal justice system increases antisocial characteristics among boys followed from childhood through adolescence and early adulthood. It uses complimentary techniques for controlling confounding over time to isolate any effects of the criminal justice system on criminogenic risk factors. Chapter 4 tests whether differences in individual susceptibility to criminal behavior explain differences in exposure to the criminal justice system between groups. Using traditional mediation analysis, it tests the assumption that, if antisocial characteristics tap into the origins of criminal behavior or exposure to the criminal justice system, then a group with higher exposure to the criminal justice system should have higher levels of antisocial characteristics. Chapter 5 provides a discussion and conclusion of the dissertation's findings.

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Chapter 2

The trouble with criminogenic risk assessment: A meta-review and critical analysis

2.1 Abstract

A vast body of research has supported the ascendancy of the criminogenic risk framework. It is unclear, however, whether its empirical status is being interpreted correctly, and whether existing research provides valid evidence for its expansion from the back-end of the criminal justice system, where it was designed to predict recidivism, to the front, in pre-trial processing, sentencing, and policing. This meta-review thus attempts to answer the following questions: 1) How well does the criminogenic risk framework differentiate people who are at high risk of recidivism from those at low risk of recidivism? 2) Do reviews of criminogenic risk assessment interpret the framework in a way that is theoretically and empirically appropriate, or do they overreach? I conducted a systematic literature search of research databases and identified 38 meta-analyses and systematic reviews that met inclusion criteria. I summarized and synthesized review data, and critically assessed authors' interpretations of their findings. I found that findings for the criminogenic risk framework's predictive performance are inconsistent, based on inadequate or insufficient statistical information, and often overstated. Three thematic areas of interpretational overreach are identified and analyzed: invalid inferences from prediction to explanation, invalid inferences from criminalization to criminality, and invalid inferences from prediction to intervention. Meta-review findings suggest that we know very little about the mechanisms through which criminogenic risk factors are predictive, and reveal the limits of the framework's explanatory breadth and depth. The review concludes that there is cause for concern about transporting the framework throughout the criminal justice system.

2.2 Introduction

Over the past 25 years, risk assessment in the criminal justice system has become a predominant, "evidence-based" policy and practice, strongly promoted within expert circles of policymakers, researchers, and practitioners. Criminogenic risk assessment can be defined as the use of quantitative methods to predict an individual's criminal justice outcomes and categorize them accordingly, both to manage carceral populations through efficient and effective allocation of supervision resources and, ideally, to reduce individuals' risk through appropriate rehabilitative and social services. The first part of the definition is about quantifying certain individual characteristics thought to predict criminal behavior. Four of these risk factors—the "Big Four"—consistently predict recidivism, violence, and other criminal justice outcomes in almost any sample of people involved in the criminal justice system: a history of antisocial behavior, antisocial personality pattern, antisocial attitudes and cognitions, and antisocial associates (Dowden & Andrews, 1999; Gendreau, Little, & Goggin, 1996; Landenberger & Lipsey, 2005; Lipsey & Derzon, 1998; Simourd & Andrews, 1994). The second part of the definition is about reducing, through appropriate interventions, aspects of those risk factors that are manipulable, such as attitudes, cognitions, aspects of personality, and other criminogenic targets. It has been widely suggested that such efforts can modestly reduce recidivism rates (e.g., Andrews & Dowden, 2006; Andrews et al., 1990; Andrews, Bonta, & Wormith, 2006; Dowden & Andrews, 1999a, 1999b).

A vast body of research has supported the ascendancy of the criminogenic risk framework. As a result of its perceived success, the framework has started to move from the back-end of the criminal justice system, where it was developed to assess the risk of recidivism, to the front-end of the system, in pre-trial processing, sentencing, and policing (Desmarais & Singh, 2013; Gottfredson & Moriarty, 2006; Lowenkamp & Whetzel, 2009; Storey, Kropp, Hart, Belfrage, & Strand, 2014; Summers & Willis, 2010; Trujillo & Ross, 2008; VanNostrand & Keebler, 2009). Part of its rise can rightly be attributed to its success: after an era of resignation to the idea that "nothing worked" in criminal justice scholarship and practice (Martinson, 1974), the fact that the criminogenic risk framework was able to deliver anything at all was an improvement over the status quo.

Yet, with the field's embrace and promotion of criminogenic risk assessment, its advocates may be promising too much. Some even claim that it should characterize the proper function of the criminal justice system itself. For example, Andrews and Bonta (2010, p. 299) suggest that the prediction of criminal behavior is a central activity of the criminal justice system, because "from it stems community safety, prevention, treatment, ethics, and justice." Accordingly, Andrews, Bonta, and Wormith (2011) recommend that the framework be disseminated widely throughout the criminal justice system, and that its principles and methods be applied to other service systems. Indeed, the field has taken up this call, and the potential for the criminogenic risk framework to accomplish myriad improvements and reforms seems to know no bounds. In addition to reducing recidivism rates, proponents suggest that the framework might be able to improve sentencing procedures, facilitate jail diversion, reduce prison populations, help scale down mass incarceration without jeopardizing public safety, improve policing by helping to solve crime in real time, reduce violence, reduce corrections spending and simultaneously increase resources for community development, and ultimately, prevent crime altogether (Arnold & Arnold, 2015; Clement, Schwarzfeld, & Thompson, 2011; Jouvenal, 2016; Monahan & Skeem, 2016; Storey et al., 2014; Trujillo & Ross, 2008).

The present meta-review interrogates such claims, by charting a course between two questions, one technical and the other critical. The technical task is to determine how well criminogenic risk assessment works, in terms of predictive utility and validity. The critical task is to assess how researchers interpret the empirical performance of criminogenic risk assessment, in terms of the questions they think it addresses and problems they think it can solve. More specifically, this review will attempt to answer the following questions:

1) How well does the criminogenic risk framework differentiate people who are at high risk of recidivism from those at low risk of recidivism?
2) Do reviews of criminogenic risk assessment interpret the framework in a way that is theoretically and empirically appropriate, or do they overreach?

To date, questions such as these have remained rather isolated, part of separate literatures and separate intellectual projects. Scores of meta-analyses and systematic reviews have attempted to answer the first question, by summarizing and synthesizing vast amounts of research on the predictive utility and validity of criminogenic risk factors and particular risk assessment instruments. These reviews typically conclude that the evidence supports the continued use and expansion of criminogenic risk assessment. However, most of these analyses tend not to engage with the social implications of the framework's expansion, and tend to dismiss non-psychological theoretical insights. On the other hand, much has been written about the scientific, cultural, and political forces that brought risk assessment to the forefront in the era of mass incarceration (e.g., Feeley & Simon, 1992; Garland, 2003; Simon, 2007), and on the ways in which risk may be gendered, racialized, and constitutive of neoliberal morality (for excellent analyses, see Hannah-Moffat, 1999; Hannah-Moffat & Shaw, 2001). These critiques have not always engaged directly with the empirical evidence supporting the criminogenic risk framework, or the methodological issues that inform that evidence, but rather tend to reject the framework's premises outright.

In essence, then, "insiders" focus on the first question while bypassing the second, and "outsiders" ask a more totalizing version of the second question while bypassing the first. One side mobilizes to play a technical role in the forecasted decline of the era of mass incarceration, while the other side critiques the framework's discursive dominance with social and political analyses of what they consider a problematic approach. The present meta-review offers a different perspective. It approaches the criminogenic risk framework on its own terms, and is sympathetic to its rehabilitative and reformist origins, its pragmatic interventionist orientation, and its grounding in empirical science. However, it is also sympathetic to the social theory that has been brought to bear against the framework and assumes a sharply critical posture informed by these socio-structural considerations.

The methodology of a meta-review, which takes as its units of analysis existing meta-analyses and systematic reviews, provides the appropriate tools for the task at hand. The approach of the present paper is to employ transparent and replicable procedures to identify its units of analysis, extract and summarize basic descriptive meta-data and quantitative findings, and then move into an analysis and critique. The remainder of this study is structured accordingly. First, I lay out the methodology of the literature search, data extraction, and data summarization. Next, under "Results," I tabulate and summarize descriptive meta-data from the meta-analyses and systematic reviews. Following these results, in the "Analysis" section, I set out to answer this meta-review's two motivating questions. Regarding the first, technical question, I begin by tabulating and summarizing empirical findings from the meta-analyses and systematic reviews and then assess the strength and quality of this evidence. Regarding the second, critical question, I analyze the way that researchers interpret the criminogenic risk framework, and then identify and critique areas where the framework overreaches, or makes invalid inferences. Finally, I summarize this meta-review's limitations and offer concluding comments. To avoid confusion, I will subsequently refer to the present project's units of analysis (prior meta-analyses and systematic reviews) as "reviews" and will refer to the primary studies and data sources that constituted those reviews as "primary studies" or "studies."

2.3 Methods

2.3.1 Inclusion criteria

I conducted a systematic literature search and review to identify meta-analyses and systematic reviews that examined the predictive utility of criminogenic risk factors. Reviews were included if they were peer-reviewed, published in English language journals between 1990 and 2015, focused on a criminal justice outcome (e.g., recidivism or arrest), and focused on male subjects. Reviews were excluded if they were not meta-analyses or systematic reviews, did not include any criminal justice outcome (e.g., if the sole outcome was "violence"), focused only on non-criminogenic risk factors (e.g., psychiatric disorders), or focused only on subjects convicted of sex offenses. Based on prior experience with the criminogenic risk literature, 9 frequently cited meta-analyses or systematic reviews were flagged for inclusion *a priori*.

2.3.2 Search strategy

Reviews were identified by searching PubMed, JSTOR, Web of Science, Sociological Abstracts, and the National Criminal Justice Reference Service with combinations of the following terms: criminogenic, antisocial, deviance, delinquency, conduct problems, impulsivity, personality, prediction, intervention, treatment, screening, assessment, arrest, charge, conviction, incarceration, jail, prison, recidivism, probation, and parole. I downloaded search results into a reference management system, de-duplicated, and screened titles and meta-data to isolate meta-analyses and systematic reviews. I then screened titles and abstracts of retained reviews based on inclusion criteria to obtain a final inclusion sample for the present meta-review.

2.3.3 Data extraction and analysis

I compiled meta-data from the final sample of reviews, including publication source, journals represented, and reviews per journal. Citation information was obtained from Web of Science (Thomson Reuters, 2016). I tabulated the following information from included reviews:

publication year, search timeframe, authors, country, number of primary studies, number of primary samples, number of primary effect sizes, primary study sample setting and characteristics (e.g., community, correctional), risk assessment instruments represented, outcome definition, and major conclusions. I summarized, assessed, and synthesized review authors' interpretations of their findings based on a close reading of their research and writing. This latter analysis, and its organization into three thematic areas of inferential overreach, is qualitative.

2.4 Results

Figure 2.1 is a diagram of the flow of information through the meta-review process. The initial search yielded 10,715 records. Using the reference management software's native search capacity, articles were retained if their titles or abstracts contained the terms *meta-analysis* or *review*. This reduced the number of records to 404. I read the titles and abstracts of these 404 reviews to determine whether they met inclusion criteria, after which 38 meta-analyses or systematic reviews were retained for complete analysis. All 9 studies flagged for inclusion *a priori* were captured by the initial search strategy.

2.4.1 Meta-data and selected review characteristics

Table 2.1 provides a description of the meta-analyses and systematic reviews included in this meta-review, and Table 2.2 presents selected information from each, including disaggregated data from Table 2.1. The 38 reviews, two-thirds of which were meta-analyses, were published in 27 unique sources. Five journals published more than one meta-analysis or systematic review; *Law and Human Behavior* and *Criminal Justice and Behavior* were most frequently represented, with four and five reviews each. The vast majority of studies were peer-reviewed (N=35, or 92%). Those that were not peer reviewed appeared in books or government-sponsored publications. Primary studies from the reviews cover a half-century, from 1965-2015, and meta-analytic sample sizes (of combined participants from primary studies) ranged from roughly 2,400 to nearly 140,000, though many reviews did not report this information. Collectively, the meta-analyses and systematic reviews in this meta-review have been cited 2,729 times by other journals, according to the Web of Science. While the plurality of reviews has been cited between one and 20 times, 60% of the total citations can be attributed to only five high-impact reviews. There has also been a rapid increase in the publication of meta-analyses and systematic reviews over time, with the plurality published between 2011 and 2015. The single most reviewed risk assessment instrument was the Psychopathy Checklist (Hare & Neumann, 2006), followed by reviews that compared numerous risk assessment instruments and those that did not report which risk assessment instruments were covered in their analyses. The samples from primary studies in 84.2% of reviews were drawn from people who were involved with the criminal justice system (either adult or juvenile "offenders"). Despite this meta-review's search criteria permitting any criminal justice outcomes, the outcome investigated by nearly all reviews was recidivism. However, definitions of this construct were heterogeneous: types of recidivism were not distinguished (i.e., re-arrest, re-conviction, and technical violations were considered the same outcome), or the definition was not provided. No meta-analyses or systematic reviews used the word *cause* in reference to criminogenic risk factors, even when the emphasis was on dynamic, manipulable risk factors. One study referred to psychopathy as a cause of violent behavior (Edens, Campbell, & Weir, 2007).

2.5 Analysis

The previous section provided an overview of the criminogenic risk framework's empirical corpus. In this section, I delve into the literature to answer this review's motivating questions. First, I extract and examine the quantitative findings regarding how well the criminogenic risk framework differentiates people who are at high risk of recidivism from those at low risk of recidivism. I then assess the strength and quality of these findings. Next, I analyze how the reviews interpret their findings, and identify three areas of theoretical and empirical overreach: invalid inferences from prediction to explanation, invalid inferences from criminalization to criminality, and invalid inferences from prediction to intervention.

2.5.1 How well does the criminogenic risk framework differentiate people who are at high risk of recidivism from those at low risk of recidivism?

Table 2.3 presents meta-analytic effect size estimates and other predictive performance indicators for criminogenic risk factors and general recidivism, from the meta-analyses and systematic reviews included in the present analysis. Most meta-analyses and systematic reviews reported findings in terms of weighted point-biserial correlation coefficients or Cohen's *d* statistics, which were typically referred to as "effect sizes." For studies that reported correlation coefficients, the range of weighted mean effect size estimates for history of antisocial behavior was 0.06 – 0.35, for antisocial attitudes 0.16 – 0.2, for antisocial personality 0.18 – 0.31, and for antisocial peers 0.18 – 0.27. The range of correlation effect size estimates for demographic characteristics such as sex, racialized group membership, education/employment status was 0.05 – 0.26. As effect size estimates, point-biserial correlations are difficult to interpret because they depend on the coefficient itself and the prevalence of the outcome. However, a heuristic is that coefficients of 0.1, 0.3, and 0.5 are small, medium, and large, respectively (Rice & Harris, 2005).

Also in Table 2.3, for studies that reported weighted mean Cohen's *d,* the range of effect size estimates for history of antisocial behavior was 0.32 – 0.57, for antisocial attitudes 0.23 – 0.51, for antisocial personality 0.42 – 0.6, and for antisocial peers 0.39 – 0.41. For demographic characteristics, the range was 0.16 – 0.44. Cohen's *d* is somewhat easier to interpret, as it does not depend on the prevalence of the outcome. Cohen's *d* can be interpreted as the proportion of a standard deviation difference between two groups. Cohen's heuristic for small, medium, and large effects is 0.2, 0.5, and 0.8, respectively (Rice & Harris, 2005).

Other meta-analyses reported weighted mean effect size estimates for particular instruments overall. Table 2.3 shows that the correlation coefficient effect size estimates for the Level of Services Inventory ranged from 0.06 – 0.6, and for the Psychopathy Checklist, 0.26 – 0.28. Factor 2 of the Psychopathy Checklist, which measures antisocial characteristics, anger/aggression, and impulsivity, had a stronger effect size (0.29 – 0.32) than Factor 1, which measures callous, unemotional, narcissistic traits (0.15 – 0.18).

A small number of meta-analyses calculated the weighted mean area under the Recover Operating Characteristic curve (ROC-AUC) statistics. This statistic is interpreted as the probability that a randomly chosen individual who has recidivated would be ranked as having higher criminogenic risk than an individual who had not recidivated (Hanley & McNeil, 1982). Schwalbe (2007) calculated an ROC-AUC of 0.64 from a meta-analysis of 28 different risk assessment instrument validation studies. Whittington (2013) found a mean ROC-AUC of 0.69 from 65 studies. In a meta-analysis of 23 samples using the Level of Services Inventory and the Psychopathy Checklist, Fazel and colleagues (2012) found a mean ROC-AUC for recidivism of 0.66, a sensitivity of 0.4 (the probability that someone was assessed as high-risk given that they recidivated), a specificity of 0.8 (the probability that someone was assessed as low-risk given that they did not recidivate), a positive predictive value of 0.52 (the probability that someone will recidivate given that they were assessed as high-risk), and a negative predictive value of 0.76 (the probability that someone will not recidivate given that they were assessed as low-risk).

Eighteen of the reviews, or roughly 47%, tested for heterogeneity in meta-analytic results as a function of study characteristics such as sample composition (male/female, white/racialized group, domestic/international), study design (cross-sectional, longitudinal), source of risk assessment coding (interview/files), publication status (published/unpublished), etc. In general, these reviews found moderate to high degrees of heterogeneity that were attributable to the above characteristics. Seven reviews, or roughly 18%, discussed the quality of the primary studies that they meta-analyzed. Four of these considered study design to be a proxy for quality, and as a result two included only prospective, longitudinal designs (Bonta, Blais, & Wilson, 2014; Bonta, Law, & Hanson, 1998), and two assessed whether design moderated meta-analytic results. One of these found that design had no effect on results (Andrews & Dowden, 2006), and one found that prospective studies were more likely to obtain statistically significant results (Whittington et al., 2013). One study found that coder-rated quality of the outcome variable was positively associated with effect size (Lipsey & Derzon, 1998). Eight reviews mentioned publication bias and 6 (16%) tested for it, and found that the likelihood of publication bias was low. The finding that only 16% of reviews assessed publication bias is consistent with Singh and Fazel's (2010) meta-review, which found that only a quarter of reviews assessed for publication bias, which likely biases results in favor of positive significant findings.

2.5.1.1 Assessing the strength and quality of evidence

Table 2.4 paraphrases the primary conclusions of the reviews included in the present analysis. Based on the summaries in Table 2.4, 36.8% of the reviews concluded that evidence for predictive performance was strong, 28.9% concluded it was moderate, 7.9% concluded it was small-to-moderate, 13.2% concluded it was weak or that results should be interpreted cautiously, and 13.2% did not draw explicit conclusions. Combining the middle two categories reveals that equal proportions of reviews judged predictive performance to be strong as judged it to be small to moderate.

Thus, while over a third of the reviews judged the predictive performance of criminogenic risk assessment to be weak to modest, over a third of the reviews, including many with the highest impact, deemed it to be strong. The task at hand, then, is to assess these conflicting interpretations. Only one meta-analysis (Fazel et al., 2012) provided the information necessary to comprehensively answer the first, technical question of this meta-review, by reporting sensitivity, specificity, positive predictive value, and negative predictive value. Fazel and colleagues found that criminogenic risk assessment instruments seemed to be better at identifying people at low risk for recidivism than people at high risk for recidivism. They argued, however, that given the potential restriction of freedom triggered by criminogenic risk assessments, positive predictive values were unacceptably low: only 52% of individuals judged to be moderate to high risk went on to commit any offense. (To put this figure in perspective, 52% is virtually equivalent to flipping a coin.) However, negative predictive values were high, suggesting that criminogenic risk assessments do a good job of identifying people at low risk of recidivism (Fazel et al., 2012). The authors concluded that even after 30 years of development, the view that an individual's risk of recidivism can be predicted is not evidence-based. Although Fazel and colleagues' meta-analysis has been criticized on some methodological grounds (e.g., Olver, Stockdale, & Wormith, 2014), it remains an important and alarming analysis deserving of serious consideration.

All other meta-analyses drew conclusions about predictive utility based on point-biserial correlations, Cohen's *d,* or ROC-AUC. The vast majority relied on the former two statistics, which do not contain information about sensitivity, specificity, positive predictive value, and negative predictive value. There are a number of major problems with the use of point-biserial correlations and Cohen's *d* as measures of effect. First, both statistics are calculated with the standard deviation of at least the independent variable, and because the standard deviation is extremely sensitive to arbitrary features of study design, comparisons of these statistics across studies can confound design features with the effect of interest (Greenland, Maclure, Schlesselman, Poole, & Morgenstern, 1991; Greenland, Schlesselman, & Criqui, 1986). Greenland and colleagues (1986; 1991) have gone so far as to say that the use of such statistics, particularly the correlation coefficient, for estimating effects is never justified. This is because it depends on the marginal distributions of both the exposure and the outcome, and because it cannot be expressed as a causal contrast of a target under two exposure distributions of interest (Maldonado, 2002). Another problem is that standardized measures such as the point-biserial correlation or Cohen's *d* have no units, which makes it difficult to interpret the actual implications of an association. For example, Cohen, of Cohen's *d,* once recalled a newspaper article that reported a small but statistically significant correlation of 0.11 between children's IQ and height, but did not report that this correlation implied that a 30-point increase in IQ would require 3.5 feet of additional height, or that a 4-inch increase in height would require a 233-point increase in IQ (Cohen, 1990).

Finally, the point-biserial correlation coefficient depends on the prevalence of the outcome, which was frequently not reported in the meta-analyses and systematic reviews or the primary studies that constituted them. Of even greater concern is that a large number of reviews made conversions among correlation coefficients, Cohen's *d*, and ROC-AUC, in order to implement meta-analytic procedures, using methods that are sensitive to outcome prevalence. The problem is that they rarely reported the outcome prevalence estimates used in conversions or acknowledged that commonly cited tabular conversion charts (e.g., Rice & Harris, 2005) assume an outcome prevalence of 50%. Using a 50% prevalence, or base rate, can overestimate the converted correlation coefficient if the true base rates are lower or higher. Figure 2.2 demonstrates the instability of point-biserial correlations converted from Cohen's *d,* as a function of outcome prevalence and the magnitude of *d*. I developed this plot in R version 3.2.4, using the standard conversion formula from Rice and Harris (2005). The potential for serious bias revealed in Figure 2.2 has been comprehensively discussed in the psychology literature (e.g., McGrath & Meyer, 2006).

It became apparent during the construction of Table 2.4 that one source of variation in how reviews evaluated the strength of evidence was the involvement of particular researchers. Certain researchers tended to be authors on reviews that interpreted predictive performance to be particularly weak or strong. Some combination of Desmarais, Singh, or Fazel were authors on the reviews that deemed predictive performance to be weak. Some combination of Andrews, Bonta, or Wormith—the creators of the Level of Services Inventory—or their students and frequent co-authors (e.g., Dowden, Gendreau) were authors on almost all of the reviews that judged predictive performance to be strong. Three of the five most-cited reviews included combinations of the Level of Service Inventory's creators or students/co-authors. Andrews, Bonta, and Wormith have a proprietary interest in the Level of Services Inventory and receive royalties on sales of the instrument from its publisher, Multi-Health Systems.

It appears, then, that there are reasons to be concerned about the methodological and inferential rigor of the meta-analyses and systematic reviews in the present analysis. There is also cause for concern about the overall quality of the primary studies that constituted these reviews. Beyond the heterogeneity and study quality findings noted above, one of the well-conducted meta-analyses reviewed here found that journals that publish primary validation studies for criminogenic risk assessments do not follow standardized reporting protocols, that an author for the manual of a particular risk assessment instrument was also an author of the validation study 27% of the time, and that fewer than half of studies used more than one methodology to measure predictive validity (Singh & Desmarais, 2013). Even more troubling is that the Receiver Operator Characteristic curve was defined incorrectly in 27.8% of studies, the Area Under the Curve statistic was defined in only 50% of studies, and when it was defined, the definition was incorrect 37.5% percent of the time (Singh & Desmarais, 2013). Most alarmingly, the Area Under the Curve statistic was only interpreted in one-third of the studies, and was interpreted correctly in only 12.5% of these. The same meta-analysis found that there was enormous variation in the magnitude heuristics for interpreting Area Under the Curve as small, medium, or large. Another meta-analysis (Blair, Marcus, & Boccaccini, 2008) found an "allegiance effect" in risk assessment validation studies: effect sizes were significantly larger in validation studies conducted by instrument authors, even when the initial validation studies were excluded.

One consequence of these oversights may be the mischaracterization of certain "demographic" risk factors, such as income, employment, education, family context, etc., as less effective targets for policies and interventions. Of the nine studies that provided effect size estimates for demographic risk factors, roughly 56% found effect sizes equal to or greater than the Big Four risk factors. Table 2.3 shows that demographic risk factors generally had only slightly smaller effect sizes than the Big Four. In the meta-analyses and systematic reviews analyzed here, demographic factors did not perform much worse (and sometimes performed better) than antisocial characteristics in predicting recidivism. In reviews where demographic risk factors did have weaker associations with recidivism, this would be expected, if 1) these characteristics are causal antecedents of antisocial behaviors, attitudes, peers, and personality and the latter were included in the model and 2) samples comprised people already involved in the criminal justice system. The first point is merely that controlling for a mediator reduces the total effect of the primary exposure variable. The second point is that, in a sample of only people who are involved in the criminal justice system, the mean level of "demographic" factors may be too unfavorable, and the variance around that mean too low, for these factors to register as strong predictors of individual differences. It was not possible to fully assess these two possibilities based on information provided in the meta-analyses and systematic reviews, and would likely require closer examination of primary studies that constituted these reviews. Prior theoretical commitments and a lack of attention to sample construction and comparison groups in the majority of reviews may account for the relative undervaluing of demographic risk factors.

Thus, while empirical indicators provide relatively consistent "effect sizes" for the association between criminogenic risk factors and recidivism, the most commonly used statistics do not directly answer the first question regarding the criminogenic risk framework's ability to distinguish people at high risk of recidivism from people at low risk of recidivism. These statistics do not allow for contrasts between groups, and are difficult to interpret in the real world. And because the most common statistic—the point-biserial correlation coefficient—is unstable relative to outcome prevalence, reported correlation coefficients were often inflated. The one meta-analysis that provided the information necessary to answer this meta-review's first question—positive and negative predictive values—found that risk assessments were good at correctly identifying people at low risk of recidivism, but virtually no better than chance at identifying people at high risk of recidivism. The technical performance of criminogenic risk assessment has thus been interpreted inconsistently, and arguably, inappropriately by the framework's proponents and those with a vested interest in its dissemination. Those who judged the evidence to be moderate still supported the framework's use for recidivism prediction, and those who interpreted the framework's predictive performance to be weak and urged caution in its use were in the minority.

2.5.2 Do reviews of criminogenic risk assessment interpret it in a way that is theoretically and empirically appropriate?

In the prior section I assessed how well the criminogenic risk framework predicts recidivism. In this section I analyze how the reviews interpreted their findings, and assess whether they drew conclusions that are supported by the data. I identify three areas of theoretical and empirical overreach: invalid inferences from prediction to explanation, invalid inferences from criminalization to criminality, and invalid inferences from prediction to intervention.

2.5.2.1 Invalid inferences from prediction to explanation

The outcome in nearly all of the reviews was recidivism, and roughly 74% provided an explicit definition of this outcome. However, many of the reviews drew conclusions from their data that were not restricted to recidivism, and made inferences about crime or criminal behavior more broadly. As noted above and in Table 2.1, 58% of the reviews drew on primary studies whose samples were exclusively juvenile and adult “offenders.” In the 26% of reviews that included primary studies that had both “offender” and “non-offender” samples, recidivism outcomes were, by definition, explored only in the criminal-justice-involved portions of the samples, whereas behaviors that did not necessarily result in contact with the criminal justice system (e.g., “delinquency” or “antisocial behavior”) were studied in the non-offender portion of the samples. No reviews stated *verbatim* that findings regarding recidivism from offender samples also explained the onset of criminal behavior or first contact with the criminal justice system. However, roughly 42% discussed their theoretical orientation and findings in a way that strongly suggested that their findings may be tapping into the origins of crime or criminal behavior, and that predictors of recidivism might be applicable to the onset and duration of criminal behavior. What follows is a sampling of quotations from select reviews that illustrate this slippage:

Bonta, Blais, and Wilson (2014):

> GPCSL [General Personality and Cognitive Social Learning theory] proposes that the causes of crime are to be found within the individual and his/her social learning environment. (p. 279)

Bonta, Law, and Hanson (1998)

> Outcomes were combined to produce two criterion variables for the meta-analysis: general recidivism (criminal justice and rehospitalization, accounting for 62.8% of the correlations) and violent recidivism (criminal justice and rehospitalization, accounting for 37.2% of the correlations) (p. 126)
>
> The general findings of the current meta-analysis are consistent with broad social psychological perspectives of criminal behavior. (p. 138)

Gendreau, Little, and Goggin (1996):

> This meta-analysis extended Tittle and Meier's (1990, 1991) pessimistic conclusions regarding the social class-crime link with delinquent samples to that of adult offenders. (p. 589)
>
> These authors assert that it is absolutely essential that criminogenic needs and antisocial associates are two of the strongest correlates of criminal conduct. (p. 590)

Olver, Stockdale, and Wormith (2014):

> The Big Four and Central Eight 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, and informs methods for predicting, reducing, managing, and preventing criminal behavior. (p. 157)
>
> These considerations would suggest that the present findings are representative of a key psychometric property for which this family of tools are most frequently applied—their criterion-related validity for future recidivism. (p. 171)

Olver, Stockdale, and Wormith (2009):

> The LSI was developed from a general personality and social psychological perspective of crime (Andrews & Bonta, 2003), embodied in the Big Four covariates of criminal conduct—antisocial attitudes, antisocial associates, antisocial personality, and a history of antisocial behavior (the constellation is sometimes referred to as the Central Eight, with the inclusion of the needs areas leisure and recreation, family and marital, substance abuse, and employment and education). These covariates are linked to the origin of criminal behavior (and are hence called criminogenic needs), and services directed toward these areas of risk and need might reduce antisocial behavior. (p. 331)

Simourd and Andrews (1994):

> It should be noted here that our research and its findings focused on youth criminality (delinquency) rather than on adult criminality. (p. 26)

As shown in the above quotations, reviews involving Andrews, Bonta, Wormith, Dowden, and Gendreau tended to motivate their analyses with their "general personality and cognitive social learning" *theory of crime* or *theory of criminal behavior*, although their reviews focused on studies of recidivism, in which individuals were already involved in the criminal justice system. As shown in Table 2.4, they also tended to conclude that their findings provided evidence against sociological theories of crime, when in fact such findings are only potentially relevant to sociological theories of *recidivism.* A handful of meta-analyses and systematic reviews also incorrectly claim that their findings for recidivism prediction provide evidence that other criminological theories are "antiprediction" and "antipsychological" (e.g., Andrews & Dowden, 2006; Gendreau, Little, & Goggin, 1996).

Few reviews engaged directly with the implications of generalizing from their criminal-justice-involved sampling frames to individuals not yet involved in the criminal justice system, and thus made the extension from recidivism to crime or onset of criminal behavior without discussing the validity of the generalization. One exception is a thoughtful explanation in Cottle and colleagues (2001 p. 372), as to why their meta-analysis would focus only on recidivism, rather than both initial offending and recidivism:

> The purpose of this distinction lies in the comparability of the two offender populations. It is not feasible to make meaningful assumptions about predictors of reoffending behavior based on predictors found to be associated with first-time delinquency.… …studies examining recidivism risk factors typically are based on more homogenous samples of adolescents already identified as delinquent. Therefore, variables significantly associated with reoffending behavior in juveniles are not necessarily useful in initially distinguishing between adolescents who will or will not become delinquents.

In this passage, the authors are distinguishing their approach from that of an early and influential meta-analysis of criminogenic risk factors (Simourd & Andrews, 1994) that did not differentiate its sample. The Simourd and Andrews meta-analysis went on to help form the backbone of claims that the criminogenic risk framework was empirically superior to social-structural theories of crime, because it tapped into the origins of criminal behavior and should thus be the basis of interventions to reduce recidivism and crime writ large. As Cottle and colleagues correctly point out, however, the causes of the onset of criminal behavior are not necessarily the same as the causes of duration and recidivism.

One implication of making invalid inferences from prediction of recidivism to explanations of criminal behavior—or from the causes of recidivism to the causes of onset and duration of criminal behavior— is that reviews were blind to the potentially criminogenic role of the criminal justice system itself. None of the reviews entertained the possibility that, in primary studies of people who have already moved from the front-end to the back-end of the criminal justice system, there may be reverse causation or feedback loops if arrest, incarceration, or supervision generate or magnify antisocial characteristics. For example, Nieuwbeerta and colleagues (2009) have found that, after accounting for selection processes and "criminal propensity," first-time imprisonment was associated with an increase in criminal activity three years following release. Likewise, Shermer and colleagues (2013) examined all individuals (N > 13,000) entering more than 100 facilities in the Federal Bureau of Prisons, and found that, by examining prison fixed effects, harsher prison environments were associated with higher institutional misconduct, and argued that a portion of the predictive accuracy thought to be associated with risk assessment instruments was actually caused by facility-level endogeneity.

Thus, in nearly half of the reviews analyzed here, there appears to be slippage from what the evidence says about recidivism prediction to what researchers say about crime and criminal behavior—it's onset, duration, and origins. The same can be said regarding the difference between explaining recidivism and explaining crime. Nonetheless, it is striking that the vast majority of other meta-analyses and systematic reviews are silent on these matters altogether. This lack of engagement with the appropriate scope of inferences that can be made from data supporting the criminogenic risk framework has implications for policies, practices, and theories based on the framework's evidence base.

2.5.2.2 Invalid inferences from criminalization to criminality

By invalid inferences from criminalization to criminality (Story, 2016), I mean that reviews tended to conflate the causes of rearrest, reconviction, or the revocation of probation or parole with the causes of recidivism resulting from new crimes. In other words, reviews tended to conflate *exposure to the criminal justice system* with *criminal behavior.* As shown in Tables 2.1 and 2.2, 50% of the reviews meta-analyzed data using heterogeneous definitions of recidivism or did not report a definition of recidivism. While in many ways the heterogeneity of the outcome strengthens support for the relationship between the broad construct of criminogenic risk and the broad construct of recidivism, the nuances of this relationship are crucial for proper theory and intervention. Only two of the meta-analyses and systematic reviews acknowledged the difference between exposure to the criminal justice system and criminal behavior. The remainder of the reviews took for granted that criminal justice outcomes were the result of agential behaviors that emerged from within deviant individuals (recall the quotation above from Bonta, Blais, and Wilson [2014] that the causes of crime are to be found within individuals and their social learning environments). These concerns involve a classic agent-structure problem, and ultimately point to an issue with unelaborated mechanisms.

There are two broad categories of situation that can result in recidivism: new criminal offenses and technical violations of the terms of community supervision, e.g., missing an appointment with a parole officer. Most technical violations are not instances of criminal behavior, and there is often great discretion among individual community corrections officers and agencies about which technical violations are pursued. The first category is sometimes further delineated by the nature of the new offense (e.g., non-violent, violent, sexual, etc.) and whether an arrest results in reconviction or reincarceration. Thus, incident criminal behavior is sufficient but not necessary for recidivism. In their review, Desmarais and Singh (2013) found that of 19 risk assessment instruments validated in U.S. correctional settings, 31% of validation studies defined recidivism as a new arrest, 13% as reconviction, 10% as reincarceration, and 4% as technical violations. Furthermore, there is evidence that the definition of recidivism influences the predictive performance of risk assessment instruments. For example, the Level of Services Inventory was found to be a valid predictor of recidivism half as often when the definition was rearrest versus reincarceration (Vose, Cullen, & Smith, 2008).

Since recidivism is sometimes the result of an individual's own behaviors (committing a new crime), the proclivities of their supervision officer (discretion over revoking parole because of a missed appointment), or institutional policies and customs (the degree of specialized training provided to officers or the amount of discretion permitted), it follows that the causal mechanisms for recidivism are not uniform across these scenarios. For example, someone's impulsivity and pro-criminal attitudes may be the mechanism for committing a new robbery, but family or employment problems may be the mechanism for missing a mandated treatment session. And the disposition of a community corrections officer might supersede both of these mechanisms in some circumstances.

As Schwalbe (2008) notes in his review, none of this is important if the goal of criminogenic risk assessment is only prediction:

> As statistical prediction devices, actuarial risk assessments do not assume an underlying causal process related to recidivism. Rather, they count risk factors irrespective of the specific factors that may or may not be present for an individual case. (pp. 1368-1369)

But for *explaining* crime or criminal behavior, and *reducing* risk, enumerating the correct mechanisms of recidivism is paramount, and thus it does not follow for Schwalbe to go on to say:

> Indeed, risk assessment classifications of risk for recidivism may contribute meaningfully to judicial decisions and agency practices related to sanctioning severity and level of care for male and for female offenders. (p. 1379).

An analogous problem arises with criminogenic predictor constructs, when criminal behavior and exposure to the criminal justice system are also conflated. As noted, only two reviews recognized the conceptual and empirical distance between these constructs, both within the context of racialized discrimination in the criminal justice system. In the first, Wilson and Gutierrez (2013) compared the predictive ability of the Level of Services Inventory among Aboriginal versus non-Aboriginal offenders in Canada, and found effect measure modification between Aboriginal status and risk score: low-risk Aboriginals had a higher probability of recidivism than low-risk non-Aboriginals, but high-risk Aboriginals and non-Aboriginals had the same probability of recidivism. The authors characterized this finding as an "underclassification of low-scoring Aboriginals," but a more critical interpretation is that low-risk Aboriginals were subject to a lower threshold of policing, arrest, and sentencing, i.e., they were victims of racialized discrimination. Similarly, in a review of studies that compared risk assessments for ethnic minority and white offenders in the United Kingdom, Raynor and Lewis (2011) found that ethnic minorities consistently had significantly lower risk scores, but received the same sentences as higher-risk white offenders. The authors attributed this finding to racialized discrimination in the British criminal justice system.

Findings such as these reveal that, because crime is viewed agentially, as emerging from within deviant or abnormal individuals, criminogenic risk assessments struggle to account for distortions in the purported signal of individual differences that are in fact due to social-structural "noise." Such findings challenge, rather than support, a psychology of criminal conduct or general personality and social cognitive learning theory of crime (Andrews et al., 2006; Andrews & Dowden, 2006; Bonta et al., 2014), In fact, whether or not a person will be re-arrested or re-convicted is influenced by factors that have nothing to do with their criminogenic risk profiles, such as the way the criminal justice system responds to the color of their skin.

Indeed, the criminogenic risk framework avoids altogether basic questions about which behaviors are considered crimes and whether behaviors that are deemed criminal are surveilled, policed, prosecuted, convicted, sentenced, released, and recidivated differentially in different places or among different groups of people. Story (2016, p. 10), who provides a clear definition of the difference between criminality and criminalization, also notes its implications for criminal justice reform:

> While criminality is understood to be a state of objective deviance located in the individual, to be criminalized is to be subjectified as well as subjugated by the coercions of law enforcement and the criminal justice system, both of which are highly malleable relative to changes in laws, policy, and institutional dictates…. …Indeed, the framework of criminality as opposed to criminalization throws up a very different set of political and economic interventions.

To be clear, the point is not that criminogenic risk instruments may contain racial, gender, or other sorts of bias, but rather that, even if they do not, they may still perform unevenly across groups if they attempt to map onto individuals any potentially discriminatory operations of the criminal justice system. Calibrating individual-level risk items for the sole purpose of reducing the uneven performance of risk assessments across racialized groups, as Wilson and Gutierrez (2013) suggest, without addressing institutional sources of disparities, may thus have diminishing practical and explanatory returns.

It is striking that all but two of the meta-analyses and systematic reviews do not attend to this reality. Instead, they imply that the question *Why do some people engage in criminal behavior more than others?* is the same as the question *Why do some people come into more contact with the criminal justice system than others?* Conflating these questions has implications for the types of interventions that are prioritized. In this case, it means targeting individuals rather than systems. Given statements such as the following from the framework's originators, this conflation is perhaps not surprising:

> The risk principle of case classification relates not to the retributive or deterrent aspects of justice but to the objective of reduced reoffending through rehabilitative programs. Let justice be done and let the just penalty be set, the just obligations be established, and the just decisions be made. The risk principle of human service becomes relevant when, in that just context, interest extends to public protection through the delivery of human services (Andrews & Dowden, 2006, p. 90).

In other words, advocates of the criminogenic risk framework take as a premise that the criminal justice system is just. If there are unjust distortions, they are distinct from the framework because they belong to the system as a whole. According to this view, it is not a problem for the criminogenic risk framework to conflate criminality with criminalization. But if criminogenic risk assessment becomes a central, characteristic activity of the criminal justice system—which is what its proponents advocate (e.g., Andrews & Bonta, 2010, p. 299)—then that conflation becomes normative, and undermines the basic premise that the criminal justice system is a neutral background condition. The intellectual indifference implied by the above quotation thus becomes untenable.

2.5.2.3 Invalid inferences from prediction to intervention

A major contribution of the criminogenic risk framework is its potential to reduce risk, not merely predict recidivism. This potential stems from its identification and quantification of dynamic, manipulable factors that are taken, either implicitly or explicitly, to be causes of criminal behavior or recidivism—this is the case whenever proponents of the criminogenic risk framework switch from talking about prediction to talking about risk reduction through the provision of services and programs. Below is a sampling of quotations from select reviews that illustrate this language:

Bonta, Blais, and Wilson (2014):

> The importance of these dynamic risk factors is that, in addition to being predictive of criminal behavior, they can serve as targets for treatment programming. Treatments that successfully address these dynamic risk factors or criminogenic needs are associated with reduced recidivism (p. 280)

Dowden and Brown (2002):

> Changes in dynamic factors achieved through treatment that are subsequently linked to reductions in recidivism are known as criminogenic needs. (p. 243)

Gendreau, Little, and Goggin (1996):

> Moreover, the design of effective offender treatment programs is highly dependent on knowledge of the predictors of recidivism (p. 575)…Dynamic risk factors, or what Andrews and Bonta commonly refer to as criminogenic needs (e.g., antisocial cognitions, values, and behaviors), are mutable and thus serve as the appropriate targets for treatment (p. 575)

Olver, Stockdale, and Wormith (2014):

> The Big Four and Central Eight underpin a general personality and cognitive social learning theory of criminal behavior that…informs methods for predicting, reducing, managing, and preventing criminal behavior. (p. 157)

Olver, Stockdale, and Wormith (2009):

> Although the prediction of adult criminal recidivism is important and interesting, some have argued (Douglas & Kropp, 2002), and we concur, that the ultimate purpose of risk assessment should be the prevention as opposed to the prediction of criminal recidivism. (p. 346)

Vose, Cullen, and Smith (2008):

> This theory argues that interventions should target for change empirically established predictors of recidivism (such as antisocial peers, antisocial attitudes, and antisocial personality. (p.23)…Given the fact that the LSI includes a number of dynamic items, a reduction in an offender's total LSI score should occur after the offender has received treatment services appropriate for his or her risk…. (p. 27)

Wilson and Gutierrez (2013):

> …fourth-generation risk assessments (e.g., Level of Service/Case Management Inventory; Service Planning Instrument), which encompass a more comprehensive actuarial assessment of an offender's risk to reoffend that also facilitates the development of an intervention plan. (p.197)

Beyond statements such as these and references to a promising program evaluation literature, the reviews analyzed in the present meta-review tended not to engage directly with the ramifications of their causal assumptions about the effectiveness of intervening on criminogenic risk factors such as the Big Four. When they did, reviews tended to distance themselves from causal claims, such as Schwalbe's (2008) quotation above about actuarial risk assessments not assuming an underlying causal process related to recidivism. This is ironic given the emphasis the framework places on the importance of interventions on dynamic, manipulable risk factors. An intervention on non-causal factors will likely not have intended effects, but even when an intervention on purported causal factors has intended effects, this does not entail that those factors were causal. Interventions can work through multiple pathways that bypass the factor of interest. Indeed, there is an increasing recognition in the methods literatures of biostatistics and epidemiology that *how* a purported cause is manipulated can make or break its interpretability. Successful interventions require knowledge of causal mechanisms, but intervention effects and causal effects are only equivalent under strict assumptions (Gatto, Campbell, & Schwartz, 2014; Greenland, 2005; Hernán & Taubman, 2008; Hernán & VanderWeele, 2011; Pearl, 2014).

The reviews in the present study did not discuss these issues. While numerous analyses have been conducted on the effectiveness of interventions that target criminogenic risk factors to reduce recidivism, these studies tend to find small to moderate effects and have not confirmed hypotheses about mechanisms of action (e.g., Andrews & Dowden, 2006; Lowenkamp, Latessa, & Holsinger, 2006). In fact, intervention effects are significantly larger when programs are combined with other services, such as mental health counseling, employment and vocational training, and educational programs (Landenberger & Lipsey, 2005). Evidence for the risk reducing potential of the criminogenic risk framework is thus suggestive, but modest, and there is very little evidence that recidivism reduction is achieved by reducing the Big Four criminogenic risk factors per se, rather than more general therapy outcomes combined with real improvements in the material conditions of people's lives. The assumptive transition, then, in many of the reviews analyzed in this meta-review, from risk prediction to risk reduction, is not yet supported by the data.

One possible explanation for this disjunction may be that theory-driven tests of explicit causal models have not been part of criminogenic risk framework's ethos. This may be because the psychology of criminal conduct and the general personality and cognitive social learning theory of criminal behavior were developed with a "radical empirical approach to building theoretical understanding" (Andrews & Bonta, 2010, p. 132). That is to say, because the theory was developed to fit the data, rather than proposed *a priori* and then subjected to empirical confirmation, the nuances of a fully elaborated causal model were never staked out.

At the very least, the state of evidence on criminogenic risk reduction does not seem to warrant claims from the originators of the framework that "the theoretical and empirical base of RNR-based human service should be disseminated widely for purposes of enhanced crime prevention throughout the justice system and beyond (e.g., general mental health services)" (Andrews, Bonta, & Wormith, 2011). While it is easy to understand such sentiments—the criminogenic risk framework has had more success than any approach to recidivism reduction that came before it—existing evidence does not speak to its efficacy beyond tertiary prevention, and certainly not to other social services. To begin to entertain such notions, methodological, definitional, and inferential problems discussed above must be systematically addressed, and a complete causal model that elaborates the antecedents, confounders, and mediators of criminogenic risk factors must be subjected to explicit hypothesis testing in appropriate samples.

2.6 Limitations

The present meta-review is limited in the following ways: First, I was solely responsible for screening studies against inclusion criteria and then for extracting data from retained studies. It is thus possible that, despite systematized procedures, there were undetected errors in which studies were included or excluded, and how data were extracted. Second, the meta-review was limited in its ability to quantify its synthesis of findings across meta-analyses and systematic reviews. Statistical methods have not been developed to combine the results of meta-analyses, due to the likely repetition of primary data sources and reproduction of study error (Singh & Fazel, 2010). This meta-review is thus constrained by the methodological deficits of its constituent meta-analyses and systematic reviews. Third, despite being firmly grounded in epidemiologic principles and methodology, and rudimentary social theory, the analytic and critical components of this review are limited by the subjectivity, inherent biases, conceptual orientation, and political and normative perspectives of the author. Its findings should thus be understood in that context.

2.7 Conclusion

This meta-review analyzed 38 meta-analyses and systematic reviews on the predictive performance of criminogenic risk factors such as history of antisocial behavior, antisocial attitudes and cognitions, antisocial personality, and antisocial peers. As a review of reviews, this study represents a synthesis of knowledge generated from thousands of studies, of hundreds of thousands of participants, carried out over the course of a half-century. This meta-review provides a bird's eye view of not only what we know about criminogenic risk assessment, but how we understand and interpret that knowledge.

The findings of this analysis reveal that we know a great deal about which factors are associated with recidivism among people who have already come into contact with the criminal justice system. We know in very broad terms about the magnitude of the framework's predictive performance, but we interpret this knowledge inconsistently and inappropriately. We know comparatively little about false positives, false negatives, and other metrics derived from these measures. We know even less about how and to what effect decisions about sensitivity, specificity, and positive and negative predictive values are implemented and evaluated in the field, only that these metrics are poorly understood by researchers in the rare cases they are even considered. We know very little about the mechanisms through which criminogenic risk factors are predictive, and even less about the mechanisms through which interventions that target these factors operate. We do know that these interventions have small to modest success in reducing recidivism, especially when combined with other social services. We know very little about the generalizability of the framework, though there are reasons to be concerned about transporting it to the front-end of the criminal justice system. Finally, this meta-review reveals that we often talk about the framework in ways that are not supported by evidence, and are limited in theoretical breadth and depth.

The present meta-review also identified a number of questions for future research. First, are the causes of onset and duration of criminal behavior the same as the causes of recidivism? That is, what is the empirical landscape in representative samples that can assess individuals before their first contact with the criminal justice system? Second, what is the effect of contact with the criminal justice system on individuals' antisocial characteristics? In other words, are there instances of reverse causation or feedback loops, such that contact with the criminal justice systems increases antisocial characteristics? Third, how are proximate risk factors such as antisocial attitudes and antisocial personality connected to more distal, yet manipulable risk factors? That is, what is the structure of confounding and mediation in a fully elaborated causal model of criminogenic risk? Finally, and related to the third question, what are the mechanisms of recidivism reduction in programs that target criminogenic risk factors? In other words, what are the active ingredients of these programs, and would they be the same active ingredients if the criminogenic risk moves to the front-end of the criminal justice system?

This meta-review set out to assess the state of knowledge surrounding the criminogenic risk framework, from a perspective that was both sympathetic and critical. As the framework gains discursive hegemony at the same time that the criminal justice system inches toward the precipice of reform, it is essential that we are clear about what the evidence does and does not say, in order to resist the hubris of overreach and to prevent the production or reproduction of harmful, unintended consequences. While much is known about the prediction of recidivism and criminal behavior, targeted, strategic, and theory-driven research on the mechanisms of prediction and successful interventions—both individual and structural—is paramount as the field moves forward.

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2.9 Figures and tables

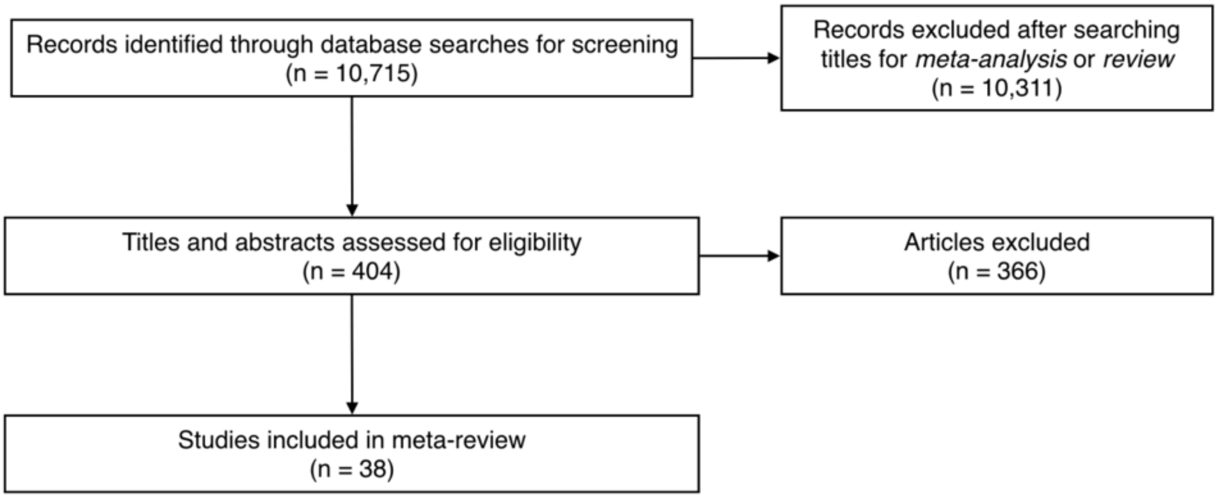

Figure 2.1. Diagram of the flow of information through the different phases of the meta-review

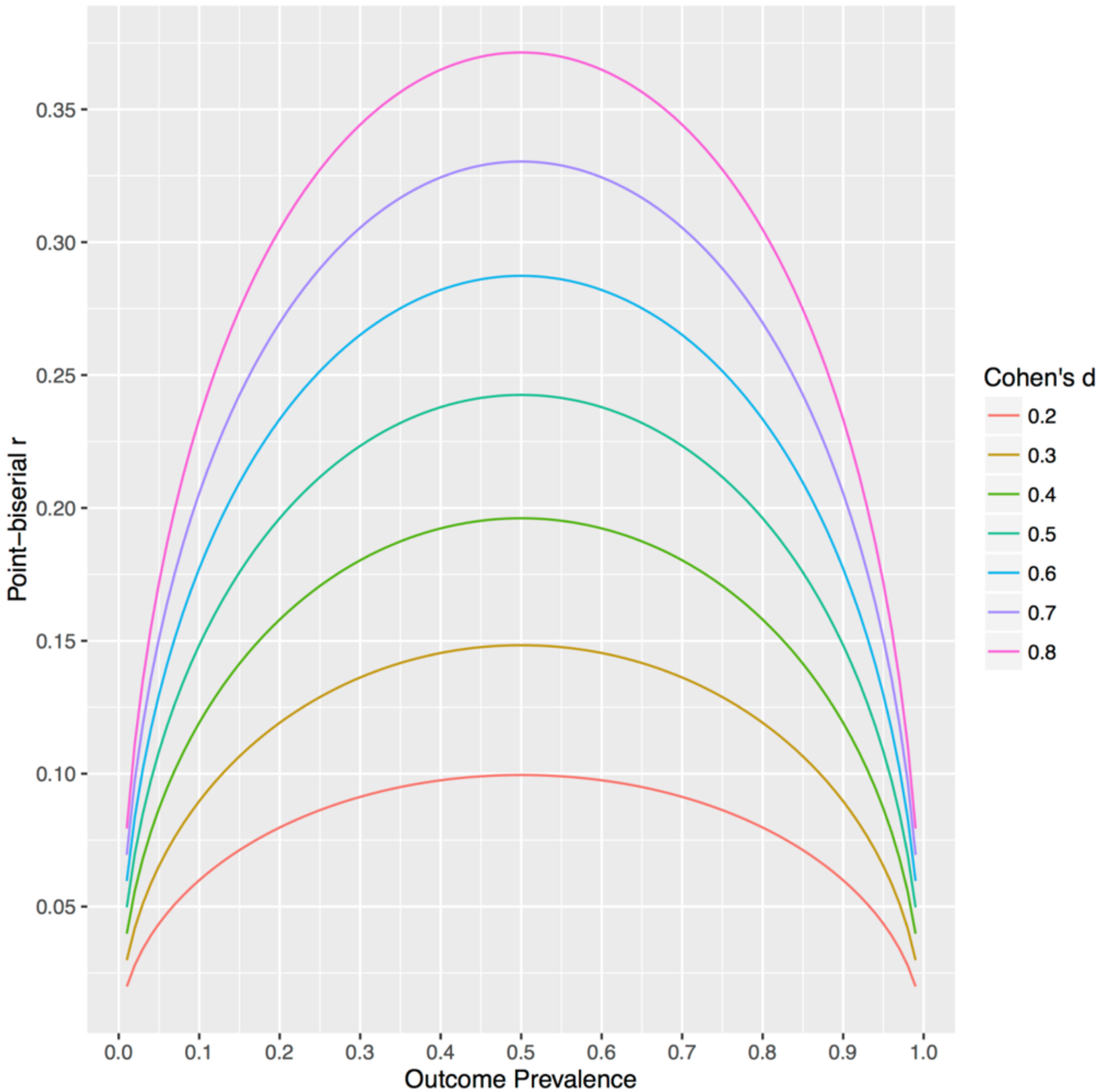

Figure 2.2. Instability of the conversion of point-biserial correlations from Cohen's *d*, as a function of outcome prevalence (i.e., base rate) and the magnitude of *d*.

Table 2.1. Meta-description of included meta-analyses and systematic reviews

	N	%
Studies included in meta-review	38	
Unique publications sources	27	
Study type		
Meta-analysis	25	65.8
Meta-regression	1	2.6
Meta-review	1	2.6
Systematic review	7	18.4
Narrative review	4	10.5
Publication source frequency		
One study	23	
Two studies	2	
Three studies	1	
Four studies	2	
Five studies	1	
Peer reviewed		
Yes	34	89.5
No	4	10.5
Cited by	2,729	
1 – 20	13	34.2
21 – 40	4	10.5
41 – 60	4	10.5
61 – 80	3	7.9
81 – 100	2	5.3
101 – 200	2	5.3
200 – 500	5	13.2
Not available	5	13.2
Top five most-cited articles	1645	60.3
Cottle, Lee, & Heilbrun, 2001	206	7.5
Leistico, Salekin, DeCoster, & Rogers, 2008	220	8.1
Andrews, Bonta, & Wormith, 2006	286	10.5
Bonta, Law, & Hanson, 1998	427	15.6
Gendreau, Little, & Goggin, 1996	506	18.5
Year of publication		
1990 – 2000	7	18.4
2001 – 2005	5	13.2
2006 – 2010	11	28.9
2011 – 2015	16	42.1
Risk assessment instruments†		
Many	13	34.2
Level of Services Inventory	4	10.5
Psychopathy Checklist	8	21.1
Other	3	7.9
Not reported	11	28.9
Not applicable	3	7.9
Sample characteristics		
Offenders	16	42.1
Juvenile offenders	6	15.8
Offenders and community	10	26.3
Not reported	3	7.9
Not applicable	3	7.9

	N	%
Outcome definition		
Any recidivism	13	34.2
General recidivism	3	7.9
Violent recidivism	3	7.9
General and violent recidivism	3	7.9
Any or violent recidivism	3	7.9
Any re-arrest or re-conviction	3	7.9
Violent and sexual reoffending	1	2.6
Not reported	6	15.8
Not applicable	3	7.9

Note: Percentages are of the 38 studies included in this meta-review unless otherwise noted.
* Percentage of the 2,729 total citations
† Some studies counted in multiple categories, e.g., they reported the LSI and PCL

Table 2.2. Summary of meta-analysis and systematic review publication characteristics, designs, and samples

Authors	Publication Year	Study Type	Peer Reviewed	Cited by	Search Years	N	# Studies	# Samples	# Effect sizes
Andrews & Dowden	2006	Narrative review	Y	83	NA	NA	NA	NA	NA
Andrews et al.	2006	Narrative review	Y	286	NA	NA	NA	NA	NA
Asscher et al.	2011	Meta-analysis	Y	26	1990-2010	10,073	53	60	NR
Bonta, Blais, & Wilson	2014	Meta-analysis	Y	10	1959-2011	23,900	126	96	NR
Bonta, Law, & Hanson	1998	Meta-analysis	Y	427	1959-1995	NR	NR	64	548
Campbell, French, & Gendreau	2009	Meta-analysis	Y	70	1980-2006	40,944	88	NR	185
Cottle et al.	2001	Meta-analysis	Y	206	1983-2000	15,256	23	22	30
Davison & Janca	2012	Narrative review	Y	4	NA	NA	NA	NA	NA
Desmarais & Singh	2013	Systematic review	Y	NA	1970-2012	NR	53	NR	NR
Dolan & Doyle,	2000	Narrative review	Y	5	NR	NA	NA	NA	NA
Dowden & Andrews	1999	Meta-analysis	Y	NA	NR	NR	134	NR	229
Dowden & Brown	2002	Meta-analysis	Y	29	1950-1998	84,578	45	NR	116
Edens, Campbell, & Weir	2007	Meta-analysis	Y	126	1990-2005	2,867	21	21	NR
Fazel, Singh, Doll, & Grann	2012	Meta-analysis	Y	54	1995-2011	24,847	68	73	NR
Gardner, Boccaccini, Bitting, & Edens	2015	Meta-analysis	Y	1	1998-2015	~7,800	30	NR	28
Gendreau, Andrews, Goggin, & Chanteloupe	1992	Meta-analysis	N	NA	1970-1991	NR	372	NR	1,734
Gendreau et al.	1996	Meta-analysis	Y	506	1970-1994	NR	131	NR	1,141
Gutierrez, Wilson, Rugge, & Bonta	2013	Meta-analysis	Y	13	1988-2010	NR	32	49	1,908
Kennealy, Skeem, Walters, & Camp	2010	Meta-analysis	Y	48	1992-2008	10,555	26	NR	32
Leistico et al.	2008	Meta-analysis	Y	220	1965-2004	15,826	95	NR	NR
Lipsey & Derzon	1998	Systematic review	N	NA	1960-1990	NR	34	NR	793
Mokros, Vohs, & Habermeyer	2014	Meta-analysis	Y	9	2005-2012	2,412	11	NR	NR
Olver, Stockdale, & Wormith	2014	Meta-analysis	Y	8	1981-2012	13,7931	128	151	NR
Olver, Stockdale, & Wormith	2009	Meta-analysis	Y	11	1990-2008	8,746	49	44	NR
Raynor & Lewis	2011	Narrative review	Y	93	2001-2006	NA	7	NA	NA
Schwalbe	2008	Meta-analysis	Y	2	1998-2007	NR	19	20	25
Schwalbe	2007	Meta-analysis	Y	45	1988-2006	53,405	28	33	42
Simourd & Andrews	1994	Meta-analysis	N	77	NR	NR	60	NR	464
Singh & Desmarais	2013	Systematic review	Y	NA	1990-2011	NR	47	25	NA
Singh & Fazel	2010	Meta-review	Y	10	1995-2009	NR	40	NA	NA
Vose et al.	2008	Systematic review	Y	20	1982-2008	NR	47	NR	NR
Walters	2012	Meta-analysis	Y	45	1997-2011	NR	6	7	NR
Walters	2003a	Meta-analysis	Y	10	1985-2001	NR	50	NR	62
Walters	2003b	Meta-analysis	Y	67	1985-2001	NR	42	NR	50
Watt, Howells, & Delfabbro	2004	Narrative review	Y	152	NA	NA	NA	NA	NA
Whittington et al.	2013	Systematic review	Y	24	NR	NR	959	NA	NA
Wilson & Gutierrez	2014	Meta-analysis	Y	3	1988-2010	NR	12	16	1,186
Yu, Geddes, & Fazel	2012	Meta-regression	Y	16	1966-2009	>10,000	14	NR	NR

Table 2. Continued

Authors (repeated)	Publication Year	Risk Assessment Instrument	Offender status	Recidivism definition
Andrews & Dowden	2006	NR	NR	NR
Andrews et al.	2006	NR	Offenders	Any
Asscher et al.	2011	Many	Offenders/Community	Re-arrest or re-conviction
Bonta, Blais, & Wilson	2014	NR	Offenders	Any
Bonta, Law, & Hanson	1998	NR	Offenders	Re-arrest or re-conviction
Campbell et al., 2009	2009	Many	Offenders	Violent
Cottle et al.	2001	NR	Offenders	General
Davison & Janca	2012	NA	NA	NA
Desmarais & Singh	2013	Many	NA	NA
Dolan & Doyle,	2000	Psychopathy Checklist	NR	Violent
Dowden & Andrews	1999	NR	Juvenile offenders	NR
Dowden & Brown	2002	NR	Offenders	General and violent
Edens, et al.	2007	Psychopathy Checklist	Juvenile offenders	General and violent
Fazel, et al.	2012	Many	NR	Any
Gardner, et al.	2015	Many	Offenders	Any
Gendreau, et al.	1992	NR	Offenders	Any
Gendreau et al.	1996	Many	Offenders	NR
Gutierrez, et al.	2013	NR	Offenders	Any or violent
Kennealy, et al.	2010	Psychopathy Checklist	Offenders	Violent
Leistico et al.	2008	Psychopathy Checklist	Offenders/Community	Any
Lipsey & Derzon	1998	NA	Offenders/Community	Any
Mokros, et al.	2014	Psychopathy Checklist	Offenders	Violent and sexual
Olver, et al.	2014	Level of Services Inventory	Offenders	Any
Olver, et al.	2009	Level of Services Inventory, Psychopathy Checklist, Structured Assessment of Violence Risk in Youth	Juvenile offenders	Any
Raynor & Lewis	2011	Many		NA
Schwalbe	2008	NR	Juvenile offenders	General
Schwalbe	2007	Many	Juvenile offenders	Re-arrest or re-conviction
Simourd & Andrews	1994	NR	Juvenile offenders	NR
Singh & Desmarais	2013	25 instruments	Offenders/Community	NR
Singh & Fazel	2010	Many	Offenders/Community	NR
Walters	2012	Psychological Inventory of Criminal Thinking Styles	Offenders	General and violent
Vose et al.	2008	Level of Services Inventory	Offenders	Any
Walters	2003a	Psychopathy Checklist, Lifestyle Criminality Screening Form	Offenders/Community	Any
Walters	2003b	Psychopathy Checklist	Offenders/Community	Any
Watt, Howells, & Delfabbro	2004	NA	NA	NA
Whittington et al.	2013	Many	Offenders/Community	Any
Wilson & Gutierrez	2014	Level of Services Inventory	Offenders	Any or violent
Yu, Geddes, & Fazel	2012	Many	Offenders/Community	Any or violent

Table 2.3. Meta-analytic effect sizes and other performance indicators for criminogenic risk factors and general recidivism

Study	History of antisocial behavior	Antisocial attitudes	Antisocial personality	Antisocial peers	Demographics	LSI Total	PCL Total	PCL Factor 1	PCL Factor 2
Asscher et al. , 2011	*0.32*	*0.37*	*0.42*						
Bonta, et al., 2014	*0.5*	*0.51*	*0.56*		*0.17 - 0.42*				
Bonta et al. , 1998	0.08	0.07			0.12				
Desmarais et al., 2013						0.24 - 0.36			
Campbell et al., 2009	5 instruments, 0.22 – 0.32								
Cottle et al. , 2001	0.06 - 0.35			0.2	0.03 - 0.23				
Edens et al., 2007						0.25	0.27	0.18	0.29
Fazel et al., 2012						ROC-AUC = 0.66 Sensitivity = 0.4 Specificity = 0.8 Positive Predictive Value = 0.52 Negative Predictive Value = 0.76			
Gardner, et al., 2015	*0.23 - 0.31*								
Gendreau, et al., 1992	0.22	0.16	0.19	0.27	0.06 - 0.18				
Gendreau et al., 1996	0.18	0.18	0.18	0.18	0.05 - 0.16				
Gutierrez, et al., 2013	*0.44*	*0.36*	*0.51*	*0.41*	*0.16 - 0.43*				
Kennealy, et al., 2010								OR = 1.04	OR = 1.15
Leistico et al., 2008							*0.55*	*0.38*	*0.6*
Lipsey & Derzon, 1998	0.09 - 0.27			0.04 - 0.43	0.09 - 0.26				
Mokros, et al., 2014						*0.29 - 0.76*			
Olver et al., 2014	0.28	0.19	0.31	0.22	0.12 - 0.24	0.29			
Olver et al., 2009						0.32	0.28		
Schwalbe, 2008						0.32 - 0.4			
Schwalbe, 2007	28 instruments, Mean ROC-AUC = 0.64								
Simourd & Andrews, 1994		0.39 - 0.4			0.06 - 0.24				
Vose et al., 2008						0.07 – 0.6			
Walters, 2012		Cognitions: 0.2							
Walters, 2003a							0.26		
Walters, 2003b								0.15	0.32
Whittington et al., 2013						ROC-AUC = 0.69			
Wilson & Gutierrez, 2014	*0.57*	*0.39*	*0.6*	*0.39*					
Yu, Geddes, & Fazel, 2012			OR = 2.4						

Note. Italicized numbers are Cohen's *d*, non-italicized numbers are correlation coefficients. OR: Odds ratio. LSI: Level of Services Inventory. PCL: Psychopathy Checklist. Factor 1 represents callous/unemotional/narcissistic. Factor 2 represents antisocial, anger/aggression, impulsivity.

Table 2.4. Main conclusions about the predictive performance of criminogenic risk factors and assessment instruments from 37 meta-analyses and systematic reviews

Study	Conclusions	Strength
Andrews, 2006	Overall, the results from the present meta-analysis provided solid support for the risk principle. This report is the first extended meta-analytic survey with a focus on the risk principle and the first to document the significant dampening of the magnitude of the risk effect as a function of having to rely on aggregate categorizations of the risk level of cases.	Strong
Andrews et al., 2006	The promise of 4G assessments is that linkages among assessment and programming, and of each with reassessments, and ultimate outcome will be very rewarding in theory and practice. The value of the assessments resides in planning and delivering effective service. …greatly enhance clinical supervision of direct contact staff members.	Strong
Asscher et al. , 2011	…moderate relationships between psychopathic traits in juveniles and (later) delinquent behavior and (violent) recidivism. Sample type moderated the relationship between psychopathy and (violent) recidivism, with the largest effect sizes for samples combining offenders and non-offenders. This result is not surprising, as the variation in both psychopathy and delinquency is likely to be largest in these samples, which can result in higher correlations. …the present meta-analysis indicates that early signaling of psychopathy can be useful, because delinquent behavior and recidivism are moderately related as early as the transition from middle childhood to adolescence.	Moderate
Bonta, 2014	For mentally disordered offenders, in general, the Central Eight risk/need factors were better predictors of both general and violent recidivism than the clinical factors. Contrary to established findings among general offenders, we did not find the Big Four as standing apart from the other Central Eight risk/need factors, at least in the prediction of general recidivism. The only clinical variables that significantly predicted recidivism were intelligence for general recidivism and antisocial personality/ psychopathy for both types of recidivism. Although no support was found for prioritizing the Big Four in the prediction of general recidivism and mild support in the prediction of violent recidivism, more research is needed before a final conclusion can be reached. Finally, the validity of the Central Eight for risk assessment also suggests that targeting these risk/need factors in treatment would lead to reduced recidivism.	Strong
Bonta, 1998	…the predictors of recidivism among mentally disordered offenders were almost identical to the predictors found among nondisordered offenders. This conclusion held for both general and violent forms of recidivism. …a case can be made to apply what is known about general offender risk assessment to the risk assessment of mentally disordered offenders. …these results strongly suggest that risk assessments of mentally disordered offenders should pay close attention to the general offender prediction literature. Clinical variables and clinical judgments contribute minimally in the prediction of recidivism. Social psychological theories suggest that the most effective programs for reducing recidivism are those that target needs closely related to criminality, for example, procriminal attitudes, criminal associates, and unstable lifestyle. Finally, the findings also speak to the limited utility of sociological criminology in risk prediction. The major explanatory concepts in many criminological theories pertain to indicators of social position. Two of the key indicators are class and race. Neither of these two variables predicted general recidivism, but race did predict violent recidivism. Although age and gender are considered by some theories as indicators of social position, these factors may more properly be subsumed under biological theories of crime. The results support the theoretical perspective that the major correlates of crime are the same, regardless of race, gender, class, and the presence or absence of a mental illness.	Strong
Campbell et al., 2009	…moderate ability to predict risk outcomes consistent with estimates reported in other risk prediction meta-analyses. …predicted violent recidivism with at least a moderate degree of success. Although this analysis found little difference among the predictive validities of actuarial and structured instruments for violent reoffending, this does not mean that they would be equally informative for case planning when the goal is risk reduction.	Moderate
Cottle et al. , 2001	…the strongest individual predictors to be a younger age a first commitment, younger age at first contact with the law, and history of nonsevere pathology. …the domains of offense history and family and social factors were consistently associated with recidivism…. The sample of participants ...is considerably more homogenous than it tends to be in delinquency research with first-time or nonoffenders. The present meta-analysis sample consisted entirely of adolescents who had already been adjudicated delinquent at least once. This may account for some of the results, including the low correlations between recidivism and variables such as substance use, school attendance and achievement, and history of treatment. The accurate identification of higher risk individuals and the ongoing assessment of changing risk status could be useful for decision makers in program planning, resource allocation and legislation and policy affecting juveniles.	Moderate
Davison, 2012	There is now much evidence that personality disorder is related to offending. …some personality disorders other than antisocial are related to particular types of offending behaviour. …although rates of personality disorder are high in all serious offenders, the role played by personality disorder may be greater in some offences than others…. These types of studies are only able to show an association between personality disorder and offending but tell us nothing of the causal link.	Strong

Desmarais, 2013	There were very few U.S. evaluations examining the predictive validity of assessments completed using instruments commonly used in U.S. correctional agencies. In most cases, validity had only been examined in one or two studies conducted in the United States, and frequently, those investigations were completed by the same people who developed the instrument. Also, only two of the 53 studies reported evaluations of inter-rater reliability. There was no one instrument that emerged as systematically producing more accurate assessments than the others. Performance within and between instruments varied depending on the assessment sample, circumstances, and outcome. …it is important to remember that the goal of risk assessment is not simply predict the likelihood of recidivism, but, ultimately, to reduce the risk of recidivism. To do so, the risk assessment tool must be implemented in a sustainable fashion with fidelity; findings of the risk assessment must be communicated accurately and completely; and, finally, information derived during the risk assessment process must be used to guide risk management and rehabilitation efforts.	Weak
Dolan, 2000	This review indicates that structured clinical judgment and systematic risk assessment scales should be used cautiously and judiciously. The assessment tools chosen, and how to interpret the scores, will largely be influenced by the populations or settings and the questions we want answered.	Weak
Dowden, 1999	…strong empirical support for the applicability of the principles of human service, risk, need and responsivity for young offenders. …increased adherence to these principles is associated with increased reductions in reoffending. …clinically relevant and psychologically informed approaches to reducing recidivism, outlined by many of the scholars of the rehabilitation literature, are indeed effective for young offender populations	Strong
Dowden, 2002	…a combined drug/alcohol abuse category alongside exclusive drug abuse demonstrated the strongest predictive power followed by parental substance abuse history and alcohol abuse …substance abuse factors play an important role in predicting recidivism. However, care should be taken to ensure that several substance abuse factors are examined as some are clearly better predictors than others. In fact, it appears that among those substance abuse factors examined to date, drug abuse may be the strongest single predictor of recidivism. Recall, that Gendreau et al. (1996) reported that substance abuse was one of the weakest predictors of recidivism compared to other criminogenic factors. Interestingly, this study demonstrates that drug abuse rather than substance abuse per say, is equally important as criminal associates, criminal attitudes, education and employment in the enterprise of risk prediction. This information has the potential to significantly augment the predictive utility of several existing risk assessment instruments.	Strong
Edens, 2007	…the relationship between psychopathy and both general and violent recidivism among male adolescents is statistically significant and of a magnitude that borders on what Cohen conventionally would define as a "medium" effect. …the moderate to severe heterogeneity observed among the obtained effects indicates a lack of consistent results across studies…. …the magnitude of these effects, despite being significant, indicates the vast majority of variability in recidivism remains to be explained by factors other than psychopathy. …psychopathy was significantly associated with both general and violent recidivism among male youths. …moderate to severe degree of heterogeneity noted among the effect sizes, the very modest effects for female offenders and for sexual reoffending, and the possibility that psychopathy may be less predictive among ethnically diverse samples of juvenile offenders….	Moderate
Fazel, 2012	…even after 30 years of development, the view that violence, sexual, or criminal risk can be predicted in most cases is not evidence based. … there was heterogeneity in the performance of these measures depending on the purpose of the risk assessment. If used to inform treatment and management decisions, then these instruments performed moderately well in identifying those individuals at higher risk of violence and other forms of offending. However, if used as sole determinants of sentencing, and release or discharge decisions, these instruments are limited by their positive predictive values.	Weak
Gardner, 2015	Predictive effects for the majority of Personality Assessment Inventory scales were small to moderate in size. …associations between PAI scores and recidivism provide support for the construct validity of…antisocial and aggressive tendencies. The extent to which our findings reflect on the utility of the PAI for predicting recidivism is less clear. The current findings also support the practical utility of PAI administrations, while highlighting the need for studies to report classification accuracy statistics for PAI cut scores. Our results provide the strongest support for the utility of PAI scores in correctional settings, as predictors of institutional misconduct, including violent institutional misconduct.	Small - Moderate
Gendreau, 1992	…there can be no denying that personal temperament, anti-social attitudes, beliefs and behavior, are powerful predictors of recidivism and cannot be ignored by anti-personality adherents. The favored predictor of sociological theory - social class - has been confirmed again as inconsequential. Offender assessments should routinely cover the content areas of companions/criminal associates, behavioral history, personal temperament, anti-social attitudes/beliefs and problems in family of origin.	Strong
Gendreau et al., 1996	In fact, mean r values in this range (e.g., .10 - .30) can be indicative of substantial practical import. Indeed, the percentage improvement in predicting recidivism can equal the value of r, assuming base rates and selection ratios that are not in the extreme. …reasonable confidence can be placed in the results. Additional research, in our view, is not likely to change the direction or ordering of the results of the predictor domains to any marked degree. The time is long past when those offender risk factors that are dynamic in nature can be cavalierly ignored. It would be reasonable, therefore, to assume that programs that insist on alleviating offenders' personal distress, as many do, will have little success in reducing offender recidivism. This meta-analysis extended Tittle and Meier's (1990, 1991) pessimistic conclusions regarding the social class-crime link with delinquent samples to that of adult offenders. It is difficult to judge how social class theories will evolve in the future…the most probable scenario is that social class theories will incorporate more psychological concepts (e.g., Agnew, 1992). …it is absolutely essential that criminogenic needs and antisocial associates are two of the strongest correlates of criminal conduct.	Strong

Gutierrez, 2013	…all of the central eight risk/need factors predicted general recidivism and seven of the eight…predicted violent recidivism for Aboriginal offenders. The present results with Aboriginal offenders only partially replicated the primacy of the big four. For the prediction of violent behaviour, none of the big four stood apart from the other risk/need factors. This raises the question as to whether the big four for non-Aboriginal offenders is also the big four for Aboriginal offenders. …most important implication…is that the central eight risk/need factors are valid predictors of recidivism for Aboriginal offenders. The failure to use risk instruments that tap into the central eight with Aboriginal offenders runs the risk of over-classification. …in the absence of objective risk assessment, one is left to rely on professional judgment and this leads to unnecessary placement of offenders into a higher security. Knowledge of the major criminogenic needs of the offenders can serve as treatment targets, and there is now considerable evidence that programs that address these needs yield lower recidivism. All of this can only benefit Aboriginal offenders.	Moderate
Kennealy, 2010	First, the social deviance scale exhibited stronger predictive utility for violence than the interpersonal-affective scale when controlling for their shared variance. Second, the interpersonal-affective scale did not interact with the social deviance scale to predict violence. Utility of social deviance in predicting violence does not depend on core interpersonal-affective traits of psychopathy. …behavior-based conceptualization emphasizing the disinhibition and chronic criminality of ASPD are most useful for the purpose of risk assessment. Taken together, the results of this study challenge common assumptions about the interactive relationship assumed to exist between the PCL-R factor scores and violence. A refined understanding of psychopathy and related constructs can only improve psychological assessment and legal decision making in applied settings.	NA
Leistico et al., 2008	The overall weighted mean effect sizes were clearly within the range of those reported by prior meta-analyses. The impulsive and antisocial behavioral traits of psychopathy (i.e., F2) had a stronger relation with antisocial conduct than did the affective and interpersonal traits (i.e., F1), which is consistent with previous meta-analyses. Psychopathy explained recidivism/infractions equally well across younger and older samples. Using psychopathy as a clinical measure of the likelihood of institutional misconduct and post-release outcomes is moderately supported by the empirical evidence to date. However, researchers, clinicians, and decision-makers in this area need to take care that information about psychopathy is used appropriately. Given the seriousness of…psycho-legal determinations, we must recommend that clinicians and legal decision makers consider risk and protective factors beyond psychopathy when attempting to predict future behaviors. Our results suggest that predictions of antisocial conduct based on the Hare PCLs should be interpreted more cautiously for members of minority ethnic groups, males, and prisoners than for Caucasians, females, and psychiatric patients. Furthermore, our work suggests that predictions of antisocial conduct will be less reliable for shorter follow-up periods than for longer follow-up periods.	Moderate
Lipsey & Derzon, 1998	…predictor variables most frequently studied in prospective longitudinal studies of antisocial behavior are statistically related to subsequent violent or serious delinquency. The outcome of interest…has a rather low base rate and is consequently more difficult to predict. …the primary practical issue is whether correlation coefficients represent sufficient proportions [of true positives], relative to [false positives], to constitute useful identification of juveniles headed for…delinquency. …it would be desirable for the proportion [of false negatives], relative to [true negatives], to be small… The risk variables most predictive of subsequent serious or violent delinquency are also potential targets for intervention.	Moderate
Mokros, 2014	the PCL-R achieved a cutscore-dependent effect size in the low to medium range, depending on the frame of reference. …the present data complement the consensus that violence risk assessment with the PCL-R works about as well as treadmill-echocardiography for heart conditions but less well than mammography for breast cancer. …low sensitivity, high specificity…. Still, diagnosticians should be aware that even the choice of a cutoff like 25 points on the PCL-R would likely entail a comparatively large group of false- positive. The presence of a sizable proportion of false-positive cases is a matter of concern. If the PCL-R/SV instruments were used and individuals with critical scores barred from release from custody, for example, then a considerable number of individuals, the false-positive ones, would be deprived of their liberty.	Moderate
Olver, 2014	…the family of LS tools and its individual need domains predicted general and violent recidivism among both broad and specific ethnic minority and nonminority groups. One notable difference was the lower predictive accuracy of LS total scores observed with the ethnic minority samples in fixed-effects models. The LS tools predicted general recidivism among female offenders at a broadly comparable magnitude to past research, and importantly, the predictive accuracy of the LS total score was very similar for males and females. …there continued to be a substantial amount of heterogeneity among effect sizes for both gender groups, although this decreased somewhat as additional moderators were examined (e.g., geographic region). …the present findings are representative of a key psychometric property for which this family of tools are most frequently applied—their criterion-related validity for future recidivism. The results also support the consolidation of the LS scales into the Central Eight domains…. They do, however, raise some question about the primacy and universality of the Big Four.	Strong
Olver, 2009	All three measures significantly predicted general, nonviolent, and violent recidivism with comparable degrees of accuracy. …the magnitude of prediction for the three measures was comparable to prediction findings for their adult counterparts. …the ultimate purpose of risk assessment should be the prevention as opposed to the prediction of criminal recidivism. …the most productive inroads in the field of young offender risk assessment might be found in assessing risk and preventing recidivism through treatment, effective case management, and supervision, so as to prevent young offenders from becoming adult offenders…. …findings support the predictive efficacy of three forensic youth measures for general and violent recidivism. Although we would hardly expect the current study to quell the controversy that comes with clinical applications of these tools with this clientele, we submit that a conscientious, ethical, appropriate, and standardized administration of these tools can be part of effective clinical service provision.	Strong

Raynor & Lewis, 2011	Average risk–need scores for minority ethnic offenders are lower than for comparably placed or comparably sentenced white British offenders. Differences are sometimes small but, in most cases, significant and the direction of the differences is strikingly consistent. …the pattern is that minority ethnic offenders with lower criminogenic needs (i.e. lower-risk offenders, who are less likely to continue to offend) have tended to receive the same sentences as higher-risk white majority offenders. The most likely explanation is that the criminal justice process shows a slight but consistent tendency to sentence minority ethnic offenders more severely than equivalent white majority offenders.	NA
Schwalbe, 2008	Results of this study support the use of risk assessment instruments with both male and female offenders. …risk assessment predictive validity did not vary appreciably by gender. …gender-specific risk assessments should not be required for most jurisdictions and programs that implement these decision aids. As statistical prediction devices, actuarial risk assessments do not assume an underlying causal process related to recidivism. Rather, they count risk factors irrespective of the specific factors that may or may not be present for an individual case. It appears that as constructed, we can infer that most risk assessment instruments measure an array of risk factors sufficient to identify risk for girls as well as for boys. …this study supports the use of risk assessment instruments in varied juvenile justice agencies with male and female offenders. Indeed, risk assessment classifications of risk for recidivism may contribute meaningfully to judicial decisions and agency practices related to sanctioning severity and level of care for male and for female offenders. …risk assessment instruments, and the research that supports them, can serve to increase, rather than undermine, gender equity in the juvenile justice system.	Moderate
Schwalbe, 2007	…on average, risk assessment instruments in juvenile justice predict repeat offending as expected…. This finding lends support to the continued use of risk assessment instruments in juvenile justice settings. The YLS/CMI…measures criminogenic needs that, if reduced through intervention, would improve risk scores and presumably prevent repeat offending.	Moderate
Simourd & Andrews, 1994	The risk factors that are important for male delinquency are also important for female delinquency. …the most important are antisocial peers or attitudes, temperament or misconduct problems, educational difficulties, poor parent-child relations, and minor personality variables. In contrast, lower social class, family structure or parental problems, and personal distress are not strongly related to delinquency for either gender. These results support recent social psychological models of criminal conduct that suggest a variety of personal, interpersonal and structural factors are related to delinquent behaviour in males and females. However, our results seriously challenge the value of early delinquency theories. …notions of female delinquency as exclusively symptomatic of personal distress or familial difficulties have been shown to be inadequate. Early male theories, which focused on lower social class as a major route to criminal behaviour, can also be questioned. …the similarity across gender can no longer be ignored. The factors examined to date suggest a unique set of correlates may not be required for female delinquency.	Strong
Singh & Desmarais, 2013	The use of analytic methodologies (ROC curve analysis, correlational analysis, logistic regression, survival analysis) and performance indicators (AUC, r, OR, and HR) measuring a risk assessment instrument's global accuracy were much more common than those that measure the ability of an instrument to accurately identify groups of individuals at higher or lower risk of committing antisocial acts. When the predictive validity of risk bins or final risk judgments were examined, the bins or judgment categories recommended in the instruments' manuals were used in only a third of cases. Lack of reporting consistency in the description and interpretation of performance indicators across studies suggests the need for standardized guidelines for risk assessment predictive validity studies. Because AUC values representing small, moderate, or large magnitude effects varied from one study to the next, caution is warranted when using benchmarks to interpret ROC curve analysis findings. Decisions as to which risk assessment instrument to implement should not be based on this sole criterion, or, at least, on authors' interpretations of the AUC. Indeed, AUC values were misinterpreted in nine-tenths of studies in which an interpretation was offered. In studies where total scores rather than actuarial risk bins or structured risk judgments are used to examine predictive validity, study authors should clarify that the validity of total scores and categorical estimates are not necessarily the same.	Weak
Singh & Fazel, 2010	There was mixed evidence regarding the comparative accuracy of actuarial and clinically based tools. Five of the six meta-analyses that compared actuarial measures with clinically based instruments found that the former produced higher rates of predictive validity than the latter. The sixth meta- analysis found no difference in efficacy between actuarial tools and those that employ structured clinical judgment. Of the 126 risk assessment tools…no one measure was consistently found to be better than any other. There was mixed evidence as to whether risk assessment tools were equally valid in individuals of different genders. Evidence of predictive validity was also inconsistent with regard to ethnicity. There was no clear evidence of risk assessments' validity in psychiatric samples; we found that the meta-analytic evidence on the topic came to different conclusions. There was heterogeneity in the criteria that studies used to define recidivism. Three meta-analyses found that a sample's definition of recidivism moderated effect size, whereas two did not. Given the different criteria used in these reviews, however, it is difficult to compare the findings. The meta-analytic evidence varied on whether length of follow-up moderates effect size. …different risk factors were reported as having the strongest associations with recidivism in the various reviews. Systematic reviews and meta-analyses of the forensic risk assessment literature have a number of potentially important limitations that make their findings provisional.	Weak
Vose et al., 2008	…the majority of studies on the LSI conclude that the instrument is a valid predictor of recidivism. …the instrument has proven to be a valid predictor of recidivism with adults, juveniles, males, and females. The LSI has been validated across a variety of correctional placement settings and with domestic and international offendrs. The notion that the LSI is appropriate for general use (that is, for a variety of offender populations) as opposed to a specific use (only appropriate for use with a selct offender population) will likely add to the already braod appeal of the LSI….	Strong

Walters, 2012	Two meta-analyses were performed in an attempt to answer this question. In the first meta-analysis, the Psychological Inventory of Criminal Thinking Styles (PICTS) General Criminal Thinking, Proactive Criminal Thinking, and Reactive Criminal Thinking scores were correlated with future recidivism in seven prospective non-overlapping samples of participants. The results indicated that all three scores were effective predictors of recidivism, although the General Criminal Thinking score performed slightly better than the Proactive and Reactive scores. In the second meta-analysis, the PICTS General score showed signs of being an incrementally valid predictor of recidivism above and beyond the contributions of two well-known static risk factors, age and criminal history. In conclusion, the present series of meta-analyses indicate that the PICTS General score is moderately effective in predicting recidivism and capable of predicting recidivism after controlling for commonly used static risk factors like age and criminal history.	Moderate
Walters, 2003a	…the PCL-R and LCSF are equally capable of predicting future criminal justice outcomes, using either point-biserial correlations or ROC.	NA
Walters, 2003b	Factor 2 (Antisocial/Unstable Lifestyle) of the PCL/PCL-R is significantly more predictive of recidivism than Factor 1 (Affective/Interpersonal Traits). Factor 1 may capture the essence of psychopathy but it is inferior to Factor 2 in prognosticating recidivism, if not institutional adjustment, in forensic clients and prison inmates.	NA
Watt, 2004	Most consistent support has been provided for the criminal propensity variables of age of onset, criminal history and self- control indices; social control variables of family cohesion and school achievement; and social learning variables of antisocial attitudes and peers. …risk assessment such as the YLSI, is likely to produce the most comprehensive and accurate estimates of recidivism risk and factors contributing to that risk. Such approach to risk assessment is necessary in guiding effective interventions with young adjudicated offenders.	NA
Whittington, 2013	A very large number of studies examining the relationship between a structured instrument and a violent outcome were published in this relatively short 7-year period. The general quality of the literature is weak in places (e.g. over-reliance on cross-sectional designs) and a vast range of distinct instruments have been tested to varying degrees. However, there is evidence of some convergence around a small number of high-performing instruments and identification of the components of a high-quality evaluation approach, including AUC analysis. The upper limits (AUC≥0.85) of instrument-based prediction have probably been achieved and are unlikely to be exceeded using instruments alone.	Small-moderate
Wilson & Gutierrez, 2014	For general offenses, the LSI, in its entirety, significantly discriminated between Aboriginal recidivists and nonrecidivists, ...indirect support for the generalizability of the GPCSL model to Aboriginal offenders. Despite the lower predictive validity of several subscales, the usefulness of the Central Eight with Aboriginal offenders should not be ignored. …the Central Eight risk/need factors…are significant predictors of recidivism with Aboriginal offenders and could, therefore, serve as effective treatment targets. …it could be that Aboriginal offenders scoring low on the LSI assessments do, in fact, more closely resemble medium-scoring offenders. it may be that low-scoring Aboriginal offenders could benefit from greater treatment opportunities than would be afforded to them if they continued to be classified as low risk. The renorming of the LSI without additional information explaining the underclassification would impede these potentially useful treatment opportunities and, therefore, cannot be supported. As such, action should be grounded in further research into what works best with Aboriginal offenders.	Strong
Yu, et al., 2012	There was a threefold increase in the odds of violent outcomes in individuals with all PDs compared with general population controls. Unsurprisingly, the risk in antisocial PD was substantially higher (reported as an odds ratio of 12.8). Second, there were high levels of heterogeneity in overall risk estimates, which was partly explained by higher risk estimates in samples with more female participants. …offenders with PDs had two to three times higher odds of being repeat offenders than mentally or non–mentally disordered offenders. Unlike the situation with nonoffenders, a diagnosis of ASPD or gender did not materially alter risk estimates. The relationship of PD to violence and the quantification of the risk are important from public health and public policy perspectives. …this review implies that, in principle, if the link between PD and offending was modifiable, it could provide one approach to reduce crime. Because the evidence to date suggests that it is at most weakly modifiable, and because the risk estimates in ASPD were found to be similar to those in relation to alcohol and drug abuse, the particular emphasis on addressing severe PD as a means of crime reduction could be questioned. We found higher risks of violence and criminality for individuals with PD than for general population controls, and for offenders with PD compared with other offenders. The utility of risk assessment and management may differ by PD category and gender.	Small - moderate

Chapter 3

Criminogenic or criminalized? Testing a presupposition of the criminogenic risk assessment framework

3.1 Abstract

Proponents of criminogenic risk assessment, which was developed to predict recidivism, claim that it captures the origins of criminal behavior, and can thus be leveraged to reduce correctional supervision rates and prevent crime. These claims rest on the assumption that the best predictors of recidivism are the same as the best predictors of the onset and duration of criminal behavior, and by extension, that interventions for tertiary prevention apply to primary and secondary prevention. This assumption would be threatened, however, if contact with the criminal justice system itself increases criminogenic risk levels. This would imply that a portion of what is observed to predict recidivism is in part the result of causal feedback that does not exist for individuals who have not yet had any "dose" (or a smaller dose) of contact with the criminal justice system. The present study tests this possibility with data from a prospective cohort of 503 boys assessed before their first contact with the criminal justice system, and every 6 to 12 months through early adulthood. Antisocial attitudes, behaviors, and peers were ascertained with validated measures and arrests and convictions were ascertained through official records. Inverse-probability-weighted marginal structural models and fixed-effects multilevel models were employed to triangulate causal inference in the presence of time-varying confounding. Analyses indicated that each arrest, and to a lesser extent conviction, an individual experienced increased their subsequent antisocial attitudes, behaviors, and peer affiliations. Findings raise concerns about the criminogenic risk framework's applicability for crime prevention and etiology.

3.2 Introduction

Current evidence-based corrections practice has adopted a "high-risk strategy" (Rose, 1985), wherein the focus of community corrections is on identifying people at the highest risk of recidivism and targeting supervision and treatment resources at these individuals. This is accomplished through the use of risk assessment instruments such as the Level of Services Inventory (Andrews, Bonta, & Wormith, 2004; Andrews & Bonta, 2010), which were developed to assess individual differences in recidivism. Often, programs designed to intervene upon these inter-individual risk factors reduce recidivism among high-risk individuals. This apparent effect on recidivism has been interpreted as evidence that the high-risk strategy has tapped into the causes of criminal behavior more broadly, and can thus reduce criminal behavior and correctional supervision rates overall. Indeed, an explanatory framework has emerged around proximate criminogenic risk factors as fundamental to the origins of criminal behavior and the roots of crime itself (Andrews & Bonta, 2010). The use of criminogenic risk assessment is expanding from the back-end of the criminal justice system system to the front, in pre-trial processing, sentencing, and even policing (Desmarais & Singh, 2013; Gottfredson & Moriarty, 2006; Lowenkamp & Whetzel, 2009; Storey, Kropp, Hart, Belfrage, & Strand, 2014; Summers & Willis, 2010; Trujillo & Ross, 2008; VanNostrand & Keebler, 2009).

Yet, the widespread acceptance and expansion of risk assessment in criminal justice policy and practice (Desmarais & Singh, 2013; Hannah-Moffat, 2012; Lowenkamp & Whetzel, 2009) may be outpacing the theory and evidence to support it. For example, Andrews and Bonta (2010, p. 299), who are in many ways the forbearers of the current risk assessment framework, 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." But statements like these may presume too much. They rest on the assumption that the best predictors of recidivism are the same as the best predictors of the onset and duration of criminal behavior. Moreover, such statements make causal assumptions about these predictors when they imply that interventions successful at reducing recidivism will also be successful at reducing the onset and duration of criminal behavior.

One way to cut through these conceptual and empirical problems is to draw out into the open, and test, an underlying premise of the above assumptions. The premise is that the criminal justice system itself has no impact on criminogenic risk levels, otherwise what we may be observing in our prediction of recidivism is in part the result of a causal feedback loop: For samples at risk of recidivism, exposure to the criminal justice system is ubiquitous. This ubiquitous exposure is not present for individuals who have not yet had any "dose" (or a smaller dose) of contact with the criminal justice system. Thus, risk factors for recidivism may be influenced by the feedback of the criminal justice system itself, whereas risk factors for onset of criminal behavior are not. In other words, if the criminal justice system does influence criminogenic risks, then the predictors of recidivism are likely not entirely concordant with the predictors of onset and duration of criminal behavior. This possibility presents a fundamental challenge to the criminogenic risk framework, and has profound implications for its expansion. The present study, then, is a critical (albeit indirect) test of the framework's core assumptions: if evidence supports the causal feedback hypothesis, then there is reason to question the framework's expansion throughout the criminal justice system and its explanatory adequacy.

In the remainder of this section, I first introduce the criminogenic risk framework and its empirical basis. I then enumerate additional assumptions of the framework that are implicated in its use and expansion in criminal justice policy and practice. Next, in light of these assumptions, I identify conceptual and methodological limitations in contemporary research on criminogenic risk assessment. This critical perspective then sets up the methods and analytic approach of the present study.

3.2.1 The criminogenic risk framework

The task of risk assessment is accomplished through survey instruments such as the Level of Services Inventory (Andrews & Bonta, 2010), which were developed to assess individual differences in recidivism.[1] A central premise of such instruments is that they focus on manipulable risk factors and are thus relevant not only for the *assessment* of risk but also for its *reduction*. The Level of Services Inventory was developed based on a "radical empirical approach to building theoretical understanding" (Andrews & Bonta, 2010, p. 132). Researchers identified variables that were most strongly correlated with re-arrest among individuals under community corrections supervision, and then used those variables to categorize individuals into various risk groups for targeted interventions to reduce recidivism (Andrews & Bonta, 2010, p. 132-133). Numerous meta-analyses have since found that four proximate risk factors consistently predict recidivism in almost any sample: a history of antisocial behavior, antisocial personality pattern, antisocial cognition, and antisocial associates (Dowden & Andrews, 1999b; Gendreau, Little, & Goggin, 1996; Landenberger & Lipsey, 2005; Lipsey & Derzon, 1998; Simourd & Andrews, 1994) (See the first panel of Appendix Table 3.3 for a more detailed description of these "Big Four" criminogenic risk factors.)

The Big Four criminogenic risk factors, in addition to others in the Level of Services Inventory and similar instruments, have been shown to reliably differentiate risk categories for recidivism (Andrews et al., 2004; Andrews, 1982; Girard & Wormith, 2004). When programs

[1] A recent review (Desmarais & Singh, 2013) identified 19 risk assessment instruments validated in U.S. correctional settings. While there are subtle differences among these instruments, comments about the Level of Services Inventory generally apply, i.e., the Level of Services Inventory is an exemplar of the criminogenic risk framework—it is employed by roughly 900 corrections agencies in North America (Lowenkamp & Whetzel, 2009).

ostensibly intervene upon criminogenic risk factors, they have a small-to-modest protective effect against recidivism, which increases when interventions include cognitive-behavioral therapies (Andrews & Dowden, 2006; Andrews et al., 1990; Andrews, Bonta, & Wormith, 2006; Dowden & Andrews, 1999a, 1999b; Lowenkamp, Latessa, & Smith, 2006; Skeem, Manchak, & Peterson, 2011). Because such interventions seem to work, criminogenic risk factors are implicitly considered causes of recidivism and criminal behavior. An entire theoretical and explanatory framework has emerged based on these findings, as the title of an influential and enduring text by the framework's originators, *The Psychology of Criminal Conduct, 5th edition*, suggests (Andrews & Bonta, 2010). Although the authors admit that their "radical empirical" approach might be confused with "dustbowl empiricism" (Andrews & Bonta, 2010, p. 133) they argue that it nonetheless "lead[s] to a deeper theoretical appreciation of criminal conduct" and is "practically useful in decreasing the human and social costs of crime" (Andrews & Bonta, 2010, p. 133).

Indeed, the criminogenic risk framework has inspired optimism within the field of criminal justice scholarship and practice, which had once resigned itself to the notion that "nothing worked" (Martinson, 1974). Now, it appears that certain high-risk people who come into contact with the justice system can benefit from interventions focused on the Big Four and other criminogenic risk factors (Cullen & Gendreau, 2001; Cullen, 2011; Gendreau, French, & Gionet, 2004; Lowenkamp et al., 2006). Proponents see risk assessment as an empirically driven implementation of a rehabilitative philosophy that reserves the most intensive supervision and treatment resources for those with the highest risk profiles, and essentially leaves alone those for whom supervision might hold no benefit or even worsen criminal justice outcomes (Lowenkamp et al., 2006). Advocates rightly point out that the historical alternative—more severe criminal sanctions—actually *increases* recidivism: "this finding is so well established that specific deterrence[2] may be declared to be empirically indefensible as a rationale for increases in the severity of the penalty" (Andrews & Bonta, 2010, p. 369).

As the discourse and practice of criminogenic risk assessment expands from its original purpose—efficient and humane recidivism reduction for individuals under community corrections supervision—to other institutions and procedures within the criminal justice system, and even as a way of understanding crime, it is worth pausing for critical reflection. Upon what methods and measures are criminogenic risk assessments actually based, and what are the implications of expanding the framework beyond recidivism reduction, to questions of onset and duration of criminal behavior? Is it appropriate that "jurisdictions often rely on implementing pre-existing screening tools derived for similar purposes but on different samples" (Lowenkamp, Lemke, & Latessa, 2008, pp. 3-4), e.g., adapting tools developed for predicting recidivism risk for pretrial screening? Is the assertion that instruments such as the Level of Services Inventory contain "dynamic items that have been empirically proven to be the best predictors of crime" (Vose, Cullen, & Smith, 2008, p. 23) justified when, as described below, there is a subtle yet crucial distinction between the concepts of *predictors of crime* and *predictors of individual variation in crime*? Does empirical evidence about individual variation in criminal behavior apply to the constructs of community safety and crime prevention?

3.2.2 Conceptual and methodological issues

The criminogenic risk framework was developed with a focus on the very end of a causal process—the point at which individuals have already moved through the criminal justice system

[2] Specific deterrence refers to forms of punishment that attempt to discourage the individual in question from committing future crimes, whereas general or indirect deterrence attempts to prevent crime by making examples out of particular offenders (or classes thereof).

and are at risk for recidivism. The expansion of the criminogenic risk framework beyond a focus on recidivism presumes that these proximate causal factors for recidivism also apply to earlier (or more distal) parts of the causal process. Yet, lessons learned from debates about the success of "risk factor epidemiology" (Schwartz, Susser, & Susser, 1999; Susser, 1998); show that such a perspective limits engagement with a host of potentially important phenomena (McMichael, 1999; Rose, 1985). These include changes in risk profiles over the life course, variation over time in constructs typically measured at a single time point, conflating the causes of an individual's place within a distribution with causes of the distribution, and complex multi-directional causal relationships (e.g., feedback loops and reverse causation). Regarding the latter, if contact with the criminal justice system itself has an effect on criminogenic risk, this alone would destabilize the assumptions identified above, because such an effect is only possible for recidivism: the exposure (contact with the criminal justice system) does not exist prior to first contact and exists in varying degrees for different people over the duration of their engagement with criminal behavior. It is thus worth unpacking the constructs that inform risk assessment items such as the "Big Four" (antisocial behavior, personality pattern, attitudes, and peers) in order to explore whether they are consistent with the etiologic claims of the criminogenic risk framework.

According to the criminogenic risk framework, the causes of crime are to be found within individuals and their social learning environments (Bonta, Blais, & Wilson, 2014, p. 279), but the framework attributes most of the causal action to psychology, claiming that 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, Stockdale, & Wormith, 2014, p. 157). It is true that 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; Loeber et al., 2012). In addition to these personality characteristics, there is considerable overlap between the Big Four criminogenic risk factors and DSM-5 Section II diagnostic criteria for antisocial personality disorder and conduct disorder, which have a lifetime prevalence of roughly 2% - 5% and 1.1%, respectively, in the adult general population in the United States (Black & Blum, 2015; Compton, Conway, Stinson, Colliver, & Grant, 2005; Goldstein et al., 2007) (See Appendix Table 3.3).

Yet, the psychiatric 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, antisocial criminogenic risk constructs are circular—they contain the outcome for which they purport to be risk factors. This circularity may also serve to conflate risk factors for *criminal behavior* with risk factors for *exposure to the criminal justice system*, which makes it difficult to interpret the overrepresentation of antisocial characteristics among incarcerated populations. Early estimates placed the prevalence of antisocial personality disorder in jails and prisons at 80%, whereas more recent estimates suggest the figure is roughly 35%. The apparent decline in prevalence is attributed to dramatic increases in incarceration rates overall (Black & Blum, 2015; Black, Gunter, Loveless, Allen, & Sieleni, 2010), underscoring the distinction between criminal behavior and exposure to the criminal justice system. Thus, despite claims by proponents of the criminogenic risk framework that it is a myth that the roots of crime are buried deep in structural inequality (e.g., Andrews & Bonta, 2010, p. 70), the predictive utility of criminogenic risk factors is not inconsistent with the notion that criminal behavior and exposure to the criminal justice system are socio-structural phenomenon.

These conceptual problems raise methodological questions about etiological claims drawn from criminogenic risk assessment research. First, these risk factors have been identified by research on recidivism, and thus by definition, research samples are already in contact with the criminal justice system. It is thus not possible to determine whether upstream social factors confound the relationship between antisocial characteristics and contact with the criminal justice system, or whether contact with the criminal justice system itself has an effect on criminogenic risk factors. Second, current evidence for the predictive and intervention utility of criminogenic risk factors comes from studies in which the outcome (recidivism) follows the exposure (criminogenic risk) in a relatively proximate time window.

In epidemiologic terms, the fact that criminogenic risks have been identified, and their predictive utility tested, in samples that are already in contact with the criminal justice system renders the causal contrast unavailable for questions regarding criminogenic risk factors as causes of criminal behavior more broadly. The fact that criminogenic risk factors are typically measured at a single time point during or after incarceration renders the causal contrast unavailable for questions regarding these risk factors (or trajectories of risk profiles) as causes of initial or ongoing contact with the justice system. If one were interested in, for example, the effect of antisocial attitudes on the onset or duration of criminal behavior, “exposed” and “unexposed” groups should be free of criminal behavior at baseline. If one were interested in the effects of antisocial behavior on first contact with the criminal justice system, exposed and unexposed groups should have had the possibility of not being involved in the criminal justice system. Because virtually all research on criminogenic risk factors has been conducted with samples that have already engaged in criminal behavior or are already criminal-justice-involved (including rare inquiries into changes in criminogenic risk scores over time, e.g., Vose, Smith, & Cullen, 2013), these causal contrasts are impossible. It also means that any other factors over the life course that distinguish individuals who become involved in the justice system from those who do not are undetectable as between-person risk factors.

There is evidence that lends credence to these concerns. For example, evidence is mixed that criminogenic risk factors more broadly can predict distinct offending trajectories over the life course. This is related to an ongoing debate about trajectories of risk profiles for criminal behavior, i.e., predictors of onset, duration, and persistence/desistance of criminal careers (Loeber, Farrington, Stouthamer-Loeber, Raskin White, & Wei, 2008). While four to six developmental trajectories have been identified, an unresolved question is the extent to which they have common or distinct etiologies (and thus predictive utility) (Loeber et al., 2008; Moffitt, 1993; Sampson & Laub, 2003). One important study found that, although 20 *a priori* individual and family risk factors modestly predicted levels of crime between individuals, they did not prospectively yield distinct groupings of offending trajectories (Sampson & Laub, 2003). In other words, there was no effect of criminogenic risk factors on offending trajectories.

Regarding the potential for feedback loops, evidence is limited, although 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 sending someone to prison works only to criminalize them further (Cullen & Agnew, 2010; Plummer, 2001). One example is the effect of contact with the criminal justice system on antisocial behavior, which is thought to be a strong independent predictor of arrest and recidivism. In a recent review, 12 of 18 analyses found that arrest was associated with an *increase* in self-reported delinquency, re-arrest, or adult criminal justice outcomes (Huizinga, Henry, & Liberman, 2008). Five of the remaining six studies found that arrest had no deterrent effect on subsequent delinquent behavior (Huizinga et al., 2008). The same pattern emerged for studies on the effect of post-arrest sanctions on subsequent delinquency. However, research on this issue has been limited to the effects of criminal justice system exposure on behavior; questions about the effect on attitudes and peers have remained largely unexplored.

The conceptual and methodological issues introduced above emphasize the gravity and immensity of the assumptions necessary for the criminogenic risk framework's expansion. The significance of the current study thus resides in its capacity to destabilize these assumptions if it can demonstrate feedback loops or reverse causation. Testing this hypothesis requires that a number of conditions be met. The first is that data need to be longitudinal. The second is that data collection commence before the typical onset period for contact with the criminal justice system. Third, the data require robust measures of antisocial constructs and contact with the criminal justice system, in addition to potential confounders of these phenomena. The present study thus utilizes data that are uniquely able to meet these conditions.

3.3 Methods

This study draws on a prospective cohort of boys that began before their first contact with the criminal justice system with follow-up through adolescence and into adulthood. Demographic, family, school, clinical, and psychosocial measures, in addition to official criminal justice records, were obtained every six to 12 months. The present study implements a two-part analysis plan to determine whether exposure to the criminal justice system has an effect on antisocial attitudes, behaviors, and peers. Isolation of these causal effects has typically been beleaguered with the problem of time-varying exposure and time-varying confounding, as illustrated in Figure 3.1. But recent methodological advances have begun to clarify these issues in observational data. The first approach, marginal structural modeling, estimates the cumulative effect of contact with the criminal justice system on antisocial attitudes and behaviors, given the possibility of time-varying confounding of the exposure, outcome, and other measured factors. The second approach, fixed effects modeling, identifies the effect of change in contact with the criminal justice system over time on change in antisocial attitudes, behaviors, and peers, controlling for all measured and unmeasured stable factors in addition to measured time-varying factors. As a triangulation of methods, these approaches are an attempt to enhance causal inference about the effect of contact with the criminal justice system on antisocial attitudes, behaviors, and peers in a complex causal model in which feedback loops are likely.

3.3.1 Sample

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. The project was undertaken to study the development of juvenile offending, mental health problems, drug use, and their risk factors in inner-city boys (Loeber et al., 2012, 2008; Pardini, Loeber, Farrington, Stouthamer-Loeber, & Stouthamer–Loeber, 2012).

The study's design and sample have been described extensively elsewhere, and the summary that follows draws heavily from those descriptions (Loeber et al., 2012, 2008; Pardini et al., 2012). Boys attending the first grade in virtually all public schools in downtown Pittsburgh (*N*=31) 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, reflecting the racial composition of Pittsburgh public schools at the time. The average age at screening was 7.

3.3.2 Design

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). The present study makes use of all 19 assessment periods. Primary caretakers and teachers completed self-administered questionnaires. Most interviews took place in participants' homes. Prior to the assessment, caretakers and teachers provided written informed consent, and adolescents provided assent until age 17, and consent thereafter. The data collection procedures were approved by the institutional review board at the University of Pittsburgh. As the present study is a secondary analysis of de-identified data, the Columbia University institutional review board determined that additional review was not warranted.

3.3.3 Measures

Figures 3.2-3.4 summarize the longitudinal characteristics of the sample, using the measures described below. Appendix Table 3.4 presents the measures' means, standard deviations, minimums, and maximums.

3.3.3.1 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 constructs used in risk assessment instruments such as the Level of Services Inventory.

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 = 0.73 – 0.83, internal consistency = 0.91) (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 = 0.9) (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 = 0.77 – .8, internal consistency = 0.91) (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, & Klein, 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, & van Kammen, 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 $\alpha=0.92$. (Pardini et al., 2012)

3.3.3.2 Exposure: Criminal Justice System Contact

The present study uses constructed variables that measure the count, per assessment interval, of adolescents’ total arrests and convictions. These data were gathered from official records from the Allegheny County Juvenile Court, Pennsylvania Juvenile Court Judges’ Commission, Pennsylvania State Police Repository, and the Federal Bureau of Investigation. Data on arrests and convictions are not linked.

3.3.3.3 Potential Confounders

The present study controls for potential confounders, in addition to prior values of antisocial attitudes, behaviors, and peers, of the relationship between criminal justice contact and antisocial attitudes and behaviors. Covariates of interest include psychopathology, substance use, institutionalization, academic achievement, parenting factors, parental criminal history, neighborhood factors, and sociodemographic factors.

Internalizing and externalizing t-scores. Internalizing and externalizing problems were measured with the Childhood Behavioral Checklist (CBCL) (Achenbach, 1991a, 1991b; 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 externalizing scale represents the sum of 27 items that loaded onto “delinquent behavior” and “aggressive behavior” clinical syndrome scales. One-week test-retest stability coefficients are .89 for internalizing problems and .93 for externalizing problems (Achenbach, 1991b; Youngstrom et al., 2000).

Alcohol and marijuana 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 or never used alcohol or marijuana in the period prior to assessment.

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 though 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. Academic achievement 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 $\alpha = 0.81$) (Pardini et al., 2012).

Parenting factors. 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) Parental supervision was measured by the Supervision/Involvement Scale, a 43-question scale, was administered to both parents and youth, and assessed parents’ supervision style, with values ranging from closely supervised to poorly supervised (Loeber, Stouthamer-Loeber, Morris, & Tonry, 1986).

Parental convictions. Lifetime data on mothers' and fathers' history of arrest and conviction were collected via caretaker self-report (Loeber et al., 2008). Mothers' and fathers' convictions were summed to create a "parental conviction" score.

Neighborhood characteristics. Neighborhood characteristics were assessed by the Neighborhood Scale (Loeber et al., 1998) and measured the caretakers' perceived quality of the neighborhood in which their families resided. This instrument contained 17 items covering the presence of prostitution, assaults, burglaries, and similar problems in the neighborhood.

Adolescent demographics. 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). Participant race/ethnicity was ascertained from adolescents' caretakers at screening.

3.3.4 Analysis

All analyses were conducted in R version 3.2.2.

3.3.4.1 Missing data

Missing data in the independent variables were imputed using R package 'mice' (van Buuren & Groothuis-Oudshoorn, 2011) for "multivariate imputation by chained equations," an implementation of fully conditional specified models for imputation. The fully conditional approach differs from the more traditional joint modeling approach by specifying a multivariate imputation model on a variable-by-variable basis (van Buuren & Groothuis-Oudshoorn, 2011). This fully conditional approach is used as an alternative to traditional joint modeling when no suitable multivariate distribution can be found. The present study implemented MICE with the random forest method for imputation, an extension of classification and regression trees, which recursively subdivides the data based on values of predictor variables, and uses bootstrap aggregation of multiple regression trees to reduce overfitting (Shah, Bartlett, Carpenter, Nicholas,

& Hemingway, 2014). Random forest MICE does not rely on distributional assumptions and can accommodate nonlinear relations and interactions (Shah et al., 2014). After imputation, for phases in which particular measures were not assessed, the last observation was carried forward.

3.3.4.2 Inverse-probability-weighted marginal structural models

For the first component of the analysis, I fit marginal structural models (Robins, Hernán, & Brumback, 2000) of antisocial attitudes, behaviors, and peers, estimated with inverse probability weights of arrests and convictions, respectively. Inverse probability weighting creates a pseudo-population by weighting each individual by the inverse probability of his or her 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 confounders (Cole & Hernán, 2008). When the assumptions of consistency, exchangeability, positivity, and no model misspecification are met, the exposure parameter of a marginal (unconditional) structural model with inverse probability weighting estimates the average causal effect of the exposure in the original cohort (Cole & Hernán, 2008; Hernán, Brumback, & Robins, 2000; Robins et al., 2000).

In practice, to ensure positivity and correct model specification, weights are 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). As a result of stabilizing weights, the exposure is randomized within levels of the covariates, and so these covariates must be included in the final (conditional) structural model. For a detailed example and 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 confounders described above. That is, I used both the raw value of the confounders 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 with R's native "predict" function, and input this vector of values into R's native function for the negative binomial probability mass function (Hernán & Robins, 2016). 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 stabilized weights. 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.

I repeated these steps to create stabilized weights for convictions, modeling it with the same predictor variables as arrests, but substituting lagged and lagged cumulative conviction for the primary exposure. In all models, regardless of whether the dependent variable was the exposure (for the probability of exposure) or outcome (for the final marginal structural models) I used un-imputed original variables with missingness for the dependent variable.

Examination of the stabilized weights for arrests and convictions suggested positivity violations, as the probability of these events for certain outlier participants was nearly zero. This made the inverse probability for those participants 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' weight was changed to this maximum (Cole & Hernán, 2008).

For the final marginal structural models, I planned to estimate the effects of arrests and convictions on the original, un-imputed versions of the antisocial attitudes, behaviors, and peers. I therefore constructed inverse probability of censoring weights based on these variables. I did this by creating censored/uncensored dummy variables for the three antisocial outcome variables in each assessment interval. Next, I fit logistic regression models with these dummy variables as the outcome and contemporaneous measures of the time-varying and invariant predictors described above. The vector of model-predicted probabilities of remaining uncensored was obtained from R's native "predict" function, and this became the denominator of the censoring weight (Hernán & Robins, 2016). Construction of the numerator followed the same approach, but the numerator models for censoring included only total arrests or convictions and race/ethnicity as predictors. I multiplied the truncated, stabilized exposure weights by the censoring weights to obtain the final weights for use in the marginal structural models (Cole & Hernán, 2008; Hernán & Robins, 2016).

Using the aforementioned weights, I fit 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.

3.3.4.3 Change on change models

While the inverse-probability-weighted marginal structural models control for all measured time-varying confounding—i.e., the confounding that can arise when variables act as confounders and mediators at different time points—unmeasured confounding is still a threat to valid causal inference. As a complementary, second part of my analytic approach, fixed effects models (or change-on-change or difference-on-difference models) can help shore up causal inference for one set of potentially unmeasured confounders; fixed effects models control for all stable characteristics of study participants, whether or not they are measured. This is achieved by ignoring all between-person variation and focusing only on within-person variation (Allison, 2005, 2009; Curran & Bauer, 2011). Although discarding between-person variation can result in higher standard errors, it is this variation that is likely contaminated by unmeasured personal characteristics that confound the relationship between exposure and outcome (Allison, 2005, 2009; Curran & Bauer, 2011). By examining change on change, causal inference is enhanced because factors with stable effects that vary between individuals are ruled-out as potential confounders. 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. And while the fixed effects approach controls for all unmeasured time-invariant confounders, measured time-varying confounders can also be included as control variables.

A series of models were fit separately to test the effects of one-year-lagged arrests and convictions on total antisocial attitudes, behaviors, and peers. In addition, following Allison's “hybrid method” for fixed effects (Allison, 2009), all independent variables were centered by person-means, and the mean of each variable was also entered into the model. I thus regressed total antisocial attitudes on lagged arrests, controlling for mean arrests, and included a random intercept for individuals. I then added two-year-lagged and centered potential confounding variables, described above, two-year-lagged and centered antisocial attitudes, and all covariate means. I repeated this procedure for the effects of arrest on total antisocial behaviors and peers, and for the effects of convictions on antisocial attitudes, behaviors, and peers. Models were fit using R package ‘lme4’ (Bates, Mächler, Bolker, & Walker, 2015). Two-year lags were used for potential confounders so that they would be modeled prior to the measurement of the exposure. 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 contemporaneous to changes in arrests and convictions.

3.4 Results

3.4.1 Inverse-probability-weighted marginal structural models

Table 3.1 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. The average causal effect of arrest on antisocial attitudes was 2.25 units, (95% confidence interval [CI]: 0.87 – 3.63). This is equivalent to a 0.13 (95% CI: 0.05 – 0.2) standard deviation increase in antisocial attitudes. These results can be interpreted as the cumulative effect of arrest on antisocial attitudes, controlling for all measured time-varying and stable confounders. Results were more pronounced for antisocial behaviors: the average causal effect on antisocial behaviors was 5.33 units (95% CI: 1.01 – 9.64), or 0.21 (95% CI: 0.04 – 0.39) standard deviations. These results can be interpreted as the cumulative effect of arrest on antisocial behaviors, controlling for all measured time-varying and stable confounding. The cumulative effect of arrests on antisocial peers was 1.03 (95% CI: 0.33 – 1.73), or a 0.21 (95% CI: 0.07 – 0.34) standard deviation increase in antisocial peers, controlling for all measured time-varying and stable confounding.

Convictions had a more modest effect on antisocial attitudes, behaviors, and peers. The average causal effect of conviction history on antisocial attitudes was 1.01 units (95% CI: 0.48 – 1.54). This is equivalent to a 0.06 (95% CI: 0.03, 0.09) standard deviation increase in antisocial attitudes. These results can be interpreted as the cumulative effect of convictions on antisocial attitudes, controlling for all measured time-varying and stable confounding. The average causal effect of conviction history on antisocial behaviors was 2.41 units (95% CI: 0.02 – 4.79. This is equivalent to a 0.1 (95% CI: 0.001 – 0.19) standard deviation increase in antisocial behaviors. These results can be interpreted as the cumulative effect of convictions on antisocial behaviors, controlling for all measured time-varying and stable confounding. The cumulative effect of convictions on antisocial peers was 0.38 (95% CI: 0.12 – 0.64), or a 0.08 (95% CI: 0.02 – 0.13) standard deviation increase in antisocial peers, controlling for all measured time-varying and stable confounding.

3.4.2 Change on change models

Table 3.2 summarizes the results of fixed effects models for the relationship between change in criminal justice system contact and change in antisocial attitudes, behaviors, and peers. In the crude model for the effect of arrests on antisocial attitudes, if a person's number of arrests changed by one person-mean (i.e., a mean deviation), their antisocial attitudes increased by 1.84 units (95% CI 1.13 – 2.55). After adjusting for two-year-lagged confounders, a mean deviation in arrest resulted in a 1.74 increase (95% CI: 1.05 – 2.43) in antisocial attitudes. Results for antisocial behaviors were more pronounced. A one-year-lagged mean deviation in arrests resulted in a 3.25-unit increase (95% CI: 2.15 – 4.36) in antisocial behaviors. After adjusting for two-year-lagged confounders, this effect reduced to 2.9 (95% CI: 1.77 – 4.03). Finally, a one-year-lagged mean deviation in arrests increased antisocial peers by 1.00 (95% CI: 0.76 – 1.23). After adjusting for confounding, this effect dropped to 0.84 (95% CI: 0.61 – 1.07)

Convictions again had a more modest effect on antisocial attitudes, behaviors, and peers. In the crude model for the effects of convictions on antisocial attitudes, a one-year-lagged mean deviation in convictions resulted in a 0.57 increase (95% CI: 0.23 – 0.9) in antisocial attitudes. After adjusting for two-year-lagged confounders, the effect did not appreciably change ($\beta = 0.55$, 95% CI: 0.22 – 0.87) There was no effect of convictions on antisocial behaviors. Convictions had a small effect on antisocial peers. A one-year-lagged mean deviation in convictions resulted in a 0.26 (95% CI: 0.15 – 0.37) increase in antisocial peers, and this effect did not appreciably change after adjusting for two-year-lagged confounders.

3.5 Discussion

In a community-based sample of 503 boys followed from childhood into early adulthood, contact with the criminal justice system increased their antisocial attitudes, behaviors, and affiliation with antisocial peers. Each arrest, and to a lesser extent conviction, an individual experienced increased their subsequent antisocial characteristics. The weaker effects of convictions versus arrests may be due to a number of factors. First, arrest is a more tangible and frequent experience than conviction: many arrest events did not result in conviction. Second, because a single arrest event can result in multiple charges, and subsequently multiple convictions, the latter may lack precision as a lived experience.

Data were analyzed with techniques to reduce confounding from self-selection into criminal behavior and other potentially criminogenic individual, school, family, and neighborhood factors. Two methods, each with different strengths for confounder control, were employed to enhance causal inference. Inverse-probability-weighted marginal structural models controlled for measured sources of time-varying non-exchangeability, which arises when factors can act as exposures, mediators, and confounders at different time points. This approach isolated the average causal effect of exposure to arrest and conviction on antisocial attitudes, behaviors, and peers, even when prior antisocial characteristics and exposure to the criminal justice system influence subsequent antisocial characteristics. Fixed effects change-on-change models controlled for all time-invariant factors, whether or not they were measured, ruling out non-exchangeability due to stable characteristics. This approach isolated the average causal effect of change in past-year arrest and conviction on subsequent change in antisocial attitudes, behaviors, and peers, adjusting for baseline and past-year changes in measured time-varying factors, including prior changes in antisocial characteristics.

These findings raise fundamental questions about the empirical basis for expanding criminogenic risk assessment from the back end of the criminal justice system to the front, and whether doing so can reduce criminal behavior and correctional supervision rates overall. The results of this study also raise concerns about the theoretical adequacy of the criminogenic risk framework in explaining the roots of crime and the causes of criminal behavior more broadly. These theoretical issues strike at the framework's core conceptualizations of risk, crime, criminal behavior, and recidivism.

Regarding the expansion of criminogenic risk assessment from the back end of the system to the front, this study shows that it may be inappropriate to apply evidence for the predictive and intervention utility of criminogenic risk factors for recidivism to broader questions about the onset, duration, and related social processes surrounding criminal activity. This is because exposure to the criminal justice system causes some portion of the risk used to predict involvement in the criminal justice system. These findings indirectly suggest that criminogenic risks identified in samples under correctional supervision may be different than more distal risks that occur prior to first exposure to criminal justice involvement. As such, policymakers and practitioners should be apprehensive about transporting pre-existing risk assessment instruments to different locations in criminal justice processes.

The issue of feedback loops uncovered in this study also raises important questions about the efficiency and effectiveness of the criminogenic risk framework. If exposure to the criminal justice system increases risk factors used to predict recidivism or future criminal activity, such risk factors, while strongly predictive, may not appreciably influence overall levels of criminal justice system involvement, because intervening on these risk factors will likely not reduce the flow of people into criminogenic risk. In other words, intervening only on prevalence will not reduce incidence. Even if programs that ostensibly reduce criminogenic risks are in fact intervening upon those mechanisms, expanded policy and practice based on criminogenic risk assessment will likely not change the status quo of crime or incarceration rates unless the wider causal context is integrated into such programming. True 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. In other words, true prevention would focus on shifting the risk distribution's mean, not merely truncating its right tail (McMichael, 1999; Rose, 1985).

Regarding conceptualizations of risk, crime, criminal behavior, and recidivism, the criminogenic risk framework's general personality and cognitive social learning theory can begin to explain the present study's findings, but ultimately falls short. Nonetheless, the social learning approach is consistent with the findings presented here insofar as individuals' experiences with law enforcement, court, and corrections procedures or personnel reinforce their ambivalence about prosocial norms or negative feelings about the criminal justice system, increase exposure to other crime-involved peers, or 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, pp. 133-138). 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 present study's findings merely in terms of individuals' psychological predispositions, because prioritizing the immediate situation preceding criminal behaviors—the end of a causal process—masks the antecedents of that process and any feedback loops therein. 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 merely considering them fixed, non-manipulable background characteristics, may begin to remediate conceptual slippage that arises from the individualistic, "treated sample" approach of much psychological criminology. Indeed, concerns about the potentially circular epidemiologic characteristics of antisocial constructs and criminal behavior were expressed during the very creation of antisocial personality disorder as a diagnosis in the 1970s, in debates between Richard Jenkins, an author of the DSM-II, and Lee Robins, a member of the DSM-III's Personality Disorder Advisory Committee. In a captivating analysis of memos, letters, and meeting minutes produced by the committee and held in the American Psychiatric Association archives, Pickersgill (2012) cites Jenkins' concern that "psychiatrists will simply not put down as normal any individual who has repeated problems with the criminal courts" and that the diagnosis should be defined "so narrowly that an individual will not be classified as an antisocial personality simply because he comes from a disadvantaged group." Robins, on the other hand, argued that there was no need to be concerned that psychiatrists would pathologize individuals' problems with the criminal justice system, and the broader definition was adopted (Pickersgill, 2012).

The exchange between Jenkins and Robins foreshadowed conceptual problems that emerge from generalizing individual-level, immediate-situation risk factors to a theory of crime more broadly. 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 reduced 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 between 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.

This omission harkens back to an influential paper in epidemiology by Geoffrey Rose, (1985) who showed that only under certain circumstances are the causes of an individual's place within a distribution (of a health outcome) the same as the causes of the distribution's mean. He argued that in order to understand shifts in distributions of risk, one should study characteristics of populations, not only characteristics of individuals. In fact, "treated samples" render such antecedents undetectable by statistical methods that depend on variation, as any ubiquitous exposures within a group of people already involved with the criminal justice system will not distinguish among individuals within the group, only between the entire group and some other sample that is not involved in the criminal justice system.

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. Recidivism can be the result of a new offense or a technical violation of the terms of community supervision, e.g., missing an appointment with a probation officer. Indeed, the definition of recidivism is inconsistent in validation research on risk assessments. In a review of 19 risk assessment instruments validated in U.S. correctional settings, 31% of validation studies defined recidivism as a new arrest, 13% as reconviction, 10% as reincarceration, and 4% as technical violations (Desmarais & Singh, 2013). This is important because particular risk assessment instruments do not perform as well for certain definitions of recidivism as they do for other definitions (Desmarais & Singh, 2013). For example, 100% of 17 validation studies of the Level of Services Inventory that defined recidivism as reincarceration deemed the instrument to be a valid predictor of recidivism, whereas only 54% of 14 validation studies of the Level of Services Inventory that defined recidivism as rearrest deemed the instrument to be a valid predictor of recidivism (Vose et al., 2008). The explanatory power of the criminogenic risk framework 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 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 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, p.76) 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, p.77), drawing on Feeley and Simon's (1992) seminal analysis, suggests that

> "this strategic orientation toward prison among young men might be understood as the flip side of a juvenile 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.

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 present study's findings should be understood in light of the following limitations. First, all participants in the Pittsburgh Youth Study are male; hence, I could not examine the relationship between exposure to the criminal justice system and antisocial characteristics among girls. Nonetheless, contact with the criminal justice system is a predominantly male phenomenon, as is antisociality (Black & Blum, 2015; Carson & Golinelli, 2013; Durose, Cooper, & Snyder, 2014; Glaze & Parks, 2012; Guerino, Harrison, & Sabol, 2011; Langan & Levin, 2002;

Maruschak & Parks, 2012; Minton, 2011). Second, all participants were selected from Pittsburgh public schools, which potentially limits the generalizability of the findings 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 I examined measures of the Big Four criminogenic risk factors that are consistent with the constructs that underlie the Level of Services Inventory, I could not test this instrument directly. However, as Skeem and Cook (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. None of these limitations, however, undermine the fairness of the test of the criminogenic risk framework's assumptions.

3.6 Conclusion

This study shows that arrests and convictions result in subsequently higher levels of antisocial attitudes, behaviors, and peers among boys followed into young adulthood. Future research should engage with the social conditions that put people at risk of criminogenic risks, consider the criminalizing effect of contact with the criminal justice system, and enumerate potential mechanisms that explain this effect. These findings reveal potential weaknesses in the criminogenic risk framework's approach to crime prevention and etiology. Results caution against the wholesale expansion of criminogenic risk assessment from the back end of the criminal justice system to the front—from community corrections to policing, pretrial decision-making, and sentencing—as the causes of recidivism may not the same as the causes of onset and duration of exposure to the criminal justice system.

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3.8 Figures and tables

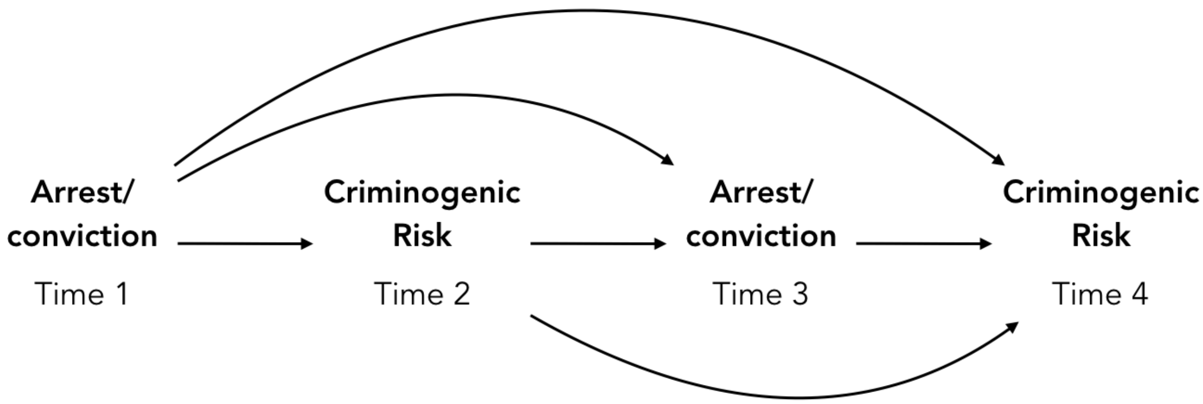

Figure 3.1. Directed acyclic graph illustrating the problem of time-varying exposure and time-varying confounding.

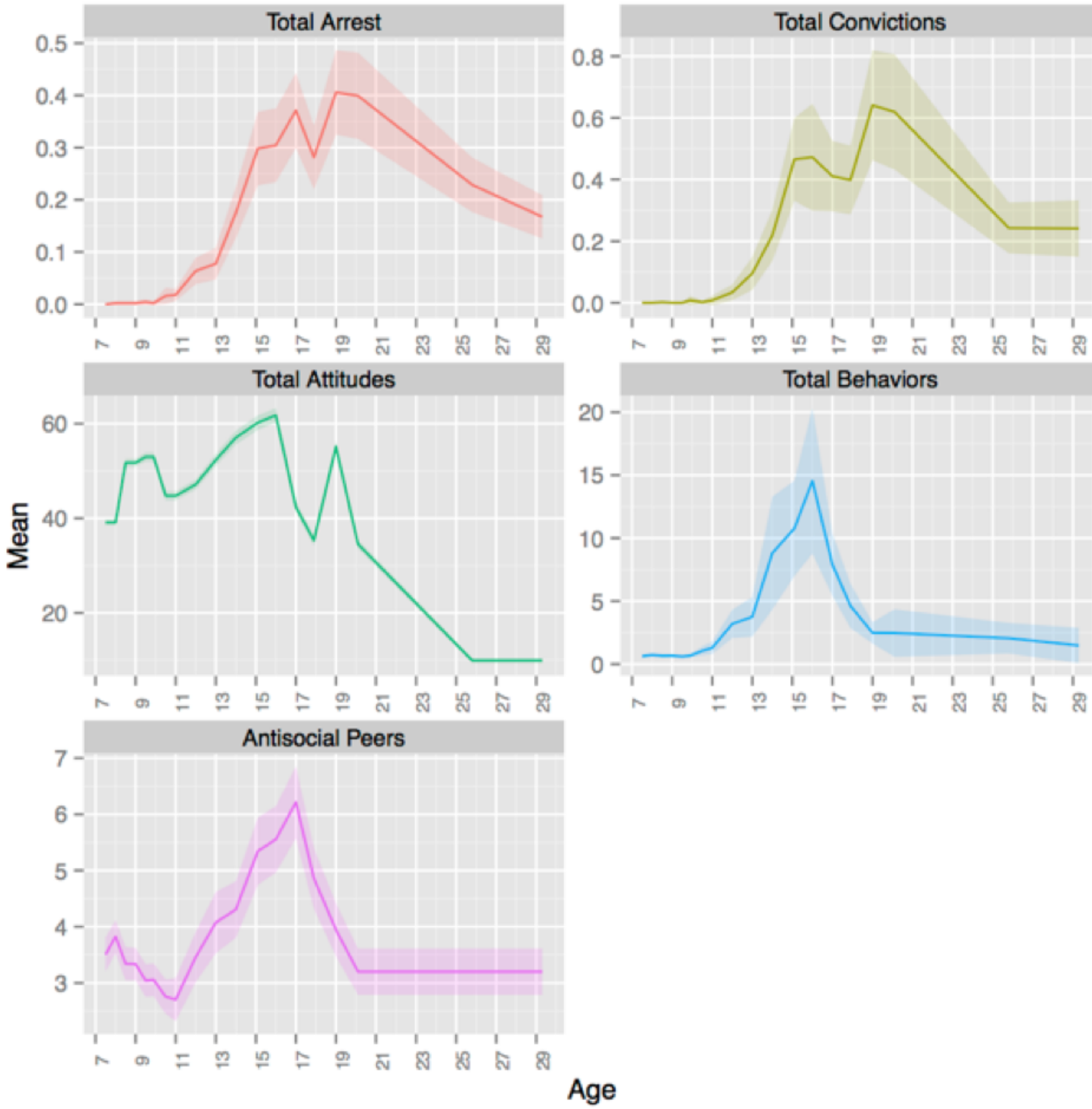

Figure 3.2. Means and 95% confidence intervals for exposure and outcome measures

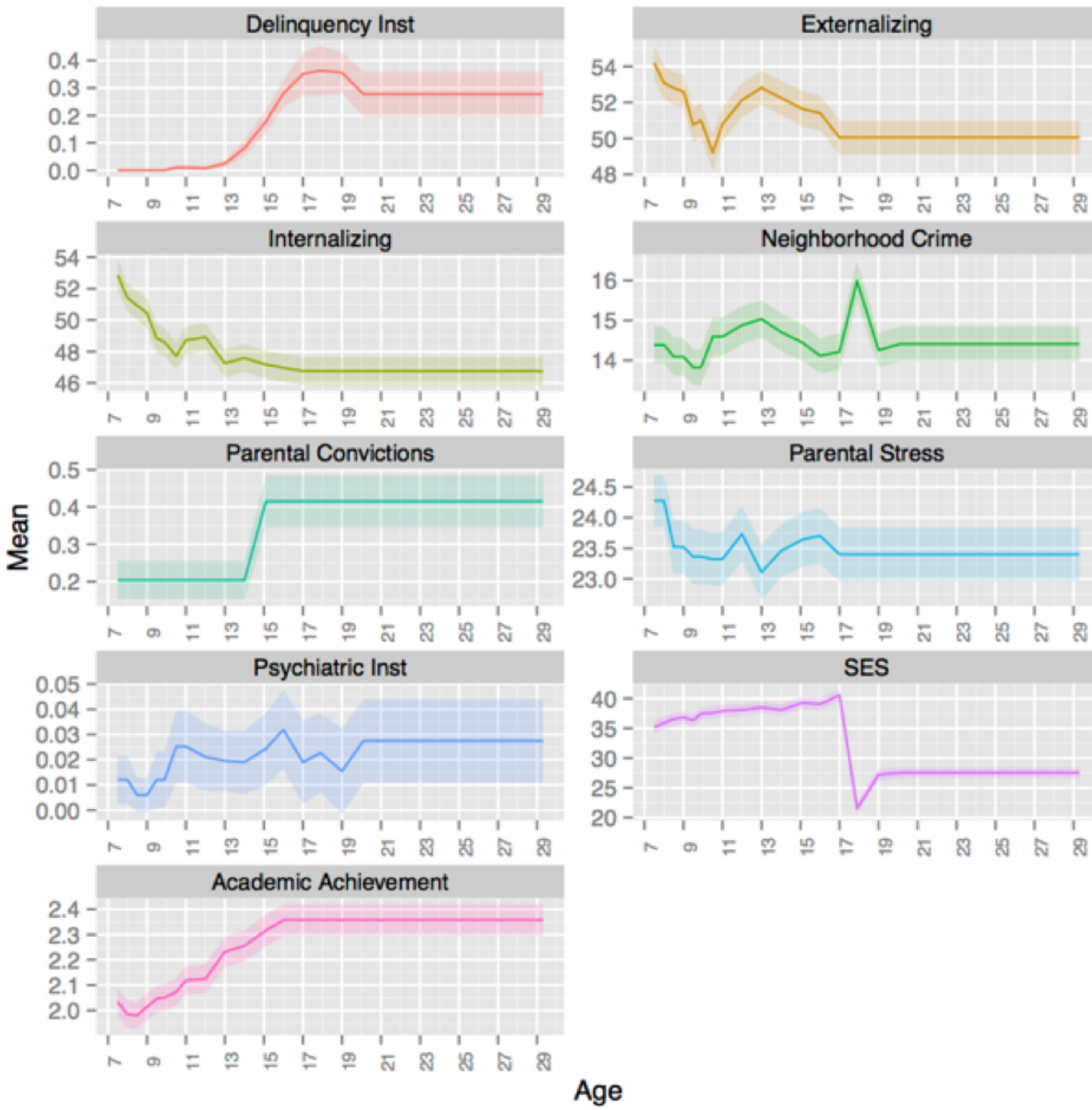

Figure 3.3. Means and 95% confidence intervals for continuous confounders

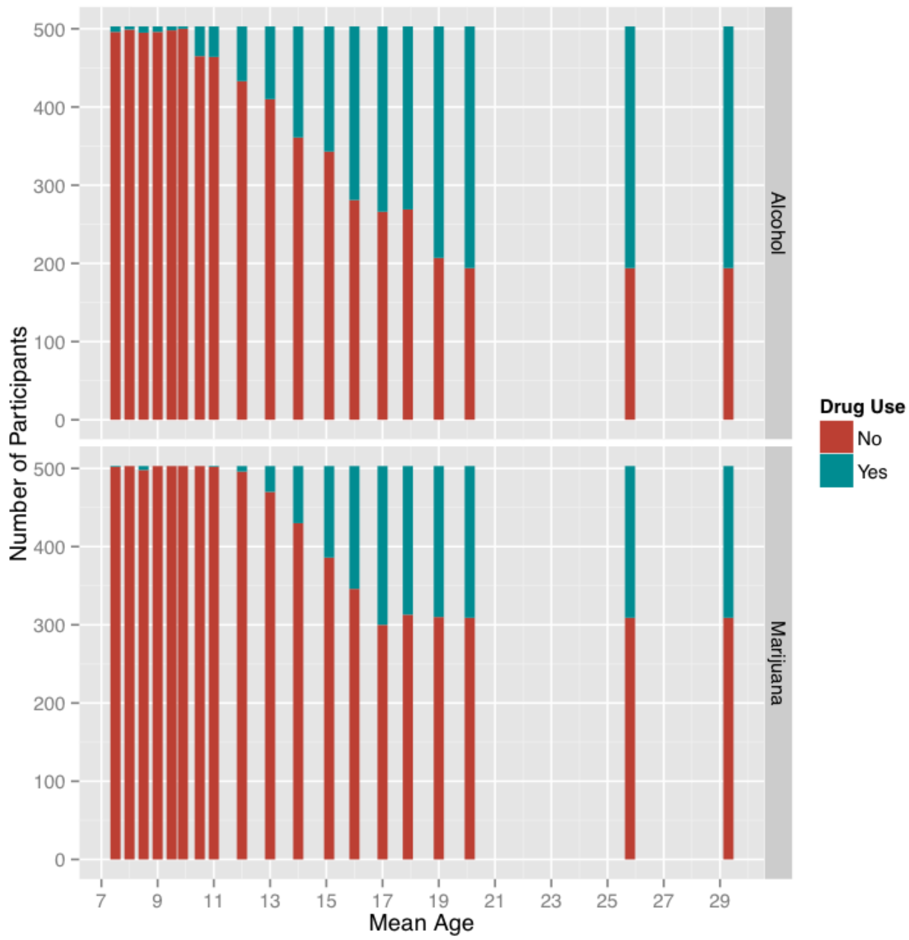

Figure 3.4. Relative frequencies of dichotomous confounders: alcohol and marijuana use

Table 3.1. Marginalized 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*	*β*	*95% CI*	*β*	*95% CI*
Raw outcome						
Arrest history	2.25	(0.87, 3.63)	5.33	(1.01, 9.64)	1.03	(0.33, 1.73)
Conviction history	1.01	(0.48, 1.54)	2.41	0.02, 4.79)	0.38	(0.12, 0.64)
Standardized Outcome						
Arrest history	0.13	(0.05, 0.2)	0.21	(0.04, 0.39)	0.21	(0.07, 0.34)
Conviction history	0.06	(0.03, 0.09)	0.1	(0.001, 0.19)	0.08	(0.02, 0.13)

Table 3.2. Fixed effect model estimates for the effect of change in exposure to the criminal justice system on change in antisocial attitudes, behaviors, and peers

	Total Antisocial Attitudes				**Total Antisocial Behaviors**				**Antisocial Peers**			
	Crude		**Adjusted**		**Crude**		**Adjusted**		**Crude**		**Adjusted**	
	β	*95% CI*	*β*	*95% CI*	*β*	*95% CI*	*β*	*95% CI*	*β*	*95% CI*	*β*	*95% CI*
Lagged total arrests	1.84	(1.13, 2.55)	1.74	(1.05, 2.43)	3.25	(2.15, 4.36)	2.9	(1.77, 4.03)	1.00	(0.76, 1.23)	0.84	(0.61, 1.07)
Lagged total convictions	0.57	(0.23, 0.9)	0.55	(0.22, 0.87)	0.07	(-0.48, 0.61)	-0.2	(-0.75, 0.37)	0.26	(0.15, 0.37)	0.2	(0.08, 0.3)

Note. Crude models contain lagged centered arrests or lagged centered convictions and age. Adjusted models contain the one-year-lagged and centered primary exposure, age, race, alcohol use, marijuana use, and two-year-lagged and centered outcome variable, other antisocial variables, academic achievement, internalizing t-score, externalizing t-score, neighborhood crime, parental convictions, parental stress, parental supervision, socioeconomic status, psychiatric institutionalization, and delinquency institutionalization.

3.9 Appendix

Table 3.3. Major criminogenic risk factors and DSM-5 criteria for antisocial personality and conduct disorders

Big Four Criminogenic Risk Factor and Description	
History of antisocial behavior	Early/continuing involvement in a variety of antisocial acts in a variety of settings
Antisocial personality pattern	Adventurous pleasure seeking, weak self-control, restlessly aggressive, impulsive, irritable
Antisocial cognition	Attitudes, values, beliefs, and rationalizations supportive of crime; cognitive emotional states of anger, resentment, and defiance
Antisocial associates	Close association with criminal others and relative isolation from anti-criminal others; immediate social support for crime

DSM-5 Section II Criteria for antisocial personality disorder

A. A pervasive pattern of disregard for and violation of the rights of others, occurring since age 15 years, as indicated by three (or more) of the following:
   1. Failure to conform to social norms with respect to lawful behaviors, as indicated by repeatedly performing acts that are grounds for arrest.
   2. Deceitfulness, as indicated by repeated lying, use of aliases, or conning others for personal profit or pleasure.
   3. Impulsivity or failure to plan ahead.
   4. Irritability and aggressiveness, as indicated by repeated physical fights or assaults.
   5. Reckless disregard for safety of self or others.
   6. Consistent irresponsibility, as indicated by repeated failure to sustain consistent work behavior or honor financial obligations.
   7. Lack of remorse, as indicated by being indifferent to or rationalizing having hurt, mistreated, or stolen from another.

B. The individual is at least age 18 years.

C. There is evidence of conduct disorder with onset before age 15 years.

D. The occurrence of antisocial behavior is not exclusively during the course of schizophrenia or bipolar disorder.

DSM-5 Section II Criteria for conduct disorder

A. A repetitive and persistent pattern of behavior in which the basic rights of others or major age-appropriate societal norms or rules are violated, as manifested by the presence of at least three of the following 15 criteria in the past 12 months from any of the categories below, with at least one criterion present in the past 6 months:

Aggression to People and Animals

1. Often bullies, threatens, or intimidates others.
2. Often initiates physical fights.
3. Has used a weapon that can cause serious physical harm to others (e.g., a bat, brick, broken bottle, knife, gun).
4. Has been physically cruel to people.
5. Has been physically cruel to animals.
6. Has stolen while confronting a victim (e.g., mugging, purse snatching, extortion, armed robbery).
7. Has forced someone into sexual activity.

Destruction of Property

8. Has deliberately engaged in fire setting with the intention of causing serious damage.
9. Has deliberately destroyed others' property (other than by fire setting).

Deceitfulness or Theft

10. Has broken into someone else's house, building, or car.
11. Often lies to obtain goods or favors or to avoid obligations (i.e., "cons" others).
12. Has stolen items of nontrivial value without confronting a victim (e.g., shoplifting, but without breaking and entering; forgery).

Serious Violations of Rules

13. Often stays out at night despite parental prohibitions, beginning before age 13 years.
14. Has run away from home overnight at least twice while living in the parental or parental surrogate home, or once without returning for a lengthy period.
15. Is often truant from school, beginning before age 13 years.

B. The disturbance in behavior causes clinically significant impairment in social, academic, or occupational functioning.

C. If the individual is age 18 years or older, criteria are not met for antisocial personality disorder

Table 3.4. Variables, constructs, instruments, and descriptive statistics for all analysis measures

Variable	**Construct**	**Instruments**	**Reliability α**	**Mean**	**SD**	**Min**	**Max**
Total Antisocial Attitudes	Attitudes toward delinquent behavior, perceptions of problem behavior, perceptions of likelihood of getting caught	Attitude to Delinquent Behavior Scale, Likelihood of Getting Caught Scale	.77 - .83	44.4	18.5	0.0	140.0
Total antisocial behaviors	Very minor, minor, moderate, serious	Self-Reported Delinquency Scale, Self-Reported Antisocial Behavior Scale	0.77 - 0.92	3.9	24.8	0.0	1002.0
Peer delinquency	Proportion of youth's peers who engaged in activities described above under "delinquent behaviors"	Self-Reported Delinquency Scale, Substance Use Scale	0.79 – 0.96	3.9	5.1	0.0	40.0
Total Arrests	Frequency of total arrests	Official records	NA	0.1	0.6	0.0	7.0
Total Convictions	Frequency of total convictions	Official records	NA	0.2	1.1	0.0	29.0
Internalizing t-score	Psychiatric symptoms, disorders	16 items based on National Youth Survey, lay interviews based on the DSM-III-R	Depression: 0.87	48.3	10.4	24.8	88.0
Externalizing t-score	Psychiatric symptoms, disorders	Revised Diagnostic Interview Schedule for Children	Inattention: 0.87 Hyperactivity: 0.87	51.3	11.0	29.2	90.0
Psychiatric institutionalization	Periods of psychiatric institutionalization	Official records	NA	0.0	0.2	0.0	4.0
Delinquency institutionalization	Periods of correctional institutionalization	Official records	NA	0.1	0.6	0.0	12.0
Academic performance	Achievement in reading, writing, math, spelling, and up to three other academic subjects	Caretakers' teachers' and youths' evaluations	0.46 – 0.56	2.2	0.7	0.5	4.3
Parental stress	Caretaker perceptions of stress in past month	Perceived Stress Scale	0.57 – 0.85	23.5	5.0	10.4	42.0
Parental supervision	Youth (and sometimes parents') perceptions of parental discipline, supervision.	Supervision/Involvement Scale		5.9	1.4	2.8	12.0

Parental criminal history	Frequency of parental arrests, charges, and convictions	Official records	NA	0.3	0.7	0.0	4.0
Neighborhood	Presence of prostitution, assaults, burglaries, etc.	The Neighborhood Sale	0.95	14.5	5.3	1.8	31.2
Socioeconomic status	Race, ethnicity, work, marital status, education of caretakers	The Demographics Questionnaire	NA	34.6	12.7	0.0	67.2
Alcohol use	Used alcohol in past assessment interval (Yes/No)	Substance use scale adapted from National Youth Survey	NA	% No: 73.9 % Yes: 26.1			
Marijuana use	Used marijuana in past assessment interval (Yes/No)	Substance use scale adapted from National Youth Survey	NA	% No: 83.7 % Yes: 16.3			

Table 3.5. Example items for constructed antisocial behavior and attitude scales

Antisocial behavior example measures

Very minor:

In the past six months, have you …
…on purpose broken or damaged or destroyed something belonging to your parents or…?
…taken money at home that did not belong to you?
…taken anything other than money that did not belong to you?
…written things or sprayed paint on walls or sidewalks or cars where you were not supposed to?

Minor:

In the past six months, have you…
…taken something from a store without paying for it?
…taken anything at school that did not belong to you?
…purposely set a fire or tried to do so?
…avoided paying for things?

Moderate:

In the past six months, have you….
…stolen or tried to steal things worth between \$5 and \$50?
…\$50 and \$100?
…>\$100?
…snatched someone's purse or wallet or picked someone's pocket?
…taken something from a car that did not belong to you?
…knowingly bought, sold, or held stolen goods or tried to do any of these things?
…gone joyriding, that is, taken a motor vehicle, such as a car or motorcycle…?
…used checks illegally or used a slug or fake money to pay for something?
…used or tried to use credit cards or bank cards without the owner's permission?
…been involved in a gang fight?

Serious:

How many times in the past six months have you…
…gone into or tried to go into a building to steal something?
…stolen or tried to steal a motor vehicle such as a car or motorcycle?
…attacked someone with a weapon or with the idea of seriously hurting or killing them?
…used a weapon, force, or strong-arm methods to get money or things from people?
…physically hurt or threatened to hurt someone to get them to have sex with you?
…had or tried to have sexual relations with someone against their will?

Antisocial attitudes example measures

How wrong do you think it is for someone your age… <and>
What do you think the likelihood is that you would be caught by the police if you…
… to skip school without an excuse?
…lie, disobey, or talk back to adults…?
…purposely damage or destroy property that did not belong to you?
…steal something worth less than \$5, \$50, or \$100, respectively?
…go joyriding, that is, take a motor vehicle such as a car or motorcycle…?
…hit someone with the idea of hurting that person?
…attack someone with a weapon with the idea of seriously hurting them?
…use a weapon force, or strong arm methods to get money or things from people?
…sell hard drugs such as cocaine, heroin, or LSD?
…use alcohol, marijuana, hashish, cocaine, heroin, or LSD, respectively?

Chapter 4

Risk in individuals or risk in populations: Do criminogenic risk factors explain group differences in arrest and conviction rates?

4.1 Abstract

The "fourth generation" of the criminogenic risk assessment framework promises not only to assess risk, but also to reduce it by intervening on dynamic and manipulable risk factors. This ability to assess and reduce risk has been trumpeted as the final word in debates about not only the causes of individual variation in criminal behavior, but the very roots of crime, and even as a means of mitigating mass incarceration. That is, evidence for recidivism prediction and reduction is sometimes interpreted as evidence about the causes of *crime rates* and the incidence of individuals' contact with the criminal justice system. The present study challenges the logic of this conceptual slippage with a relatively simple empirical test: do differences in levels of criminogenic risk factors explain population patterns in the frequency of arrests and convictions? Data are from a prospective cohort of 503 boys assessed before their first contact with the criminal justice system, and every 6 to 12 months through early adulthood. Antisocial attitudes, behaviors, and peers were ascertained with validated measures and arrests and convictions were ascertained through official records. Mediation analysis found that antisocial attitudes, behaviors, and peers do not explain group differences in arrest and conviction rates. Fully elaborated causal models of the relationship between antisocial characteristics criminal justice outcomes are needed to maximize the efficiency of high-risk prevention strategies. Researchers and policymakers should more cautiously communicate the scope of reform that the criminogenic risk framework can deliver.

4.2 Introduction

Over the past several decades, criminogenic risk assessment has become an integral, "evidence-based" component of the criminal justice system (e.g., National Institute of Corrections, 2010). Currently, in what proponents call the "fourth generation" of the criminogenic risk assessment framework, there has been a shift in focus to not only assess risk, but also to *reduce* it by intervening on aspects of criminogenic risk factors that are dynamic and manipulable (Andrews, Bonta, & Wormith, 2006). Indeed, supervision and treatment strategies that target criminogenic risk factors have been shown to modestly reduce recidivism (Andrews & Dowden, 2006; Andrews et al., 1990, 2006; Dowden & Andrews, 1999a, 1999b; Lowenkamp, Latessa, & Smith, 2006; Skeem, Manchak, & Peterson, 2011). This ability to assess and reduce risk has been trumpeted as the final word in theory and policy debates about not only the causes of individual variation in criminal behavior, but the very roots of crime (Andrews & Bonta, 2010, p. 18, 70, 114-121, 159-191, 306-307, 531-533), and even as a means of mitigating mass incarceration (Bonta, 2007; Monahan & Skeem, 2016; Stimson & Appelbaum, 2004). That is, evidence for recidivism reduction is sometimes interpreted as evidence about the causes of *crime rates* and the incidence of individuals' contact with the criminal justice system. The present study argues that these latter interpretations are mistaken, and challenges the logic of this conceptual slippage with a relatively simple empirical test: do differences in levels of criminogenic risk factors explain population patterns in the frequency of arrests and convictions?

Risk assessment instruments are based on research that identifies strong correlates of recidivism among individuals under correctional supervision (Andrews & Bonta, 2010, p. 132-133), and then uses individuals' scores on those variables 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 consistently predict recidivism in justice-involved samples (Dowden & Andrews, 1999b; Gendreau, Little, & Goggin, 1996; Landenberger & Lipsey, 2005; Lipsey & Derzon, 1998; Simourd & Andrews, 1994)—hence the label *criminogenic*. And as noted, programs that focus on "high-risk individuals" seem to reduce recidivism rates. 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 improve programmatic and budgetary efficiency and effectiveness, and be smarter about policy, resource, supervision, and treatment allocation throughout the criminal justice system (Andrews & Bonta, 2010; Lowenkamp et al., 2006).

When addressed at appropriate problems and implemented with fidelity, criminogenic risk assessment and reduction strategies should indeed improve efficiency, effectiveness, and allocation in certain areas of the criminal justice system. But as criminogenic risk assessment expands from the back-end of the system to the front, in pre-trial processing, sentencing, and even policing (Desmarais & Singh, 2013; Gottfredson & Moriarty, 2006; Lowenkamp & Whetzel, 2009; Storey, Kropp, Hart, Belfrage, & Strand, 2014; Summers & Willis, 2010; Trujillo & Ross, 2008; VanNostrand & Keebler, 2009), the framework is in danger of promising more than it can deliver for criminal justice reform. Moreover, as it becomes an increasingly dominant discourse, the criminogenic risk framework has begun to exhibit less tolerance for critical interrogation (e.g., Bonta, 2007). Yet, moments of reform and expansion are precisely when such critique is most needed, both to ensure that opportunities for deeper system transformations are not preempted or coopted by sheer momentum, and to prevent errors and unintended harmful consequences from becoming magnified and ossified. With regard to reducing crime rates, mitigating mass incarceration, or explaining the origins of crime, the criminogenic risk framework may be an example of this overreach—of providing "the right answer for the wrong question" (Schwartz & Carpenter, 1999). This insight, elaborated below, was a critique of the "risk factorology" approach to prevention that had come to dominate epidemiology for most of its modern history (e.g., McKinlay & Marceau, 2000; McMichael, 1999; Rose, 1985). The insights from these debates also motivate the present analysis.

4.2.1 Conceptual framework

Policies and practices that target individual-level risk factors are what the epidemiologist Geoffrey Rose (1985) called "high-risk" strategies, because such approaches focus on a particular type of etiologic question and resultant view of prevention. The etiologic question to which the high-risk strategy responds is about identifying the causes of cases, rather than the causes of incidence. That is, it addresses the question "*Why do some people engage in crime?*" rather than the question *"Why is the crime rate higher in group (or place or time) A than group (or place or time) B?"* Rose's point is that we may be able to provide a complete and detailed answer to the first question, and still have no answer to the second, because:

> The answer to that question has to do with the determinants of the population mean; for what distinguishes the two groups is nothing to do with the characteristics of individuals, it is rather a shift of the whole distribution—a mass influence acting on the population as a whole. To find the determinants of prevalence and incidence rates, we need to study characteristics of populations, not characteristics of individuals. (1985, p. 428)

Because the focus of the high-risk strategy is on identifying factors that put people at high risk, its target for prevention is high-risk individuals. The prevention goal of the high-risk strategy is to identify people with high susceptibility and provide them some individual protection (Rose, 1985), e.g., screening for high blood pressure and prescribing medication to prevent more serious cardiovascular disease. Thus, the high-risk strategy is about truncating the right tail of a risk distribution rather than shifting the distribution's mean (Rose, 1985). A population strategy, in contrast, is about uncovering that mass influence acting on the population as a whole—a phenomenon also known as "ubiquitous exposure"—and targeting it to lower the mean level of risk, shift the risk distribution in a favorable direction, and alter the underlying causes that make an outcome common (Rose, 1985). For example, dietary fat intake may be the primary cause of a population's incidence of coronary heart disease, but in some Western countries, the entire distribution of intake is above the threshold for being a detectable individual risk (Rose, 1985). The population strategy thus implies more radical, upstream interventions, such as banning trans fats in food production.

Epidemiology's alleged prioritization of the high-risk approach was the subject of debates that emerged about the failures of "risk factor epidemiology" in the years following Rose's insights (e.g., McMichael, 1999; Schwartz & Carpenter, 1999; Schwartz, Susser, & Susser, 1999; Shy, 1997; Susser, 1998). A concern was that we were implementing high-risk strategies yet mistakenly regarding them as population strategies, and then wondering why we were not solving the problems we thought we were solving (for an example regarding homelessness, see Shinn, 1992 and Schwartz & Carpenter, 1999). The idea that a framework designed around individual-level risk factors for recidivism-triggering behaviors might help reduce crime rates is analogous.

However, the analogy—much like the notion that criminogenic risk assessment can mitigate mass incarceration—is fraught with construct ambiguity. This is because, in the United States, *engaging in criminal behavior* is not the same as *detected crime,* which is also not the same as *contact with the criminal justice system*. In fact, there is conceptual and empirical distance between the constructs of criminal behavior, detected crime, and arrest/incarceration/recidivism, and the quantitative correspondence between them is actually tenuous (e.g., Braga & Weisburd, 2010; Kakade et al., 2012; King, Mauer, & Young, 2005; Roeder, Eisen, & Bowling, 2015; Skogan & Frydl, 2004). Yet, the criminogenic risk framework (like much criminal justice research) often conflates these constructs by using recidivism rates as proxies for criminal behavior. On the face of it, such a conflation lacks validity: we do not need to conduct a study to know that mass incarceration was not likely caused by a concordant increase in the incidence of antisocial personality characteristics in the United States population. Nevertheless, and to avoid arguing from the extreme, it is worth further unpacking the logic of this reasoning. We thus grant, for expository purposes, that *engaging in criminal behavior* is synonymous with *contact with the criminal justice system* and *crime rates*, despite the problems this entails.

4.2.2 Empirical setup

There are numerous ways to test whether differences in the prevalence and incidence of the "Big Four" criminogenic risk factors explain differences in crime rates between populations, or changes in crime rates over time. One option would be to look at two jurisdictions with different crime rates, and then compare the levels of residents' antisocial characteristics. Another option might be to explore historical records for a single jurisdiction over different periods of time. However, obtaining data on relevant individual, institutional, and structural confounders, such as demographic distributions and differences or changes in legal codes or criminal justice policies, could be daunting. A third approach is to determine whether differences in individual susceptibility to criminal behavior explain differences in contact with the criminal justice system between groups, in the same place and during the same time period. That is, if antisocial characteristics tap into the origins of criminal behavior/contact with the criminal justice system, then a group with higher contact with the criminal justice system should have higher levels of antisocial characteristics. In turn, controlling for this purported mediator (antisocial characteristics) should reduce or remove the observed effect of group membership on contact with the criminal justice system.

In the United States, known racialized disparities in contact with the criminal justice system may provide the necessary conditions for such a test. Racialized groups in the United States are overrepresented in the criminal justice system. In 2010, Blacks made up roughly 13%

of the US population but 38% of the jail population and 37% of the prison population, and Latinos made up roughly 16% of the US population and 16% of the jail population, but 34% of the prison population (Guerino, Harrison, & Sabol, 2011; Minton, 2011; U.S. Census Bureau, 2010). To put these disparities in perspective, among white men over the age of 18, 1 in 106 was incarcerated in 2008, but the number for Blacks was 1 in 15 and for Latinos 1 in 36 (Pew Center on the States, 2008). When community corrections (probation and parole) are included, the number of adults of any age or gender under some form of correctional supervision is 1 in 45 for Whites, 1 in 11 for Blacks, and 1 in 27 for Latinos (Pew Center on the States, 2011).

The logic of the criminogenic risk framework's claims about reducing crime rates and mitigating mass incarceration imply that these persistent racialized group differences in contact with the criminal justice system are at least partially due to differences in the prevalence and incidence of antisocial characteristics. And while a portion of these racialized disparities is of course due to structural and institutional bias, discrimination, and inequality in the *response* to antisocial behaviors, another portion is due to differences in real crime rates (Garland, 2013; Mauer, 2006). The leap in logic, made by some proponents of the criminogenic risk framework, from higher crime rates to higher criminogenic risk, is therefore understandable. This study's empirical setup is accordingly designed to enable a strong test of this hypothesis, on the criminogenic risk framework's own terms.

Before proceeding, however, it is important to recognize that this empirical setup opens the door to potentially racist and otherwise regressive interpretations that the author neither intends nor condones. One misinterpretation would be that the empirical setup concedes a reductionist view of race and antisocial characteristics; however, even if antisocial characteristics were to fully mediate racialized disparities in arrest and conviction rates, it would not follow that these characteristics, or their causal relationship with racialized group membership, are psychologically or biological essential, as opposed to contingencies of social location. Second, if antisocial characteristics do explain group differences in arrests and convictions, another misinterpretation would be that individual-level interventions on antisocial characteristics are, after all, the appropriate targets for reducing disparities in arrest and conviction rates. However, this does not follow because a causal effect is only equivalent to an intervention effect under strict assumptions (Gatto, Campbell, & Schwartz, 2014; Greenland, 2005; Pearl, 2014), and there would likely be mediators of the relationship between racialized group membership and antisocial characteristics that might prove more efficient, effective, and ethical targets for action. Even though the logic of the criminogenic risk framework might beckon these sorts of misinterpretations, racialized group differences in contact with the criminal justice system provide a convenient and stark test of the framework's implicit claims: groups with higher crime rates should have higher levels of antisocial characteristics. But the very real potential for reactionary policies to emerge from the criminogenic risk framework's psychologically reductionist view of crime and mass incarceration is among the reasons why the present study seeks to challenge the logic of the framework's conceptual and programmatic overreach.

The present study, then, tests whether differences in antisocial attitudes, behaviors, and peers mediate differences among whites and people of color in rates of criminal justice system contact. If the Big Four criminogenic risk factors tap into the origins of criminal behavior, and thus are to be of any use in reducing crime rates or mitigating mass incarceration, then they should account for group differences in rates of contact with the criminal justice system. The present study gauges the ability of the criminogenic risk framework to account for one of the most salient features of contemporary criminal justice system involvement—racial disparities in arrest and conviction rates. It puts to the test the notion that any proposed etiologic story about contact with the criminal justice system should be consistent with observed patterns of criminal justice system involvement over time or across populations.

This study is uniquely positioned to test these claims, as it uses data from a cohort of boys followed into adulthood with exceptional measures of antisocial characteristics and other potentially criminogenic variables, which were assessed prior to their first contact with the criminal justice system and thereafter. The fact that this sample was not already involved in the criminal justice system at baseline distinguishes it from most research on criminogenic risk factors, and reduces the potential selection bias that arises when comparison groups never had the possibility of not being involved in the criminal justice system, thus invalidating inferences about antisocial characteristics as causes of criminal behavior more broadly.

4.3 Methods

4.3.1 Sample

Data are from the youngest cohort of the Pittsburgh Youth Study, a prospective cohort of 503 boys from Pittsburgh public schools established in 1986. The study was undertaken by the Office of Juvenile Justice and Delinquency Programs' Program of Research on the Causes and Correlates of Delinquency, to understand the development of juvenile offending, mental health problems, drug use, and their risk factors in inner-city boys.(Loeber et al., 2012; Loeber, Farrington, Stouthamer-Loeber, Raskin White, & Wei, 2008; Pardini, Loeber, Farrington, Stouthamer-Loeber, & Stouthamer–Loeber, 2012)

The study's sample and design have been described extensively elsewhere.(Loeber et al., 2012, 2008; Pardini et al., 2012) First-grade boys in virtually all public schools in downtown Pittsburgh (*N*=31) were recruited in 1987-1988. Roughly 85% agreed to participate, and a random sample was selected for initial screening for antisocial behavior. This multi-informant screening drew on parent, teacher, and youth reports. Boys in the upper 30% of antisocial behaviors (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, reflecting the racial composition of Pittsburgh public schools at the time. The average age at screening was 7.

4.3.2 Design

The cohort was assessed nine times in 6-month increments from age six to nine and 10 times in yearly increments from ages 10 to 20. Participants were assessed again at ages 25 and 28. Boys were interviewed with their primary adult caretakers, mostly in their homes, until age 16. Primary caretakers and teachers also completed self-administered questionnaires. Prior to the assessment, caretakers and teachers provided written informed consent; adolescents provided assent until age 17 and consent thereafter. The data collection procedures were approved by the institutional review board at the University of Pittsburgh. As the present study is a secondary analysis of de-identified data, the Columbia University institutional review board determined that additional review was not warranted.

4.3.3 Measures

Figure 4.1 summarizes, longitudinally, the primary exposures and outcomes of interest in the sample, by racialized group membership. Figures 4.2 and 4.3 summarize additional mediating characteristics of the sample over time. The instruments used to measure the characteristics of the sample are described in detail below, and Appendix Table 4.5 summarizes measure items and descriptive statistics.

4.3.3.1 Outcomes: Criminal Justice System Contact

The present study uses constructed variables that measure the count, per assessment interval, of adolescents' total arrests and convictions. These data were gathered from official records from the Allegheny County Juvenile Court, Pennsylvania Juvenile Court Judges' Commission, Pennsylvania State Police Repository, and the Federal Bureau of Investigation. Data on arrests and convictions are not linked.

4.3.3.2 Exposure: racialized group membership

Participant race was ascertained from adolescents' caretakers at screening. At baseline, the sample included 204 (41%) white adolescents, 280 (56%) Black adolescents, 1 (0.2%) Latino adolescent, 5 (1%) Asian adolescents, and 13 (2.6%) "mixed race" adolescents. Due to the small numbers for the latter three categories, they were combined with the Black category to produce two groups: white (N=204) and "person of color" (N=299).

4.3.3.3 Purported mediators: cumulative antisocial attitudes, behaviors, and peers

The present study uses constructed variables in data from the Pittsburgh Youth Study that summarize antisocial attitudes, behaviors, and peers, which map onto constructs used in commonly used risk assessment instruments such as the Level of Services Inventory (Andrews, Bonta, & Wormith, 2004): antisocial attitudes, antisocial behaviors, and antisocial peers. A history of antisocial behavior is accounted for in the modeling strategy described below.

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 = 0.73 – 0.83, internal consistency = 0.91) (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 = 0.9) (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 = 0.77 – .8, internal consistency = 0.91) (Pardini et al., 2012; Zhang et al., 1997). The total attitudes variable was used to create lagged cumulative mean values for each assessment interval: for each individual, their cumulative mean attitudes value at time T represents the cumulative mean of their total attitude scores up to time $T - 1$. This variable can be interpreted as participants' cumulative history of antisocial attitudes up to the assessment point prior to an arrest or conviction. The total attitudes scale ranged from 0 to 140, with higher values representing more antisocial attitudes. The cumulative version of the scale ranged from 19 to 84.

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, & Klein, 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). As with cumulative attitudes, the total behaviors variable was used to create lagged cumulative mean values for each assessment interval: for each individual, their cumulative mean behaviors value at time T represents the cumulative mean of their total behaviors scores up to time T-*1.* This variable can be interpreted as participants' cumulative history of antisocial behaviors up to the assessment point prior to an arrest or conviction. The total behaviors scale ranged from 0 to 1,002. The cumulative version of the scale ranged from 0 to 94.45

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, & van Kammen, 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=0.92 (Pardini et al., 2012). This variable was used to create lagged cumulative mean values for each assessment interval: for each individual, their cumulative mean affiliation with antisocial peers value at time *T* represents the cumulative mean of their peer delinquency scores up to time *T-1.* This variable can be interpreted as participants' cumulative history of affiliating with antisocial peers up to the assessment point prior to an arrest or conviction. The peer delinquency variable ranged from 0 to 40. The cumulative version ranged from 0 to 16.1.

4.3.3.4 Mediator—Outcome Confounders

The present study considers the effects of constructs that may confound the relationship between the mediators of interest (M) and the outcomes of interest (Y), i.e., the confounders of the relationship between antisocial characteristics and criminal justice outcomes. If left uncontrolled, M-Y confounders will bias the indirect and direct effects of interest (Cole & Hernán, 2002; Hafeman, 2008; Robins & Greenland, 1992). These potential confounders include psychopathology, substance use, institutionalization, academic achievement, parenting factors, parental criminal history, neighborhood factors, and sociodemographic factors.

Internalizing and externalizing t-scores. Internalizing and externalizing problems were measured with the Childhood Behavioral Checklist (CBCL) (Achenbach, 1991a, 1991b; 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 externalizing scale represents the sum of 27 items that loaded onto "delinquent behavior" and "aggressive behavior" clinical syndrome scales. One-week test-retest stability coefficients are .89 for internalizing problems and .93 for externalizing problems (Achenbach, 1991b; Youngstrom et al., 2000).

Alcohol and marijuana 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 or never used alcohol or marijuana in the period prior to assessment.

Institutionalization. Youth institutionalization for a variety of psychopathological or behavioral problems was assessed with the Family Health Questionnaire (Loeber et al., 2008).

Academic performance. Performance in school was measured though 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. In addition, information was collected on the youth's feelings about and behavior in school. The construct was created by taking the mean of all ratings across informants (internal consistency $\alpha = 0.81$) (Pardini et al., 2012). This measure is coded such that higher scores represent worse academic performance.

Parenting factors. 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). Parental supervision was measured by the Supervision/Involvement Scale, a 43-question scale, which was administered to both parents and youth, and assessed parents' supervision style, with values ranging from closely supervised to poorly supervised (Loeber, Stouthamer-Loeber, Morris, & Tonry, 1986).

Parental convictions. Lifetime data on mothers' and fathers' history of arrest and conviction were collected via caretaker self-report (Loeber et al., 2008). Mothers' and fathers' convictions were summed to create a "parental conviction" score.

Neighborhood characteristics. Neighborhood characteristics were assessed by the Neighborhood Scale (Loeber et al., 1998) and measured the caretakers' perceived quality of the neighborhood in which their families resided. This instrument contained 17 items covering the presence of prostitution, assaults, burglaries, and similar problems in the neighborhood.

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).

4.3.4 Analysis

All analyses were conducted in R version 3.2.2.

4.3.4.1 Missing data

Missing data were imputed using R package 'mice' (van Buuren & Groothuis-Oudshoorn, 2011) for "multivariate imputation by chained equations," an implementation of fully conditional specified models for imputation. Missing data were only imputed in the independent variables. In the fully conditional approach, a multivariate imputation model is specified on a variable-by-variable basis, rather than the more traditional joint modeling approach (van Buuren & Groothuis-Oudshoorn, 2011). The fully conditional approach is used when no suitable multivariate distribution can be found. The present study implemented MICE with the random forest method for imputation. The random forest method is an extension of classification and regression trees, which recursively subdivides the data based on values of predictor variables, and uses bootstrap aggregation of multiple regression trees to reduce overfitting (Shah, Bartlett, Carpenter, Nicholas, & Hemingway, 2014). This approach does not rely on distributional assumptions and can accommodate nonlinear relations and interactions (Shah et al., 2014). After imputation, for phases in which particular measures were not assessed, the last observation was carried forward.

4.3.4.2 Modeling strategy

This study is interested in determining whether their remains a controlled direct effect of racialized group membership on arrests and convictions after blocking the pathway through antisocial characteristics. The controlled direct effect is of interest here because it considers what the effect of racialized group membership would be if we were to intervene on antisocial characteristics within a population, fixing them at a certain level (Hafeman & Schwartz, 2009; Robins & Greenland, 1992; Vanderweele, 2015). This is analogous to implementing a hypothetical high-risk strategy to target criminogenic risk factors as a means of reducing incarceration rates.

Step one in the analysis is to determine whether, consistent with exhaustive empirical evidence from across the United States, adolescents of color in Pittsburgh experienced higher rates of arrest and conviction than their white counterparts. A series of Poisson Generalized Estimating Equation (GEE) models were fit to estimate the age-adjusted effects of racialized group membership on rates of arrest and conviction, accounting for clustering of observations within individuals over time. The first set of models regressed individuals' racialized group and age on arrests and convictions, respectively. These represent the crude estimates of the effect that the totality of experience of being a person of color has on contact with the criminal justice system.

Step two is to determine whether antisocial attitudes, behaviors, and peers meet traditional criteria to be considered mediators. That is, are lagged cumulative antisocial attitudes, behaviors, and peers associated with both the exposure (racialized group membership) and outcomes (arrests and convictions)? For the criminogenic risk framework to account for group differences in contact with the criminal justice system, these associations would have to exist, despite the fact that extant epidemiologic data do not reveal any consistent racialized differences in many of the constructs that underlie the Big Four criminogenic risk factors (Black & Blum, 2015; Compton, Conway, Stinson, Colliver, & Grant, 2005). Preliminary analyses revealed that two of the three antisocial characteristics were moderately associated with racialized group membership: people of color had slightly higher antisocial attitudes (β = 1.59 [95% CI: 0.4 – 2.78]) and affiliation with antisocial peers (β = 1.55 [95% CI: 1.1 – 2.0]), but no significant differences in antisocial behaviors. (β = 0.31 [95% CI: -1.00 – 1.63]). Antisocial attitudes, behaviors, and peers were associated with arrests and convictions. A standard deviation difference in antisocial attitudes increased an individual's rate of arrest by 1.7 (95% CI: 1.58 – 1.89), a standard deviation difference in antisocial behaviors increased the arrest rate by 1.13, for for antisocial peers, a standard deviation difference increased the arrest rate by 1.35 (95% CI: 1.23 – 1.47). For convictions, the rate ratios (95% confidence intervals) for a standard deviation difference in antisocial attitudes, antisocial behaviors, and antisocial peers were 1.18 (1.12 – 1.25), 1.01 (1.00 – 1.03), and 1.16 (1.11 – 1.21), respectively. Therefore, traditional methods to assess mediation—adjusting for the mediator and examining for change in the exposure coefficient—are employed (Baron & Kenny, 1986; MacKinnon, Fairchild, & Fritz, 2007). In a second set of Poisson GEE models, the crude models were adjusted for lagged cumulative antisocial attitudes, behaviors, and peers, both respectively and simultaneously.

However, in order to estimate unbiased controlled direct effects, three assumptions must be met: 1) there must be no unmeasured confounding of the exposure–outcome relationship, i.e., of racialized group membership and arrest and conviction rates; 2) there must be no interaction between the exposure and the mediators; and 3) there must be no unmeasured confounding of the mediator–outcome (M-Y) relationship i.e., of antisocial characteristics and arrest and conviction rates (Robins & Greenland, 1992; Vanderweele, 2015). Figure 4.4a shows a directed acyclic graph of a causal model that violates these assumptions.

Regarding the first assumption, it is highly unlikely that there are confounders of the exposure–outcome relationship in the present study, because strictly speaking, there are likely no factors that cause an individual's racialized group membership (e.g., their phenotype), that also independently cause their arrest and conviction rates. A crude measure of "race" is only ever a rough proxy for the totality of an individual's experiences in a racialized social location, and therefore those experiences are mediators, or pathways of interest, not confounders. In Figure 4.4a, then, C_1 is unlikely to exist. Regarding the second assumption, a series of Poisson GEE models were fit and found no multiplicative effect measure modification between racialized group membership and antisocial characteristics, with the exception of minimal effect measure modification between racialized group membership and antisocial behaviors on arrests (See Appendix Table 4.6).

Regarding the third assumption, Figure 4.4a shows that if the individual, family, and contextual factors (C_2 in the figure) described above are positive M-Y confounders, the effect of leaving them uncontrolled should serve to *underestimate* the direct effect of racialized group membership on arrests and convictions and *overestimate* the indirect effect through antisocial characteristics (VanderWeele, 2016). Therefore, the most generous test of the criminogenic risk framework would leave these potential M-Y confounders out of the mediation model. However,

Figure 4.4b is another, potentially more realistic, causal model. In Figure 4.4b, there is exposure-induced confounding of the mediator and outcome. If this is the case, *controlling* for the M-Y confounders may 1) reduce M-Y confounding (and *increase* the direct effect), 2) block part of the legitimate mediational pathway from racialized group through C_2 through antisocial characteristics (and thus *underestimate* the indirect effect of interest), or 3) block a legitimate alternative pathway from racialized group through C_2 (and thus *underestimate* the direct effect). Leaving the M-Y confounders *uncontrolled* will leave the legitimate mediational pathway open (*increasing* the indirect effect), and permit some M-Y confounding (*underestimating* the direct effect). Therefore, it is again a more generous test of the criminogenic risk framework to leave the M-Y confounders out of the model.

Nonetheless, models are presented with and without M-Y confounders as a sensitivity analysis. Step three is thus to account for potential M-Y confounders. A third set of Poisson GEE models were fit to determine whether cumulative antisocial attitudes, behaviors, and peers, respectively and simultaneously, continued to mediate the relationship between racialized group and arrest and racialized group and convictions, after control of M-Y confounders, by adding the lagged potential confounders described above to the models. Finally, a fourth set of models controlling for these confounders, but not antisocial characteristics, was fit as an additional sensitivity analysis to provide an approximation of the potential remaining direct effect of racialized group membership after blocking alternative pathways through C_2 (although this includes some portion of the mediated effect through antisocial characteristics). The sensitivity analysis is approximate because regression techniques are unable to estimate unbiased controlled direct effects in the presence of exposure-induced confounding (Vanderweele, 2015), but the present study is less concerned with estimating the precise magnitude of this effect, and more concerned with establishing whether there is any remaining direct effect at all.

4.4 Results

Figures 4.1 – 4.3 show the means and 95% confidence intervals (CI) for individual, family, and contextual characteristics over the study period, by racialized group status. People of color tended to have higher arrest and conviction counts than whites across the life course. They had roughly equivalent levels of antisocial attitudes, slightly higher antisocial behaviors and more affiliation with antisocial peers, lived in worse neighborhoods, and had lower socioeconomic status than whites, and their parents tended to have higher levels of stress and a history of more convictions. Compared with white participants, people of color had worse academic performance and more frequent institutionalizations for delinquency. White participants and participants of color had equivalent internalizing and externalizing problems, and similar proportions used marijuana. Compared with white participants, a smaller proportion of participants of color reported using alcohol.

As shown in Table 4.1, Model 1, the age-adjusted arrest rate for people of color was 1.99 times higher (95% CI: 1.46 – 2.72) than the arrest rate for white participants. After adjusting for lagged cumulative antisocial attitudes (Table 4.1, Model 2), the rate ratio dropped by roughly 7.5% to 1.84 (95% CI: 1.36 – 2.48). This suggests that antisocial attitudes do not appreciably mediate the relationship between racialized group and arrest rate. After adjusting for lagged cumulative antisocial behaviors (Table 4.1, Model 3), the arrest rate ratio for people of color compared with whites was not appreciably different than the crude estimate. This suggests that antisocial behaviors do not mediate the relationship between racialized group and arrest rate. Adjustment for antisocial peers caused the rate ratio for racialized group to drop by 14.6%, to 1.7 (95% CI: 1.25 – 2.34), which suggests that antisocial peers modestly mediate the relationship between racialized group and arrest. However, adjusting for antisocial attitudes, behaviors, and peers simultaneously (Table 4.1, Model 5) resulted in only a 7% reduction in the effect of racialized group on arrests: the rate ratio was 1.85 (95% CI: 1.37 – 2.48).

Table 4.2 shows results from models that sequentially tested whether racialized group continues to have a direct effect on arrests after adjusting for M-Y confounders. Table 4.2 shows that individual, family, and contextual characteristics do influence the effect of racialized group on arrests through antisocial attitudes, behaviors, and peers. Adding M-Y confounders to the model reduced the effect of racialized group membership on arrests through antisocial attitudes (Table 2, Model 1) by roughly 9.8%. The rate ratio for racialized group membership dropped from 1.84 to 1.66 (95% CI: 1.25 – 2.22). Adding confounders to the model reduced the effect of racialized group on arrests through antisocial behaviors (Table 2, Model 2) by roughly 15.7%. The rate ratio dropped from 1.97 to 1.66 (95% CI: 1.24 – 2.22). The confounders reduced the effect of racialized group on arrests through antisocial peers (Table 2, Model 3) by roughly 7.6%. The rate ratio dropped from 1.7 to 1.58 (95% CI: 1.17 – 2.14) When M-Y confounders were added to the model that included all three antisocial mediators, the effect of racialized group membership dropped by 5.9%. The rate ratio for racialized group membership dropped from 1.85 to 1.74 (95% CI: 1.31 – 2.33).

The age-adjusted conviction rate for people of color was 1.57 times higher (95% CI: 1.06 – 2.34) than the conviction rate for white participants (Table 4.3, Model 1). After adjusting for lagged cumulative antisocial attitudes (Table 4.3, Model 2), the rate ratio dropped by 8.9% from 1.57 to 1.43 (95% CI: 0.97 – 2.12). This suggests that antisocial attitudes do not appreciably mediate the relationship between racialized group and conviction rate. After adjusting for lagged cumulative antisocial behaviors (Table 4.3, Model 3), the conviction rate ratio did not change. This suggests that antisocial behaviors do not mediate the relationship between racialized group and conviction rate. Adjusting for antisocial peers reduced the effect of racialized group on convictions by 19.1%. The rate ratio dropped from 1.57 to 1.27, 95% CI: 0.86, 1.88), and the coefficient for racialized group was no longer significant, suggesting that antisocial peers explains part of that relationship. Adjusting for antisocial attitudes, behaviors, and peers simultaneously (Table 4.3, Model 5) reduced the effect of racialized group by approximately 11.5%, and the coefficient for racialized group was no longer significant, which suggests that these criminogenic factors partly mediate the relationship between racialized group and convictions.

Table 4 shows results from models that sequentially tested whether racialized group continues to have a direct effect on convictions after adjusting for M-Y confounders. Table 4 shows that individual, family, and contextual characteristics do influence the effect of racialized group membership on convictions through antisocial attitudes, behaviors, and peers. Adding M-Y confounders to the model reduced the effect of racialized group membership on convictions through antisocial attitudes (Table 4, Model 1) by 11.1%. The rate ratio for racialized group membership dropped from 1.43 to 1.27 (95% CI: 0.88 – 1.84). The effect of racialized group through antisocial behaviors (Table 4, Model 2) dropped by 20%. The rate ratio fell from 1.57 to 1.26 (95% CI: 0.87 – 1.81). Table 4, Model 3 shows that M-Y confounders reduced the effect of racialized group through antisocial peers by 6.3%. The rate ratio fell from 1.27 to 1.19 (0.82 – 1.74). When M-Y confounders were added to the model that included all three antisocial mediators, the effect of racialized group membership dropped by 5.8%.

Table 4.3, Model 5, and Table 4, Model 5 show results from sensitivity models that tested whether racialized group membership has a direct effect on arrests and convictions after blocking the exposure-induced M-Y confounding pathway through individual, family, and social confounders. As noted above this pathway includes some of the mediational effect through antisocial characteristics. The rate ratio for arrests was 1.63 (95% CI: 1.21 – 2.19), and the rate ratio for convictions was 1.23 (95% CI: 0.85 – 1.80), suggesting that racialized group membership does have an effect on arrests and convictions even after accounting for the pathway through individual, family, and social factors. Comparing the first four models in Tables 3 and 4 to the respective fifth models in those tables shows that antisocial characteristics contribute minimally to any of the indirect pathways from racialized group membership to arrests and convictions.

4.5 Discussion

In a community-based sample of 503 boys followed into adulthood, antisocial attitudes and behaviors did not explain the relationship between racialized group membership and arrest and conviction rates. Only antisocial peers appreciably mediated the relationship between racialized group and arrest rates and racialized group and conviction rates; however, after blocking other pathways and confounders of the mediator-outcome relationship, such as socioeconomic status, neighborhood characteristics, parenting factors, school factors, substance use, institutionalization, and psychiatric factors, the mediational effect of antisocial peers was virtually eliminated. These findings suggest that the causes of individual variation in risk of contact with the criminal justice system may not be the same as the causes of the population distributions of risk between groups. In turn, this finding demonstrates that the high-risk prevention strategy of targeting individuals with the highest levels of the Big Four criminogenic risks will likely not reduce crime rates or mitigate mass incarceration, unless the social, contextual, and individual antecedents of those criminogenic factors are also considered.

In the most generous test of the criminogenic risk framework, in which confounders of the mediator and outcomes were left out of the model, thereby overestimating the indirect effect, affiliation with antisocial peers partially mediated the relationship between racialized group and arrest and racialized group and conviction. This emphasizes the somewhat obvious category distinction between antisocial peers and antisocial attitudes and behaviors. The latter, even if they are socially produced, ostensibly reside within individuals, but affiliation with antisocial peers requires some network of social relations. However, when a more complex causal structure was introduced, findings suggest that individual, family, and contextual characteristics (C_2 in Figure 4.4b) absorb much of the direct effect, and the indirect effect through antisocial peers, suggesting that antisocial peers plays a weak role on its pathway. This finding underscores the point that an effective high-risk strategy—even when directed at appropriate targets—often requires a fully elaborated causal model. As the directed acyclic graph in Figure 4.4b shows, simply providing an individual-level intervention to reduce affiliation with antisocial peers may not maximize effectiveness or efficiency. Interventions may be more successful if they are multi-level and connected not only to individuals' manipulable risk factors, but also to manipulable contextual risk factors operating *within* the risk distribution. That is, in Figure 4.4b, it may be more effective and efficient to intervene on C_2. For example, if low SES, poor neighborhood conditions, or low academic performance are a strong outcome of racialized group membership, a strong cause of affiliation with antisocial peers, and also have a strong effect on arrest or conviction, it would be inefficient to intervene on antisocial peers alone, without connecting such a program to more structural interventions.

The weaker direct and indirect effects on convictions versus arrests may be due to a number of factors. First, arrest is a more tangible and frequent experience than conviction: many arrest events did not result in conviction. Second, because a single arrest event can result in multiple charges, and subsequently multiple convictions, the latter may lack precision as a lived experience. Finally, racialized disparities in trial and sentencing processes may be more institutionally embedded, and thus also less perceptible, than the experience of arrest.

This study's findings, and the causal models in Figure 4.4, also clearly demonstrate that a high-risk strategy to reduce individuals' antisocial characteristics would have no effect on one of the most salient population patters of criminal justice system involvement: racialized disparities in arrest and conviction rates. This is because the results of this study strongly suggest that, comparing people of color with whites, the causes of the mean levels of risk for arrest and conviction are something other than the prevalence and incidence of antisocial characteristics in those groups. Instead, and consistent with Rose (1985), what distinguishes the two groups has little to do with the locations of Black, Latino, Asian, and white adolescents within their respective population distributions of risk; it is rather a mass influence acting on those populations as a whole. The contours of that mass influence were briefly hinted in the introduction, and reflect historical and contemporary systems of domination, exploitation, marginalization, and oppression directed at racialized groups, particularly Blacks. It is also telling that racialized disparities in arrest and conviction rates remained after controlling for antisocial characteristics and all other ascribed individual, family, and contextual markers of racialized social stratification, indicating that a richer, more relational construct such as the experience of *racism* is long overdue in research on individual risk of criminal behavior. That said, it was not the purpose of this study to explain racialized disparities in contact with the criminal justice system, but rather to demonstrate the inadequacy of the criminogenic risk framework for that task, and by extension, the misinterpretation of the framework as a population strategy to reduce crime rates or mitigate mass incarceration.

Related to the preceding point is the inappropriateness of conflating, as was granted for expository purposes in the introduction, *criminal behavior* with *contact with the criminal justice system*. In fact, the conceptual and empirical distance between these constructs may be part of the critical distance between a high-risk versus a population approach to prevention. That is, differences in criminal behavior cannot be the full explanation for differences in arrest and conviction rates if different groups of people are policed and convicted at different rates for the same crime, or if the threshold for considering some behaviors criminal is different in one group versus another. For example, changes in incarceration rates over time can be due to changes in laws, policing priorities, and standards for arrest and conviction, rather than changes in individual behavior. Indeed, it is well documented that crime rates were at historic lows, and were continuing to drop, *before* the onset of mass incarceration (Gilmore, 2007; King et al., 2005; Roeder et al., 2015). That said, managing and reducing individual risk is an important and worthy objective for criminal justice reform, as are reducing crime rates and ending mass incarceration. But ongoing disregard for structural and population approaches to the latter (e.g., Andrews & Bonta, 2010, p. 18, 34, 69-70, 93, 114-121, 306-307, 531-533; Bonta, 2007), in the name of the former, is theoretically and intellectually untenable. Worse, it may even reproduce or exacerbate existing inequalities in the criminal justice system under the guise of scientific objectivity, if structural disadvantage or discrimination pervade the social contingencies that put people at risk of criminogenic risks (Harcourt, 2007; Prins & Reich, under review).

This study should be understood in light of the following limitations. First, all participants in the Pittsburgh Youth Study are male, and so it was not possible to assess the mediational effect of antisocial characteristics on racialized group and arrest/conviction rates among girls. Nonetheless, contact with the criminal justice system and antisociality are predominantly male phenomena (Black & Blum, 2015; Carson & Golinelli, 2013; Durose, Cooper, & Snyder, 2014; Glaze & Parks, 2012; Guerino et al., 2011; Langan & Levin, 2002; Maruschak & Parks, 2012; Minton, 2011). Second, all participants were selected from Pittsburgh public schools, which potentially limits the generalizability of the findings to other areas if there were any secular trends regarding criminal justice policy or antisociality. Third, while the study's measures of the Big Four criminogenic risk factors are consistent with the constructs that underlie the criminogenic risk framework, direct tests of a particular risk assessment instrument such as the Level of

Services Inventory was not possible. However, it is difficult to imagine measures with greater convergent validity. Fourth, data on arrests and convictions were not linked, so it was not possible to truly follow participants through the criminal justice process. Finally, as in any mediational analysis, measurement error in the mediators could favor the direct effect over an indirect effect. In this instance, any error in the measurement of antisocial characteristics might underestimate their mediational effect.

4.6 Conclusion

This study shows that it is a mistake to interpret evidence for recidivism prediction as evidence about the causes of crime rates, or the incidence of individuals' contact with the criminal justice system. Findings demonstrate that the logic of this conceptual slippage is incorrect, by showing that the Big Four criminogenic risk factors do not explain group differences in arrest and conviction rates. This was seen in models where antisocial attitudes, behaviors, and peers did not appreciably mediate the relationship between racialized group status and arrest and conviction rates, after blocking other pathways and controlling for mediator-outcome confounders. Future research should more clearly and fully elaborate the causal models of the relationship between antisocial characteristics and arrest and conviction, in order to maximize the efficiency of high-risk prevention strategies. Researchers and policymakers should more cautiously communicate the scope of reform that the criminogenic risk framework can deliver, and be more open to population approaches to prevention that account for structural and contextual influences on the risk distributions of crime and contact with the criminal justice system.

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4.8 Figures and tables

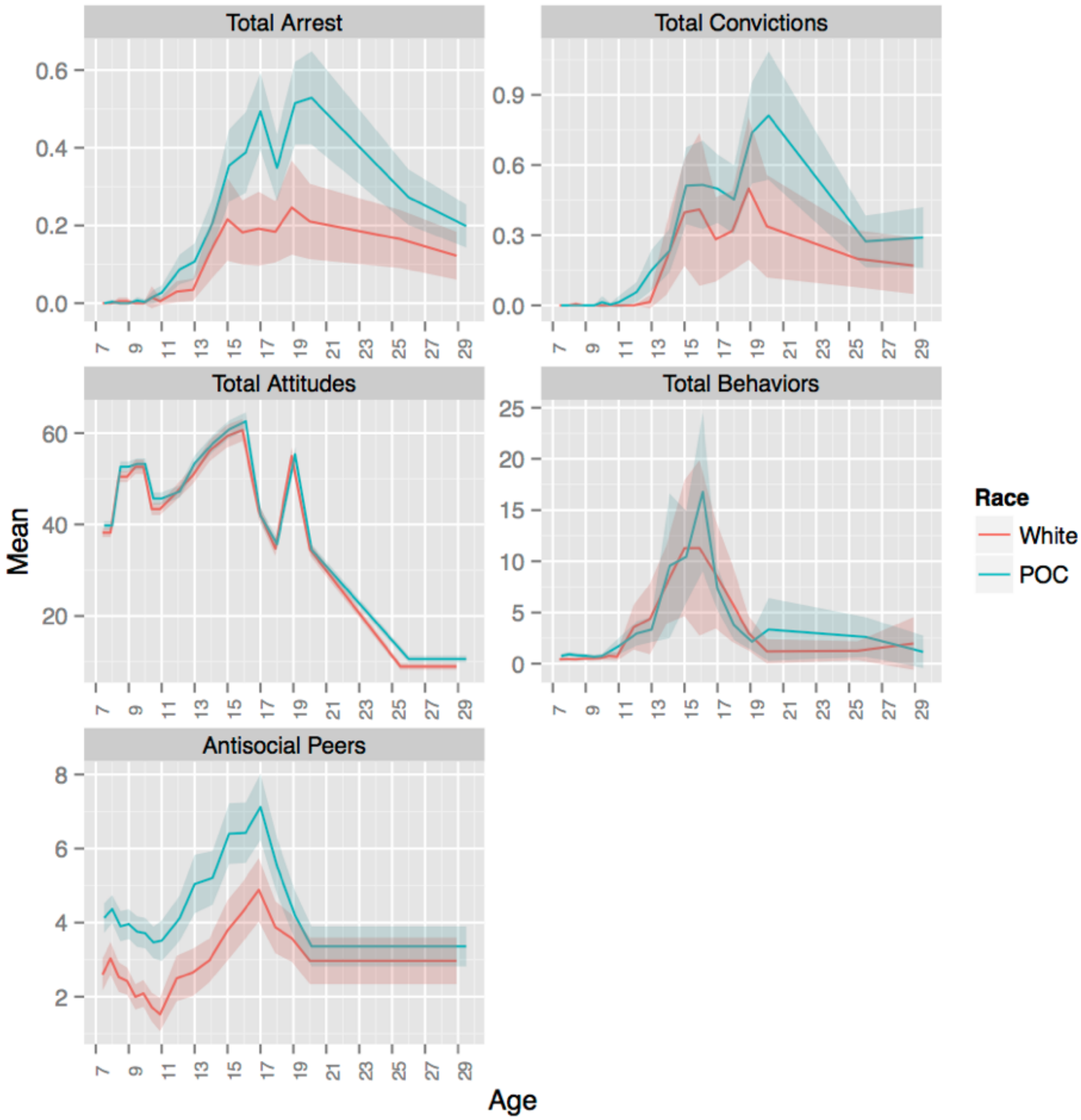

Figure 4.1. Means and 95% confidence intervals for outcome and mediator measures, by race. POC: people of color.

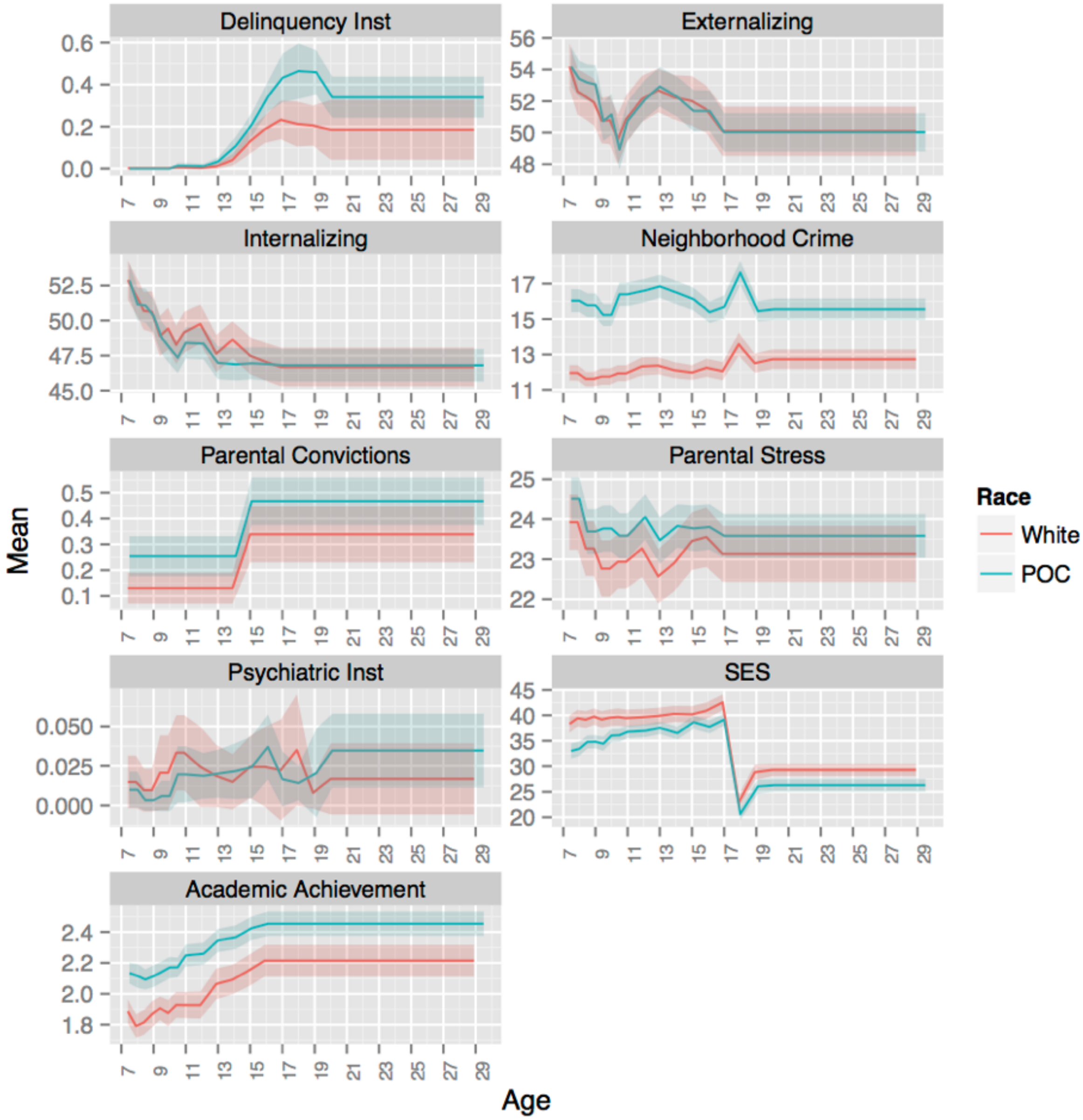

Figure 4.2. Means and 95% confidence intervals for continuous extraneous mediators, by race. POC: people of color. Inst: institutionalization. SES: socioeconomic status.

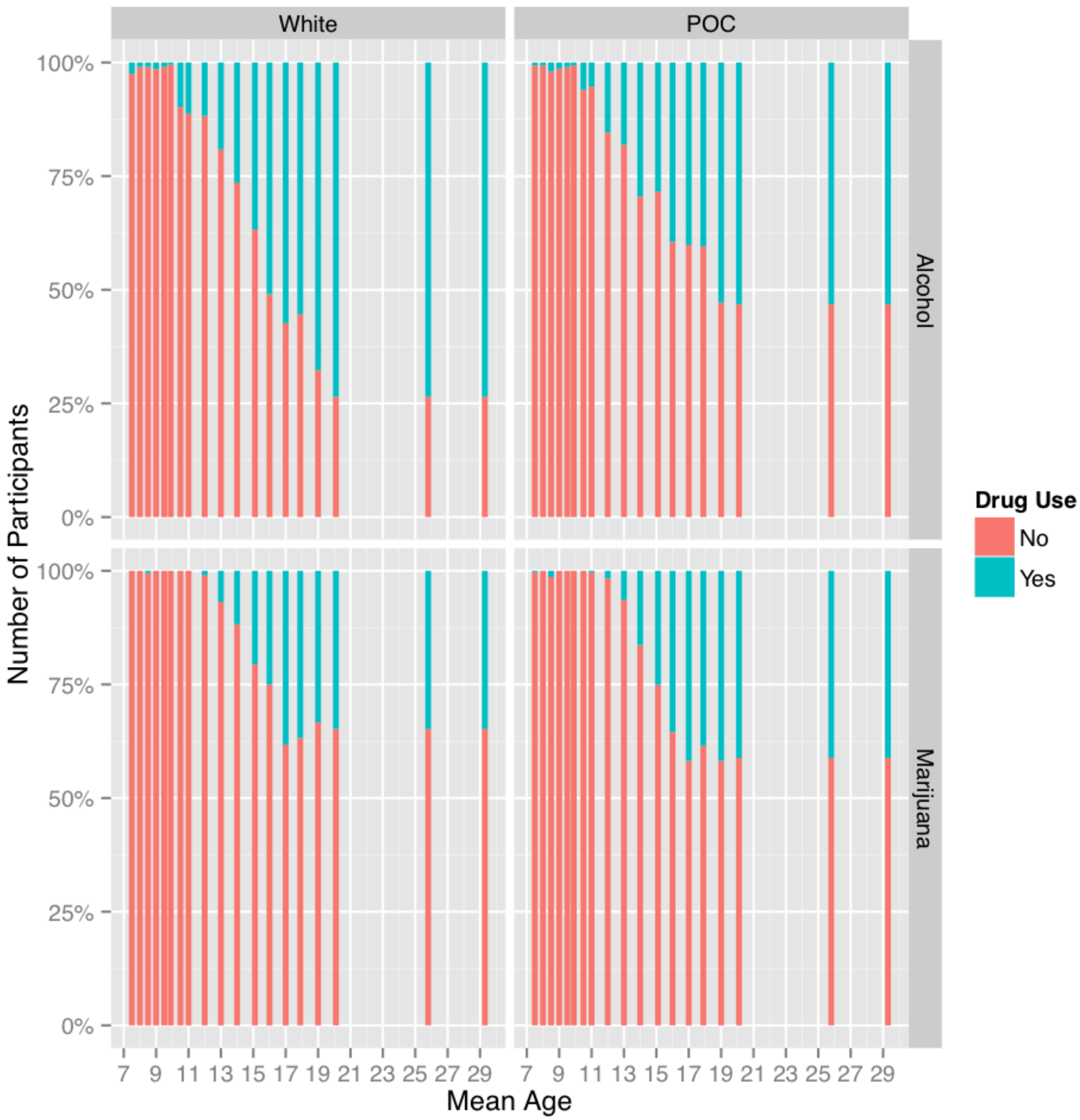

Figure 4.3. Relative proportions of dichotomous extraneous mediators, by race. POC: people of color

Figure 4.4. Causal structures, represented by directed acyclic graphs, illustrating the confounders (C1 and C2) that must be controlled to estimate unbiased controlled direct effects. Panel *a* represents a simple causal structure with no exposure-induced confounding of the mediator–outcome relationship. Panel *b* represents the more realistic causal structure with exposure-induced confounding, in which racialized group membership affects factors (C_2) that accordingly influence antisocial characteristics and arrest/conviction, e.g., socioeconomic status.

Table 4.1. Arrest rate ratios for people of color relative to whites, crude and adjusted for mediating effects of antisocial characteristics

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
	RR (95% CI)	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*
Age	1.09 (1.09, 1.10)	1.08 (1.07, 1.09)	1.08 (1.07, 1.09)	1.08 (1.08, 1.09)	1.08 (1.07, 1.08)
People of color vs. whites	1.99 (1.46, 2.71)	1.84 (1.36, 2.49)	1.97 (1.46, 2.66)	1.70 (1.24, 2.33)	1.85 (1.37, 2.48)
Antisocial attitudes		1.07 (1.06, 1.09)			1.07 (1.05, 1.09)
Antisocial behaviors			1.03 (1.02, 1.03)		1.01 (1.00, 1.02)
Antisocial peers				1.10 (1.06, 1.14)	1.00 (0.96, 1.04)
Constant	0.02 (0.02, 0.03)	0.001 (0.00, 0.00)	0.03 (0.02, 0.04)	0.02 (0.01, 0.03)	0.001 (0.00, 0.00)

Table 4.2. Arrest rate ratios for people of color relative to whites, crude and adjusted for mediating effects of antisocial characteristics and mediator-outcome confounders

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
	RR (95% CI)	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*
Age	1.04 (1.03, 1.06)	1.04 (1.02, 1.05)	1.04 (1.03, 1.05)	1.04 (1.03, 1.05)	1.04 (1.03, 1.05)
Race/ethnicity	**1.66 (1.25, 2.22)**	**1.66 (1.24, 2.22)**	**1.58 (1.17, 2.13)**	**1.74 (1.31, 2.32)**	**1.63 (1.21, 2.19)**
Antisocial attitudes	**1.05 (1.03, 1.06)**			**1.05 (1.03, 1.06)**	
Antisocial behaviors		**1.01 (1.00, 1.02)**		**1.01 (1.00, 1.02)**	
Antisocial peers			**1.02 (0.98, 1.07)**	**0.97 (0.93, 1.01)**	
Marijuana use	1.62 (1.31, 1.99)	1.76 (1.40, 2.21)	1.79 (1.43, 2.24)	1.62 (1.30, 2.01)	1.83 (1.47, 2.29)
Alcohol use	1.07 (0.87, 1.31)	1.13 (0.90, 1.41)	1.14 (0.92, 1.42)	1.06 (0.87, 1.31)	1.15 (0.92, 1.44)
Academic achievement	1.42 (1.23, 1.64)	1.50 (1.30, 1.74)	1.50 (1.30, 1.74)	1.42 (1.23, 1.64)	1.51 (1.30, 1.75)
Internalizing	0.98 (0.98, 0.99)	0.98 (0.97, 0.99)	0.98 (0.97, 0.99)	0.98 (0.97, 0.99)	0.98 (0.97, 0.99)
Externalizing	1.02 (1.01, 1.03)	1.02 (1.01, 1.03)	1.02 (1.01, 1.03)	1.02 (1.01, 1.03)	1.02 (1.01, 1.03)
Neighborhood crime	1.00 (0.98, 1.02)	1.00 (0.98, 1.02)	1.00 (0.99, 1.02)	1.00 (0.98, 1.02)	1.00 (0.99, 1.02)
Parental convictions	1.29 (1.16, 1.44)	1.30 (1.16, 1.45)	1.28 (1.15, 1.43)	1.31 (1.17, 1.46)	1.28 (1.15, 1.43)
Parental stress	1.00 (0.98, 1.02)	1.00 (0.98, 1.01)	1.00 (0.98, 1.01)	1.00 (0.98, 1.02)	1.00 (0.98, 1.01)
SES	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)
Parental supervision	1.11 (1.04, 1.17)	1.13 (1.07, 1.20)	1.14 (1.07, 1.21)	1.11 (1.04, 1.17)	1.14 (1.08, 1.21)
Psychiatric Inst	1.33 (0.99, 1.78)	1.34 (0.99, 1.80)	1.30 (0.96, 1.75)	1.35 (1.01, 1.81)	1.30 (0.96, 1.75)
Delinquency Inst	0.99 (0.88, 1.11)	1.01 (0.90, 1.13)	1.02 (0.90, 1.14)	0.99 (0.88, 1.11)	1.03 (0.92, 1.15)
Constant	0.001 (0.00, 0.00)	0.004 (0.00, 0.01)	0.003 (0.00, 0.01)	0.001 (0.00, 0.00)	0.003 (0.00, 0.01)

Table 4.3. Conviction rate ratios for people of color relative to whites, crude and adjusted for mediating effects of antisocial characteristics

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
	RR (95% CI)	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*
Age	1.10 (1.09, 1.11)	1.08 (1.07, 1.09)	1.08 (1.07, 1.09)	1.09 (1.08, 1.10)	1.08 (1.07, 1.09)
Race/ethnicity	1.57 (1.06, 2.33)	1.43 (0.97, 2.11)	1.57 (1.07, 2.30)	1.27 (0.86, 1.89)	1.39 (0.95, 2.02)
Antisocial attitudes		1.08 (1.06, 1.10)			1.07 (1.05, 1.10)
Antisocial behaviors			1.03 (1.01, 1.04)		1.01 (0.99, 1.03)
Antisocial peers				1.13 (1.08, 1.18)	1.03 (0.97, 1.09)
Constant	0.03 (0.02, 0.05)	0.001 (0.00, 0.00)	0.04 (0.03, 0.06)	0.03 (0.02, 0.04)	0.001 (0.00, 0.00)

Table 4.4. Convictions rate ratios for people of color relative to whites, crude and adjusted for mediating effects of antisocial characteristics and mediator-outcome confounders

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
	RR (95% CI)	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*	*RR (95% CI)*
Age	1.03 (1.01, 1.05)	1.02 (1.01, 1.04)	1.02 (1.01, 1.04)	1.03 (1.01, 1.04)	1.02 (1.01, 1.04)
Race/ethnicity	**1.27 (0.87, 1.84)**	**1.25 (0.87, 1.81)**	**1.19 (0.82, 1.73)**	**1.31 (0.92, 1.87)**	**1.23 (0.85, 1.80)**
Antisocial attitudes	**1.04 (1.02, 1.07)**			**1.05 (1.02, 1.07)**	
Antisocial behaviors		**1.01 (0.99, 1.02)**		**1.00 (0.98, 1.02)**	
Antisocial peers			**1.02 (0.97, 1.08)**	**0.98 (0.92, 1.04)**	
Marijuana use	1.81 (1.35, 2.42)	1.99 (1.43, 2.76)	1.98 (1.45, 2.71)	1.82 (1.35, 2.46)	2.04 (1.48, 2.80)
Alcohol use	1.17 (0.86, 1.59)	1.25 (0.91, 1.73)	1.26 (0.92, 1.72)	1.17 (0.86, 1.59)	1.27 (0.92, 1.75)
Academic achievement	1.68 (1.36, 2.08)	1.78 (1.45, 2.20)	1.78 (1.44, 2.21)	1.68 (1.36, 2.08)	1.79 (1.45, 2.21)
Internalizing	0.98 (0.97, 0.99)	0.98 (0.97, 0.99)	0.98 (0.97, 0.99)	0.98 (0.97, 1.00)	0.98 (0.97, 0.99)
Externalizing	1.02 (1.00, 1.04)	1.02 (1.00, 1.04)	1.02 (1.00, 1.04)	1.02 (1.00, 1.04)	1.02 (1.00, 1.04)
Neighborhood crime	1.00 (0.98, 1.02)	1.00 (0.98, 1.02)	1.00 (0.98, 1.02)	1.00 (0.98, 1.02)	1.00 (0.98, 1.02)
Parental convictions	1.39 (1.21, 1.59)	1.39 (1.21, 1.61)	1.38 (1.19, 1.60)	1.40 (1.22, 1.61)	1.39 (1.21, 1.60)
Parental stress	1.00 (0.97, 1.02)	0.99 (0.97, 1.02)	0.99 (0.97, 1.02)	1.00 (0.97, 1.02)	0.99 (0.97, 1.02)
SES	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)	1.00 (0.99, 1.01)
Parental supervision	1.10 (1.02, 1.19)	1.13 (1.05, 1.22)	1.13 (1.05, 1.22)	1.11 (1.02, 1.19)	1.14 (1.06, 1.23)
Psychiatric Inst	1.67 (1.15, 2.41)	1.65 (1.12, 2.44)	1.63 (1.13, 2.37)	1.67 (1.14, 2.44)	1.62 (1.12, 2.36)
Delinquency Inst	1.06 (0.95, 1.19)	1.09 (0.97, 1.22)	1.09 (0.96, 1.23)	1.06 (0.95, 1.19)	1.10 (0.98, 1.23)
Constant	0.001 (0.00, 0.00)	0.005 (0.00, 0.01)	0.004 (0.00, 0.01)	0.001 (0.00, 0.00)	0.004 (0.00, 0.01)

4.9 Appendix

Table 4.5. Variables, constructs, instruments, and descriptive statistics for all analysis measures

Variable	Construct	Instruments	Reliability α	Mean	SD	Min	Max
Total Antisocial Attitudes	Attitudes toward delinquent behavior, perceptions of problem behavior, perceptions of likelihood of getting caught	Attitude to Delinquent Behavior Scale, Likelihood of Getting Caught Scale	.77 - .83	44.4	18.5	0.0	140.0
Total antisocial behaviors	Very minor, minor, moderate, serious	Self-Reported Delinquency Scale, Self-Reported Antisocial Behavior Scale	0.77 - 0.92	3.9	24.8	0.0	1002.0
Peer delinquency	Proportion of youth's peers who engaged in activities described above under "delinquent behaviors"	Self-Reported Delinquency Scale, Substance Use Scale	0.79 – 0.96	3.9	5.1	0.0	40.0
Total Arrests	Frequency of total arrests	Official records	NA	0.1	0.6	0.0	7.0
Total Convictions	Frequency of total convictions	Official records	NA	0.2	1.1	0.0	29.0
Internalizing t-score	Psychiatric symptoms, disorders	16 items based on National Youth Survey, lay interviews based on the DSM-III-R	Depression: 0.87	48.3	10.4	24.8	88.0
Externalizing t-score	Psychiatric symptoms, disorders	Revised Diagnostic Interview Schedule for Children	Inattention: 0.87 Hyperactivity: 0.87	51.3	11.0	29.2	90.0
Psychiatric institutionalization	Periods of psychiatric institutionalization	Official records	NA	0.0	0.2	0.0	4.0
Delinquency institutionalization	Periods of correctional institutionalization	Official records	NA	0.1	0.6	0.0	12.0
Academic performance	Achievement in reading, writing, math, spelling, and up to three other academic subjects	Caretakers' teachers' and youths' evaluations	0.46 – 0.56	2.2	0.7	0.5	4.3
Parental stress	Caretaker perceptions of stress in past month	Perceived Stress Scale	0.57 – 0.85	23.5	5.0	10.4	42.0
Parental supervision	Youth (and sometimes parents') perceptions of parental discipline, supervision.	Supervision/Involvement Scale		5.9	1.4	2.8	12.0

Parental criminal history	Frequency of parental arrests, charges, and convictions	Official records	NA	0.3	0.7	0.0	4.0
Neighborhood	Presence of prostitution, assaults, burglaries, etc.	The Neighborhood Sale	0.95	14.5	5.3	1.8	31.2
Socioeconomic status	Race, ethnicity, work, marital status, education of caretakers	The Demographics Questionnaire	NA	34.6	12.7	0.0	67.2
Alcohol use	Used alcohol in past assessment interval (Yes/No)	Substance use scale adapted from National Youth Survey	NA	% No: 73.9 % Yes: 26.1			
Marijuana use	Used marijuana in past assessment interval (Yes/No)	Substance use scale adapted from National Youth Survey	NA	% No: 83.7 % Yes: 16.3			

Table 4.6. Tests for exposure-mediator interactions

	Arrests			**Convictions**		
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
	β (95% CI)	*β (95% CI)*	*β (95% CI)*	*β (95% CI)*	*β (95% CI)*	*β (95% CI)*
Age	0.08 (0.07, 0.08)	0.08 (0.07, 0.08)	0.08 (0.07, 0.09)	0.08 (0.07, 0.09)	0.08 (0.07, 0.09)	0.08 (0.07, 0.09)
POC	1.14 (-0.46, 2.74)	0.79 (0.48, 1.11)	0.68 (0.18, 1.19)	1.03 (-1.14, 3.21)	0.57 (0.16, 0.98)	0.45 (-0.23, 1.13)
Antisocial attitudes	0.08 (0.05, 0.10)			0.09 (0.05, 0.12)		
POC*Antisocial attitudes	-0.01 (-0.04, 0.02)			-0.01 (-0.05, 0.03)		
Antisocial behaviors		0.04 (0.02, 0.05)			0.04 (0.02, 0.06)	
POC*Antisocial behaviors		-0.02 (-0.03, -0.002)			-0.02 (-0.04, 0.001)	
Antisocial peers			0.12 (0.02, 0.22)			0.15 (0.05, 0.26)
POC*Antisocial peers			-0.04 (-0.14, 0.07)			-0.05 (-0.16, 0.07)
Intercept	-7.37 (-8.77, -5.97)	-3.74 (-4.06, -3.42)	-4.02 (-4.48, -3.56)	-7.36 (-9.26, -5.46)	-3.34 (-3.76, -2.92)	-3.70 (-4.32, -3.09)

Chapter 5

Conclusions

> "…it does appear that the antiprediction and antipsychological elements within mainstream criminology and justice are dampening. They are dampening because of the volume and depth of the evidence regarding individual differences and the empirical clarity of the difference between major and minor risk factors…."
>
> – Andrews & Dowden, 2006, p. 89

> "Additional research, in our view, is not likely to change the direction or ordering of the results of the predictor domains to any marked degree...."
>
> – Gendreau, Little, & Goggin, 1996, p. 588

> "…technocratic rationalization tends to insulate institutions from the messy, hard-to-control demands of the social world. By limiting their exposure to indicators that they can control, managers ensure that their problems will have solutions."
>
> – Feeley & Simon, 1992, p. 456

> "So labelling theory is dead: long live labelling theory."
>
> – Plummer, 2011 p. 90

The criminogenic risk assessment framework is a widely accepted best practice that began in recidivism prediction and, given its perceived success, is expanding across the criminal justice system. The explanatory theory that emerged around the framework has also become influential among applied criminal justice researchers and policymakers. As the framework expands from the back-end of the criminal justice system to the front, it has the capacity to redefine the logic of the criminal justice system itself—a shift that has been observed and critiqued by criminologists and criminal justice scholars since its earliest manifestations (Cohen, 1985; Feeley & Simon, 1992; Hannah-Moffat, 1999). The purpose of this dissertation was to test whether the assumptions of criminogenic risk assessment—taken on the framework's own terms—warrant its generalization, in theory and practice, beyond risk assessment for recidivism. The assumptions were that: 1) the evidence base for the predictive performance of criminogenic risk assessment is being interpreted correctly and appropriately, 2) the best causal models of recidivism are also the best causal models of the onset and duration of criminal behavior (and by extension, that interventions successful at reducing recidivism will be successful at reducing the onset, duration, and rate of criminal behavior); and 3) the causes of inter-individual variation in criminal behavior are the same as causes of the population distribution, or incidence rate, of crime. A premise of the second assumption was that the criminal justice system itself has no effect on criminogenic risk.

In order to grant the framework's expansion, the criminogenic risk framework needed to demonstrate that 1) its predictive performance is strong and being correctly interpreted; 2) risk factors for recidivism are not appreciably influenced by contact with the criminal justice system itself; and 3) criminogenic risk factors explain group differences in criminal justice system contact. A goal of the dissertation was to identify potential gaps in theory and evidence that may result from the framework being imprisoned by the proximate (McMichael, 1999)—from its failure to connect individual-level risk factors to their wider causal context.

But before summarizing this dissertation's findings, it is important to understand that this project had a number of limitations. First, despite using data that provided a unique opportunity to test the assumptions of the criminogenic risk framework, measures of criminogenic risk were derived from aggregate antisocial attitude and behavior scales—the author did not have access to individual scale items. This may have introduced measurement error within these constructs and biased results, likely toward the null. The author was also not able to test a particular criminogenic risk assessment instrument directly, such as the Level of Services Inventory. However, this dissertation's measures of antisocial attitudes and behaviors likely had greater breadth and depth than common risk assessment instruments, giving the constructs that underlie the criminogenic risk framework the best chance of demonstrating whatever effects they might demonstrate. With regard to outcome measures, while official criminal justice records were ideal, the author did not have the ability to link particular events to others, e.g., certain arrests to certain convictions. Such information would have allowed for a more precise exploration of individual trajectories through the criminal justice system. Furthermore, the sample was too small to disaggregate findings by type of offense. Finally, while the ability to establish temporal order was a strength of this dissertation's analyses, the temporal resolution of certain variables was such that only broad inferences can be made about the relationship between antisocial characteristics and criminal justice system involvement over time.

Despite these limitations, this dissertation provided evidence that challenges or destabilizes all three assumptions necessary for the expansion of the criminogenic risk framework. The meta-review assessed the first assumption, that criminogenic risk assessment's predictive performance is strong and being correctly interpreted. It found that predictive performance is weak to modest, but that the field tends to exaggerate its strength and inappropriately interpret the implications of its findings in ways that are not supported by the data.

Another troubling trend that became apparent in the meta-review is the criminogenic risk framework's dismissal of non-psychological theories of crime. The dismissal is easy to understand: other theories have not offered much for actual corrections practice, i.e., for improving the operations of corrections systems or managing or targeting resources at correctional populations. But it also reflects a failure (reciprocated by the other camps as well) to recognize that these different perspectives are actually addressing different questions. There seem to be two sets of conflations that are the source of this failure. The first is the conflation of the causes of *crime rates* and the causes of individual *criminal behaviors.* The second is the conflation of *criminality* (locating deviance within individuals) and *criminalization* (being subjected to the activities of the criminal justice system). Although this dissertation did not set out to tackle these conflations directly, both empirical studies jostled them into relief.

The first empirical study challenged a premise of the second assumption, that risk factors for recidivism are not appreciably influenced by contact with the criminal justice system itself. It found that exposure to the criminal justice system increases antisocial attitudes, behaviors, and affiliation with antisocial peers. Each arrest, and to a lesser extent conviction, an individual experienced over time increased their subsequent antisocial characteristics. Data were analyzed with methods that minimized confounding from self-selection into criminal behavior and other potentially criminogenic individual, school, family, and neighborhood factors. These findings challenge the empirical basis for expanding criminogenic risk assessment from the back end of the criminal justice system to the front, and the notion that doing so might reduce criminal behavior and correctional supervision rates overall. The theoretical issues that this study raises strike at the framework's core conceptualizations of risk, crime, criminal behavior, and recidivism.

The findings of the first empirical paper are consistent with folk wisdom: the idea that prisons are "schools for crime" is part of the popular imagination. But the finding also supports various strains in criminological and sociological theory that have since been supplanted by cognitive-behavioral perspectives. One of these is labeling theory. Narrowly, labeling theory proposed that crime is heightened, or even caused by, the label "criminal" once it is applied to people who have engaged in deviant behavior (Plummer, 2011). Broadly, labeling theory claimed that criminology was too concerned with criminals as types of people rather than on systems of social control and the processes and consequences of the criminal label (Plummer, 1979, 2011). Yet, the theory has long been criticized. A major criticism is that it failed to account for the initial motivations for criminal behavior, focusing instead on the process of "secondary deviance" that arises after a label has been applied (Plummer, 1979). Another criticism is that the theory does not pan out empirically, although some have argued that claims of the theory's empirical demise were exaggerated (Petrunik, 1980).

Perhaps short of reinvigorating the labeling theory of crime, the finding that arrests and convictions increase antisocial characteristics is nevertheless broadly consistent with Link's modified labelling perspective. According to Link and colleagues (Link, Cullen, Struening, Shrout, & Dohrenwend, 1989), labeling a person as criminal would not directly create subsequent criminality, but rather may cause negative material and psychosocial conditions that prolong or reproduce criminal behavior. Link's modified labeling theory also foreshadows Reich's (2010) notion that individuals who come into contact with the criminal justice system may adopt a strategic or negotiated orientation toward criminal behavior—that behavior does not necessarily reflect inherent psychopathology. Both Link and Reich's insights remind us that individuals who engage in criminal behavior or come into contact with the criminal justice system are not passive recipients of psychological or social categories, but are navigating and adapting to structural and institutional circumstances that have material consequences.

The second empirical study challenged the third assumption, or the notion that the causes of inter-individual variation in criminal behavior are the same as causes of the population distribution, or incidence rate, of crime. The study took advantage of known racialized group differences in contact with the criminal justice system, and a known absence of racialized group differences in certain antisocial characteristics, to show that antisocial attitudes and behaviors could not explain the relationship between racialized group membership and disparities in arrest and conviction rates. Only affiliation with antisocial peers was a partial mediator; however, after blocking other pathways and confounders of the mediator-outcome relationship, such as socioeconomic status, neighborhood characteristics, parenting factors, school factors, substance use, and psychiatric factors, the mediational effect of antisocial peers was virtually eliminated.

These findings underscore Rose's insights that the causes of an individual's place in a risk distribution are the not the same as the causes of the distribution's mean (Rose, 1985), and suggest that it is a mistake to interpret evidence for recidivism reduction as evidence about the causes of crime or incarceration rates. In turn, these findings demonstrate that the high-risk prevention strategy of targeting individuals with the highest levels of the Big Four criminogenic risks will likely not mitigate mass incarceration. Even when directed at appropriate targets, a high-risk prevention strategy requires a fully elaborated causal model of criminogenic risk factors, both to distinguish a high-risk approach to prevention from a population approach, and also to maximize the efficiency of existing high-risk strategies.

The findings of the second study emphasize that the conflation of criminal behavior with exposure to the criminal justice system, or of *criminality* and *criminalization*, is both conceptually and empirically unsupportable. When different groups of people are policed, prosecuted, convicted, sentenced, incarcerated, released, and recidivated at different rates for the same behaviors, or if the threshold for considering some behaviors criminal is different in one group versus another, locating the predictive and explanatory action inside of individuals is intellectually disingenuous. Moreover, calibrating criminogenic risk scales to account for their ostensible "underperformance" with certain groups is politically inattentive. For example, with Canadian Aboriginals, a case can be made *against* adding culturally specific individual-level risk factors to risk assessment instruments, or adjusting scoring procedures so that low-risk Aboriginals would be expected to recidivate at the rate of moderate- to high-risk whites. While this may make criminogenic risk assessments "perform better" in a predictive sense, such recalibration may also serve to mask, or reproduce, the structural and institutional discrimination that caused the instrument's underperformance in the first place, under a guise of scientific objectivity.

Managing and reducing individual risk is an important and worthy objective for criminal justice reform, as are reducing crime rates and ending mass incarceration. Yet in order to succeed, we must not provide policymakers and other agents of social change with the right answers to the wrong questions (Schwartz & Carpenter, 1999). The finding that exposure to the criminal justice system can increase some of the characteristics thought to predict criminal behavior underscores the importance of clarifying what criminogenic risk assessment was designed to do, and being careful to remain specific and precise about the causes of recidivism versus the causes of onset and duration of criminal behavior and the origins of crime. The finding that individual differences in propensity for criminal behavior likely do not explain differences in crime rates underscores the importance of clarifying what we can expect from intervening on individuals versus intervening on systems. A such, results caution against the wholesale expansion of criminogenic risk assessment from the back end of the criminal justice system to the front—from community corrections to policing, pretrial decision-making, and sentencing.

The time has long since passed that the criminogenic risk framework can continue to neglect the structural and institutional antecedents of individual-level risk of criminal behavior. In this, criminal justice scholarship can take heed of epidemiology's debates over high-risk versus population approaches to prevention. It can learn by analogy from epidemiology's conflicts over prioritizing interventions on "health behaviors" versus interventions on the social conditions that constrain those behaviors. And it can learn from the discipline's struggle to move beyond merely talking about a "cells to society" approach to population health to actually implementing it in research and practice. Both disciplines have, at times, taken up residence in the prison of the proximate. But for both disciplines, the gates were never closed: we can always walk back out.

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