

## **ABSTRACT**

ZOTTOLA, SAMANTHA ALLEN. Efforts to Improve Evidence-Based Practices from Jail Booking to First Appearance in Court (Under the Direction of Dr. Sarah Desmarais).

There is currently a great deal of national attention focused on jail reform in general and pretrial reform specifically. This attention has been an impetus for private, non-profit, and government agencies alike to invest time and resources in reform efforts. This momentum is bringing about much needed change to the practices in place from jail booking to first appearance in court. In this time period, dozens of decisions are made that have major implications for the trajectory a person will follow through the rest of their criminal justice contact. Many of these decisions are made with little guidance or transparency. Reform of these practices is needed but some reform efforts have outpaced research and are moving forward without a strong evidence-base. The goal of the manuscripts in this dissertation is to help build the evidence-base for practices that can be implemented to increase guidance and promote transparency for the decisions made from booking to first appearance

The first manuscript (Chapter 2) examines changes in responses to the Brief Jail Mental Health Screen across repeated bookings in a sample of 12,531 people. The goal of this study was to examine how the Brief Jail Mental Health Screen functions as a repeated assessment. Findings demonstrate that responses do indeed change over repeated bookings. Mechanisms for this change must be examined further in future work.

The second manuscript (Chapter 3) fills a gap in the literature about bias in bail assignments. This study examines interactions between legally relevant variable (i.e., charge count, charge severity, and booking number) and social categories (i.e., race, gender, and age). Further, this study uses a longitudinal design and a sample of 24,060 people charged with both felony and misdemeanor offenses. Findings reveal bias in the way bail is assigned particularly

among young, Black men. The results from this study emphasize the need to consider the impact of multiple social categories simultaneously when examining bail assignments.

The third manuscript (Chapter 4) contributes to the discussion around the use of pretrial risk assessment instruments with a systematic review of the literature on these instrument's predictive validity. Specifically, pretrial risk assessment instruments predict a person's likelihood of failing to appear in court or engaging in any new crime during the pretrial period.

Additionally, some instruments predict a person's likelihood of engaging in new violent crime during the pretrial period. This review examines predictive validity overall and across subgroups (i.e., race and gender) for six assessment instruments across 11 studies. Findings demonstrated good to excellent predictive validity and there was little evidence of bias across subgroups.

However, only four studies included analyses examining predictive validity across subgroups.

Overall, predictive validity across subgroups was generally comparable but more research is needed.

Taken together, the research in this dissertation supports the needs for reform and has promising findings regarding the use of screening and assessment instruments in those reform efforts. Findings also highlight several; important questions that must be addressed in future research. Overall, the studies in this dissertation contribute to the evidence-base for rapidly advancing jail and pretrial reform efforts.

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Efforts to Improve Evidence-Based Practices from Jail Booking to First Appearance in Court

by  
Samantha Allen Zottola

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APPROVED BY:

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Dr. Sarah Desmarais  
Committee Chair

---

Dr. Shevaun D. Neupert

---

Dr. Elan Hope

---

Dr. William Smith

## BIOGRAPHY

Samantha Allen Zottola was born to Rick and Autumn Scott and raised together with her two siblings in Cleveland, Ohio. She attended Lipscomb University in Nashville, Tennessee, graduating *magna cum laude* in 2013 with a Bachelor of Science in Psychology. Beginning in the fall of 2013, Samantha attended Cleveland State University where she graduated with a Master's in Psychology in 2015. Following graduation, Samantha spent two years working as a Psychometrist for the Cuyahoga County Juvenile Court. In 2017, she joined the Applied Social and Community Psychology program at North Carolina State University in Raleigh, North Carolina under the advisement of Dr. Sarah Desmarais. Samantha's work as focused broadly on improving jail practices, specifically in relation to risk assessment and the pretrial period. Samantha lives with her husband, Mike, and her dog, Jarvis.

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## CHAPTER 1

### Introduction

Mass incarceration in the United States has reached epic proportions. On any given day, 2.3 million people are detained in various facilities (e.g., state and federal prisons, local jails, juvenile correctional facilities, etc.) across the nation. Over half (56%) of the nation's detained population are held in state prison, about 27% are held in local jails, and the final 17% are held in a combination of federal facilities, territorial prisons, juvenile facilities, and other detention facilities (Sawyer & Wagner, 2020). The daily snapshot of incarceration rates across facilities gives the impression that state prisons see far more people than local jails. However, this static view misses the vast movement in and out of these correctional facilities. Each year, prisons receive around 600,000 new bookings, all representing a unique person (Bronson & Carson, 2019), while jails receive 10.7 *million* new bookings (Zeng, 2020), representing about 4.9 million unique people (Sawyer & Wagner, 2020). Additionally, some sources indicate that jail populations are decreasing (Zeng, 2020) while others indicate that they are increasing (Kang-Brown et al., 2019). In fact, one report found that the jail population recorded in 2019 is the highest it has been since 2009, suggesting that the country's jail population may be on an upswing (Kang-Brown et al., 2019).

The recent increase in jail populations coupled with the fact that far more people interact with jails than any other correctional facility, emphasizes an urgent need to focus research efforts on jail practices. Yet, jails have been the focus of relatively little research (Copp & Bales, 2018), especially compared to prisons (Kim et al., 2018). This is a problem because jails serve a very different purpose than prisons and therefore employ different practices. Specifically, jails are primarily used to hold people for short periods of time while they await trial, serve short

sentences (less than one year), or await transfer to a more permanent correctional facility. People's experience of jail is often characterized by uncertainty regarding the status of their future, limited access to services, and abrupt release with no reentry assistance (AbuDagga et al., 2016; Miller & Miller, 2010; Subramanian et al., 2015). The unique role of jails in the criminal justice process and the unique experiences of people detained in jails, necessitates research that is specifically focused on jails to develop evidence-based practices.

While all jail stays share the characteristic of being relatively short, there are many different reasons people are held in jail. Some people held in jail have already been sentenced. This includes the people who have been charged with a crime and are awaiting transfer to a prison to serve their sentence or who are serving a short sentence in jail. It also includes people who were released on probation or parole but who have violated conditions of their release and are awaiting a disciplinary hearing. However, the majority of people held in jails are in the pretrial phase of their criminal justice contact (Zeng, 2020). That is, they have been arrested for a crime in their local jurisdiction and are awaiting trial. For this group, booking into jail represents the first step of formal case processing. Booking is followed closely by first appearance, the court hearing at which a judge or magistrate decides whether to detain or release a person. The time period from booking to first appearance typically lasts hours to a few days at most (Bail Release Work Group, 2016) and people usually do not have legal counsel to represent or aid them and in this time (Worden et al., 2019). As a result, dozens of critical decisions are made without anyone advocating on behalf of the justice-involved person.

Some of the very first decisions about a person are made at booking by jail intake staff and, if available, jail medical or mental health personnel. These include decisions about whether someone should receive a full medical or mental health evaluation, what kind of medications, if

any, they should be given, and whether they need immediate medical or mental health care (Kubiak et al., 2020; McCoy, 2007; Subramanian et al., 2015; Wallenstein, 2014). At first appearance, a judge or magistrate makes decisions about whether a person should be released or detained. If a person is released, the judge or magistrate must decide whether they will be released without any conditions or whether there will be conditions to their release. These conditions can include having to pay bail, being placed on electronic house arrest, or agreeing to meet with a pretrial supervisor (Digard & Swavola, 2019). If a judge or magistrate assigns bail, they set the amount and decide how it must be paid (McCoy, 2007; Wallenstein, 2014; Subramanian et al., 2015). All of the decisions from booking to first appearance are made rapidly and often without knowing much about the person in question (Kubiak et al., 2020; Stevenson & Mayson, 2017). Judges, magistrates, and other jail staff have a lot of discretion, few constraints, and minimal information to consider when making decisions that will have a large impact on the life of the justice-involved person (Digard & Swavola, 2019; Schlesinger, 2005; Worden et al., 2019).

The lack of guidance coupled with the almost total discretion given to jail staff, judges, and magistrates makes it difficult to evaluate their decisions or to ensure that decisions are being made in the same way for everyone. Yet the series of decisions made between a person's booking into jail and their first appearance in court determine that person's trajectory through the rest of the criminal justice process. Decisions that put a person at an advantage (e.g., receiving medications while in jail or not being detained pretrial) will ensure that a person has the best chance of resolving their justice involvement quickly and will reduce their likelihood of future involvement with the system. Alternatively, decisions that put a person at a disadvantage (e.g., not receiving medication in jail, being assigned a high bail amount) can lengthen a person's

duration of justice involvement and increase their likelihood of more involvement in the future. Further, early decisions that put someone at a disadvantage lead to similar decisions later on according to the theory of cumulative disadvantage (Kurlychek & Johnson, 2019). For example, a person who is detained pretrial is more likely to be found guilty at their trial and more likely to be sent to prison at their sentencing hearing (Donnelly & Macdonald, 2018; Heaton et al., 2017; Kurlychek & Johnson, 2019; Philips, 2012; Stevenson, 2018). Decisions made between booking and first appearance have major implications for a person's ultimate case outcome (Kurlychek & Johnson, 2019).

The influence of these decisions necessitates thorough examination to determine how they are being made and who they are affecting. In addition to examining the decisions themselves, we must also examine the tools that can be used to guide decision-making. Transparent, well-tested screening and assessment instruments are necessary to guide the many, impactful decisions from booking to first appearance. During the booking process, jail staff can complete several types of assessment and screening instruments with a person to identify their needs and risks. A person's needs may include medical, mental health, or substance use treatment among other things (Copp & Bales, 2018). Risks at this stage include a person's likelihood of failing to appear in court for subsequent hearings, engaging in new criminal activity during the pretrial period, or engaging in misconduct while they are held in jail (Bechtel et al., 2011; Vuk & Doležal, 2019). Information about risk and needs can then be shared with judges and magistrates to allow for more informed decision making at first appearance. The guidance provided by screening and assessment instruments can prevent disadvantageous decisions and poor outcomes such as choosing to unnecessarily detain someone who is low risk and therefore increasing their chances of being found guilty (Cohen & Reaves, 2007; Stevenson, 2018).

Conversely, having the guidance of screening and assessment instruments can result in good decisions and positive outcomes such as addressing a person's mental health needs and reducing their likelihood of rearrest (Lamberti et al., 2001). The use of screening and assessment instruments, along with other evidence-based practices, will establish transparent, uniform guidelines. These guidelines will help jail staff, judges, magistrates, and others make the most informed decisions about the millions of people who pass through jail each year.

### **Purpose**

Jails remain an under-studied component of the criminal justice process despite the number of people that are impacted by them every year. Even during a time when jails are receiving more attention nationally and awareness of the need to reform jail practices is growing, empirical research on jails lags behind. As a result, efforts to reform jail practices are moving forward without a strong evidence base. New assessment instruments are being created and practices are being put into place with limited evidence to support their use and few plans to evaluate their success. The manuscripts included in this dissertation focus on the time period from booking to first appearance and examine practices that are used at this stage of the criminal justice process.

### **Overview of Research Chapters**

In the first manuscript (Chapter 2), I examine how responses to the Brief Jail Mental Health Screen (BJMHS) change across repeated jail bookings. The BJMHS is a screening instrument that is widely used in jails throughout the country (Callahan & Noether, 2018) to identify individuals who may have serious mental illness and who should receive further evaluation (Steadman et al., 2005). This instrument has been recommended by the Substance Abuse and Mental Health Services Administration (SAMHSA) for the past 20 years (Peters et

al., 2008; SAMHSA, 2015). However, there has been relatively limited empirical investigation of the BJMHS, particularly in terms of how it is used in practice. People who are repeatedly booked into jail are screened using the BJMHS upon each new booking. Their results may change over time due to actual changes in mental health status or due to the repeated nature of the questioning and screening process (Meehan et al., 2012). Drawing from an administrative jail dataset, I obtained a sample of 12,531 people who had each been booked into jail two or more times. I then used multi-level modeling, to examine changes in BJMHS results overall and for each of the screen's four decision rules.

In the second manuscript (Chapter 3), I examine the simultaneous impact of multiple social categories and legal factors on assigned bail amounts. A major focus of the bail reform efforts currently sweeping the nation is reducing, or eliminating, the use of cash bail (Hopkins & Doyle, 2018). Yet, most studies of bail assignment have been cross sectional examinations of the main effects of social categories on bail amount. If current bail reform efforts are going to be successful in promoting more equitable and less punitive pretrial decisions, a clear understanding of exactly who is affected by these practices and how they are affected is critical. I obtained data on three and a half years' worth of bookings for a sample of 24,060 people. Then I used multi-level modeling to examine main and interactive effects of legally relevant variables (i.e., charge count, charge severity, and booking number), and race, gender, and age on bail amounts assigned.

In the third manuscript (Chapter 4), I contribute to the discussion on pretrial reform with a systematic review examining the predictive validity of pretrial risk assessment instruments overall and by subgroups (i.e., race and gender). Pretrial risk assessments are becoming widely used across the United States as a part of broader pretrial reform efforts (Pretrial Justice Institute,



2019). However, some scholars and activists question the instruments' ability to predict risk equally well across subgroups. In fact, there is concern that pretrial risk assessment instruments increase rates of pretrial detention and exacerbate the racial bias that already exists in the justice system (Barabas, et al., 2019; Harcourt, 2015; Holder, 2014; Starr, 2014). Through a systematic review of the literature, I identified 11 studies (in 13 publications) that examined the predictive validity of six pretrial risk assessment instruments. Due to the small numbers of studies I could not statistically compare estimates across instruments or studies. Instead, effect sizes were described in terms of their practical significance for the instruments' predictive validity overall and by subgroup.

Jails are currently at the center of much national attention in large part due to major efforts to reform the pretrial process that are currently underway. Reform efforts are moving rapidly. In some cases, sweeping changes to jail and pretrial practices are being made in an effort to reduce the country's mass incarceration problem (Harris et al., 2019; Larty, 2020; Smith et al., 2017). Unfortunately, some of these efforts are moving forward without strong, evidence-based support. Indeed, in many ways, we still lack a clear understanding of exactly who is being hurt in what specific ways by the current practices. This makes it difficult to select the most appropriate reform efforts and to decide how they should be carried out. Further, lack of clarity regarding the harm of current practice will make it difficult to evaluate whether reform efforts are truly making improvements. One reform effort that is frequently used to improve current practices is the administration of screening and assessment instruments. However, these instruments are being implemented before they are thoroughly validated. As a result, there are questions about whether some of these instruments may be causing more harm rather than helping (Barabas et al., 2019; Dressel & Farid, 2018; Eckhouse et al., 2019; Harcourt, 2015). New screening and assessment

instruments are not the only concern. Older instruments, some of which have been in use for years, are also lacking rigorous examination of their use in practice (Martin et al., 2013). This dissertation aims to help build up the evidence-base for practices that can be used to achieve jail reform.

## CHAPTER 2

### **Results of the Brief Jail Mental Health Screen Across Repeated Jail Bookings**

Objectives: The Brief Jail Mental Health Screen (BJMHS) is widely used at intake in county jails to identify detainees who may have serious mental illness and who should be referred for further mental health evaluation. The BJMHS may be administered multiple times across repeated jail bookings; however, the extent to which results may change over time is unclear. To that end, we examined the odds of screening positive on the BJMHS across repeated jail bookings. Methods: Data were drawn from the administrative and medical records of a large, urban county jail that used the BJMHS at jail booking. The study sample comprised BJMHS results for the 12,531 jail detainees who were booked at least twice during the 3.5-year period (total N=41,965 bookings). Multilevel logistic modeling was used to examine changes over time overall and within the four decision rules (current psychiatric medication; prior hospitalization; two or more current symptoms; referral for any other reason). Results: Results show that the odds of a positive screen overall increase with each jail booking, as do the odds of referral for any other reason. In contrast, the odds of screening positive for two or more current symptoms and prior hospitalization decrease. There was no change in the odds of screening positive for current psychiatric medication across bookings. Conclusion: Findings show that BJMHS results change across bookings. Further research is needed to determine whether changes reflect true changes in mental health status, issues with fidelity, the repeated nature of the screening process, or other factors.

## Introduction

The prevalence of serious mental illnesses, including mood disorders, schizophrenia, and other psychotic disorders, is three to four times greater among jail detainees than in the general population (1), and estimates suggest that over two million people with serious mental illness are booked into U.S. jails yearly (2). In an effort to improve clinical outcomes, and, ultimately, reduce overrepresentation of serious mental illness in jails, the American Psychiatric Association (APA) and the National Commission on Correctional Health Care (NCCCHC) issued standards for mental health screening, referral, and evaluation for inmates in jails and prisons (3, 4). Specifically, the NCCCHC requires a mental health screen within 14 days of booking, followed by an in-depth evaluation if the screen is positive (5). Accordingly, mental health screening has become commonplace in county jails across the U.S. (6) with the goal of identifying detainees at intake who may have serious mental illness and referring them for further, in-depth mental health evaluation. In this way, mental health screening may facilitate diagnosis and aid jail staff in making decisions regarding classification, treatment, and management (7).

Given the high volume of admissions, relatively low levels of staffing, and typically short periods of stay in U.S. county jails, mental health screening tools must be *very* short and easily administered. The Brief Jail Mental Health Screen (BJMHS) is one such instrument. Administered verbally by intake officers in about five minutes or less and requiring minimal training, the BJMHS queries the presence of current psychiatric symptoms, current use of psychiatric medication, and prior psychiatric hospitalization via eight yes/no questions. Detainees are referred for further mental health evaluation if they meet one or more of four decision rules (8). The BJMHS has been recommended by SAMHSA for use as a jail mental health screening tool for nearly 20 years (9; 10) and is used in jails across the US (6).

Despite its widespread use, there has been relatively limited empirical investigation of the BJMHS. Of the handful of studies that have investigated the tool, the majority focus on its performance compared to “gold standard” mental health assessment tools, such as the Structured Clinical Interview for DSM-IV (SCID-IV) (8, 11) or the Mini International Neuropsychiatric Interview assessments (12), or compared to other jail mental health screening tools, such as the Jail Screening Assessment Tool (13, 14). Taken together, evidence regarding its performance is mixed; some studies show good concordance with the criterion (11) and others, less so (12, 13, 14). Beyond criterion validity, there remain questions regarding the functioning of the tool in practice. In particular, we are not aware of research that has examined BJMHS results over time, even though detainees may be assessed multiple times across repeated jail bookings. This is the focus of the current investigation.

Mental health screening results may change over time for reasons related to changes in mental health status. For example, following release from jail, individuals with serious mental illness may face significant challenges, including, stress (15), housing instability, negative peer influences (16), employment difficulties (17), and limited access to treatment (18), that contribute to worsened illness trajectories. However, for many with serious mental illness, jail booking may represent the first point at which their symptoms are identified, and they are referred to treatment (19). As a result, they may experience improved illness trajectories across repeated bookings (20-23). Alternatively, BJMHS results may change over time for reasons other than true changes in illness trajectories. For instance, impaired insight or cognitive functioning could impact self-report at one screening but not at another (24). Further, because the BJMHS is administered as a face-to-face interview, social desirability, impression management, and rapport also may impact results (25; 26; 27). A detainee may be more comfortable with certain staff or

may become more comfortable with the process (and more likely to disclose information) over time. Staff may become familiar with an individual detainee over repeated admissions (28) and refer them for further evaluation, even in the absence of current symptoms. Or, staff beliefs, attitudes, and experiences may influence the way in which they approach detainees (29), which may affect detainee self-report and staff interpretation of that self-report. Finally, there are no training guidelines for the BJMHS (8), which may impact staff knowledge or understanding of the decision rules and, consequently, the fidelity with which the BJMHS is administered.

### **The Present Study**

While there are many reasons mental health screening results may change across repeated bookings, empirical evidence is lacking with respect to the BJMHS or any jail mental health screening tool, for that matter. Yet, mental health screening results have important implications for justice-involved persons with serious mental illness. As such, understanding factors that may affect these screening results, including repeated administration, is critical. Our goal was to determine the odds of screening positive on the BJMHS across multiple jail bookings, after accounting for relevant sociodemographic (i.e., age, sex, race, housing status) and other clinical (i.e., substance use) characteristics that may impact results. We used multilevel logistic modeling to examine changes in BJMHS results overall and for each of the four decision rules across repeated bookings in a population of detainees admitted to a large, metropolitan jail.

### **Methods**

All study procedures were approved by the university's Institutional Review Board. The sample comprised 12,531 detainees across 41,965 bookings to a large, urban county jail in the southeastern U.S. This sample was drawn from the population of detainees (N=37,988) booked into the study jail between January 1, 2013 and June 30, 2016. Only those who were readmitted

to the study jail at least one time (N=12,531) were included in this study, since our interest was in examining change in screening results over time. Data were drawn from administrative and medical records.

The BJMHS was administered to all detainees in the context of routine practice by jail nurses at intake. As briefly described in the introduction, the BJMHS consists of eight, self-report dichotomous items (no=0, yes=1). These eight items are divided into two sections. The first section includes six items that query the presence of psychiatric symptoms in the past six months. The second section queries treatment history, including one item that addresses current use of psychotropic medication (item seven) and one item that addresses lifetime psychiatric hospitalization (item eight) (8). A positive screen occurs when a detainee meets at least one of the following decision rules: 1) yes to at least two of items one through six; 2) yes to item seven; 3) yes to item eight; or 4) if the screener feels the detainee should be referred for any other reason (even in the absence of any yeses). We examined odds of meeting each of these four decision rules, as well as an overall positive screen (i.e., meeting at least one of the decision rules), as outcome variables.

An optional third section includes the screener's comments or impressions regarding several extenuating circumstances (e.g., language barrier, inmate was under the influence of drugs/alcohol). This section does not bear on screening results, and thus, was not included as an outcome variable.

The predictor variable, *booking number* during the study period, allowed us to ascertain change in BJMHS across repeated jail bookings. Covariates included: age, sex, race, homeless status, and substance use problems. *Age* (in years at the time of each booking), *sex* (0=female, 1=male), and *race* (0=non-Caucasian, 1=Caucasian) were obtained from the administrative

records. *Homelessness* (0=no, 1=yes) was self-reported at intake. *Substance use problems* (0=no, 1=yes) was a composite measure comprised of the optional third section of the BJMHS and four items from the jail intake medical screen. A person was coded 'yes' for substance use problems if they met any of the following criteria: under the influence, signs of withdrawal, self-reported drug use, or self-reported problematic alcohol use (30).

We performed several multilevel logistic regression models using SAS 9.4 PROC GLIMMIX, with bookings (Level 1) nested within individuals (Level 2). This technique allowed for examination of the rate of change in outcomes, incorporated all available data from each person, and controlled for the differences in timing of each repeated measure (i.e., time lapse between bookings). We ran one model examining odds of an overall positive screen followed by four models in which we examined each of the BJMHS decision rules separately. These five models were run once without covariates and once with covariates.

## **Results**

### *Sample Characteristics*

The average number of bookings per person in our analytic sample was  $3.35 \pm 2.53$  (Range=2-65) during the study period, the average length of stay was  $21.42 \pm 50.59$  days (Range=0-1049), and the average time between bookings was  $846.15 \pm 288.37$  days (Range=0-1276). The sample was 34.8% (N=4362) Caucasian and 65.2% (N=8162) Non-Caucasian (65.1% (N=8151) African American, 0.1% (N=11) Asian). Average age at first booking during the study period was  $31.50 \pm 11.57$  years (Range=16-79). The vast majority (81.7%, N=10232) of the sample were men. About 8.0% (N=1002) of detainees reported homelessness and 40.0% (N=4994) screened positive for problematic substance use. At their initial booking, 62.9% (N=6600) of the sample were charged with more serious offenses (typically felonies punishable



by imprisonment for a year or more; e.g., assault, burglary, sexual offenses, etc.), while 37.1% (N=3886) were charged with less serious offenses (typically misdemeanors punishable by imprisonment for less than a year; e.g., stolen property, disorderly conduct, etc.). The most common charges at initial booking included assault (21.2%, N=2654), drug/narcotic (15.3%, N=1916), and larceny/theft (11.5%, N=1440).

### *Descriptive Statistics*

In total, 23.3% (N=2900) of detainees screened positive overall on the BJMHS at initial booking during the study period. Rates of positive screens for each decision rule at initial booking ranged from 2.7% (N=340) for current psychiatric symptoms to 20.6% (N=2579) for referral for any other reason (i.e., at the assessor's discretion). Visual exploration of the descriptive statistics suggested the frequency of positive screens may be changing over time; however, rates and directions of change seem to differ across items and decision rules (see Figure 1). A breakdown of positive responses to individual BJMHS items and to BJMHS decision rules can be seen in Table 1.

### *Multilevel Logistic Models*

Table 2 presents the estimates for each of the five multilevel logistic models. Results of the first multilevel logistic model confirmed that the odds of a positive screen overall (i.e., meeting at least one of the decision rules) increased with each additional booking into jail. Specifically, each additional booking was associated with a 2% increase in the odds of a positive screen (OR=1.02, 95%CI=1.01-1.04). Results of the model including covariates showed the same the pattern. (Full results available in online supplement.)

Results of the multilevel logistic models examining each of the decision rules showed that the odds changed across repeated bookings for three of the four rules (see Table 2). The odds

of screening positive for prior hospitalization and for reporting two or more current psychiatric symptoms *decreased* over time. With each additional booking, the odds of screening positive for prior hospitalization decreased by about 4% (OR=0.96, 95%CI=0.95-0.98) and the odds of screening positive for current symptoms decreased by about 5% (OR=0.95, 95%CI=0.93-0.98). In contrast, detainees were more likely to be referred for any other reason (i.e., at the assessor's discretion) with each additional booking into jail. For this decision rule, each additional booking was associated with a 3% increase in the odds of referral (OR=1.03, 95%CI=1.02-1.04). Finally, the odds of screening positive for current use of psychiatric medication did not change across bookings (OR=1.01, 95%CI=1-1.02). As before, results of the models including covariates showed the same the patterns of results. (Full results available in online supplement).

## **Discussion**

Across the United States, the BJMHS is administered multiple times to detainees who cycle in and out of county jails; yet, little is known about how these mental health screening results may change across repeated jail bookings. Drawing data from bookings into a large, metropolitan county jail, we examined whether BJMHS results changed overall and by decision rule. Our findings demonstrate that BJMHS results do change across repeated jail bookings. In particular, we found that the odds of screening positive for prior hospitalization and current psychiatric symptoms decreased across bookings while the odds of screening positive for current psychiatric medication remained unchanged. Odds of referral for any other reason and of screening positive overall increased across bookings. These patterns of results remained even after controlling for demographic variables, homelessness, and substance use. In the sections that follow, we discuss these findings in more detail.

## Summary of Findings

Detainees were less likely to screen positive for current psychiatric symptoms across repeated bookings. On the one hand, it is possible that a decrease in the reporting of symptoms reflects improvements in illness trajectories. Indeed, as discussed earlier, jails may represent a critical point of identification, referral, and initiation of psychiatric treatment for many adults with serious mental illness. On the other hand, changes in responses to the symptom items across bookings may not necessarily represent improved illness trajectories. Instead, different responses may reflect natural fluctuation - but not necessarily marked improvement - in clinical presentation (31). Further, symptoms not queried by the BJMHS, such as symptoms associated with anxiety disorders, may have worsened over time and gone undetected by the BJMHS (32).

Our findings also may be attributable to problems associated with the self-report nature of the items, such as differences in insight or willingness to disclose information. Indeed, the question about hospitalization on the BJMHS queries lifetime hospitalization. The fact that detainees were less likely to screen positive for lifetime hospitalization across repeated bookings cannot indicate that lifetime hospitalization rates decreased. Rather, this indicates that reporting of hospitalization decreased among detainees who had previously reported being hospitalized; in other words, a small proportion of detainees (0.5%) recanted their prior hospitalization. This calls into question the reliability of this item across repeated administrations. Alternatively, it is possible that jail staff skipped this question at some bookings or forgot to record detainees' responses. It is also possible that changes in this item are due to data entry errors. These possibilities are important areas for further investigation.

To further illustrate the susceptibility of BJMHS items to reporting issues, research demonstrates that some people find repeated screening or assessment to be therapeutic, possibly

increasing the likelihood they will self-report symptoms, while others find it frustrating and suspicious (33). The jail mental health screening process is unlikely to confer positive therapeutic effects. Instead, it seems more realistic that having to answer the same questions across multiple bookings led detainees to feel that staff are not paying attention to their answers or that staff are trying to catch them giving conflicting answers. Detainees may grow tired of repeating the same responses across multiple screens and become less likely to report current symptoms or prior hospitalization. Further, detainees may be fearful that their disclosing symptoms will be used to deny them pretrial release, lead to involuntary hospitalization, or impact their case in some way (33). Finally, detainees may be especially less likely to disclose mental health information if they feel that the disclosure of this information is not being used in a meaningful way (33), a strong possibility given the limited availability of treatment in jails generally (34) and the study jail, specifically.

In spite of the decreases in self-reported symptoms and hospitalization, the odds of a positive screen overall increased across bookings. This increase appears to be driven by increases in discretionary referral for “any other reason”. This may indicate that jail staff do not rely on the results of the other three decision rules when making referral decisions. In this study, the BJMHS only detected current symptoms in about 2% of detainees while other studies place estimates of serious mental illness at 14.5% for men and 31.0% for women (2). Staff may not trust the screening results due to incredibly low detection rate which may explain why staff referral rates increase over time even though positive screens for the other three decision rules decrease. At the same time, however, this discretion is built into the BJMHS itself as one of the decision rules dictates that detainees should be referred “if [staff] feel it is necessary for any other reason” (8). Thus, it may not be that staff do not trust the screening results but rather that they do not trust the

detainee self-report. Such a “professional override” is included in many instruments designed for use in correctional settings (35). In this way, assessors are using the BJMHS as designed.

Whether this rate of professional override is acceptable is questionable; no guidance nor research exists to support the acceptable rate of professional override on this or any other instrument. This is an important avenue of investigation in relation to the BJMHS and more generally.

It is also possible that symptoms not included on the BJMHS increased across repeated bookings and were identified by jail staff, resulting in referral. Or, staff may become familiar with a detainee across repeated bookings and refer based on past rather than current clinical presentation. The familiarity hypothesis suggests that an evaluator will gain additional information with each subsequent administration of a test which leads to improved assessment accuracy (28). However, this hypothesis has not always held when tested within the context of risk assessments (36; 28) and thus, warrants further examination in the context of jail mental health screening. Indeed, confirmation bias suggests that such prior knowledge – or familiarity - may result in decreased screening accuracy over time. It is also possible that changes in screening results across bookings reflect the limited training jail staff receive prior to implementation of the BJMHS. There is no formal training required for the BJMHS, which may impact the fidelity with which the screen is administered and scored. Establishing a standardized training protocol, including booster training, should improve adherence to items and decision rules. Finally, changes in screening results may be due to poor reliability of the BJMHS items. A review of BJMHS studies demonstrated highly variable findings across studies (37) calling into question the tool’s accuracy and consistency in detecting mental health problems. Jails may be better served by other screening measures such as the Correctional Mental Health Screen or the

Jail Screening Assessment Tool, which have been demonstrated in other studies to outperform the BJMHS in terms of detecting mental health problems among jail inmates (14, 38).

### **Limitations**

A few limitations of the study design should be noted. First, we do not have information on the number of times a person was booked into this jail prior to our study period. As such, our first BJMHS results may not be the first time they were screened. Second, because the BJMHS is the only mental health screen administered in this jail and follow-up with an in-depth mental health evaluation was inconsistent, we were unable to compare results to another screening tool or against a criterion measure, such as the SCID or clinical diagnosis. For these reasons, we cannot determine whether the observed changes reflect actual changes in mental health status, issues with fidelity of administration, issues of disclosure and reporting of symptoms, characteristics of the BJMHS itself, or otherwise. Third, we did not have access to treatment data and cannot speak to the treatment-related or other mechanisms through which BJMHS results changed. Fourth, we investigated changes in mental health screening results across repeated jail bookings using one tool in one jurisdiction; generalizability of findings must be examined in future research. Despite these limitations, the study's strength lies in our longitudinal analysis of results of a widely used mental health screening tool in a large jail population. This is the first study of its kind.

### **Conclusion**

Mental health screening tools may be administered repeatedly to individuals who cycle in and out of jails (6). Yet, there has been limited investigation of whether results of these jail mental health screens change over time and the regulatory bodies that issue standards for screening and assessment of jail detainees have not provided guidance regarding screening

protocols for repeat detainees (3; 4). Mental health screening results can have serious implications for jail detainees, as they are administered at a critical intercept for identification, referral, and intervention (19). Given that jails have the highest volume of justice-involved individuals in the U.S., mental health screening tools used in jails have the potential to affect millions of people. For these reasons, it is imperative that research examines how mental health screening results change over time, as well as the mechanisms of change. Indeed, examinations of test-retest reliability and measurement of mental health symptoms over time are recognized as crucial steps in the development of psychiatric measures (39). Jail mental health screening tools must be examined in this way as well. Furthermore, decision rule number four, referral for any other reason (i.e., the professional override), was the only decision rule to increase over repeated bookings. While it is positive that jail staff are attentive to the mental health needs of detainees beyond those captured by the BJMHS, it is critical to uncover the factors that jail staff are considering in making these override decisions. On the one hand, such research may elucidate factors that could improve the accuracy and utility of the BJMHS or other jail-based, mental health screening tools. On the other hand, such research may identify jail staff's reliance on factors irrelevant to mental health but instead reflective of misconceptions or bias. The latter finding would represent issues of concern for implementation that may require training. Professional overrides are an understudied component of jail assessment tools. Our findings provide evidence of an increased use of professional overrides across repeated assessments and emphasize the need for research to understand the use of professional overrides in jail assessment tools.

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**Table 1.** Affirmative Responses to Brief Jail Mental Health Screen Items and Decision Rules Overall and Across First Three Bookings

Construct	Brief Jail Mental Health Screen Item	All Bookings N=41965		Booking Number					
				1 N=12531		2 N=12531		3 N=6199	
		n	%	n	%	n	%	n	%
Thought control	1. Do you <i>currently</i> believe that someone can control your mind by putting thoughts into your head or taking thoughts out of your head?	543	1.3	194	1.6	166	1.3	63	1.0
Paranoia	2. Do you <i>currently</i> feel that other people know your thoughts and can read your mind?	484	1.2	167	1.3	146	1.2	64	1.0
Weight loss/gain	3. Have you <i>currently</i> lost or gained as much as two pounds a week for several weeks without even trying?	579	1.4	202	1.6	162	1.6	82	1.3
Activity level	4. Have you or your family or friends noticed that you are <i>currently</i> much more active than you usually are?	531	1.3	178	1.4	151	1.4	74	1.2
Lethargy	5. Do you <i>currently</i> feel like you have to talk or move more slowly than you usually do?	492	1.2	184	1.5	135	1.5	65	1.1
Feeling useless/sinful	6. Have there <i>currently</i> been a few weeks when you felt like you were useless or sinful?	1941	4.7	701	5.7	578	4.6	278	4.5
Current medication	7. Are you <i>currently</i> taking any medication prescribed for you by a physician for any emotional or mental health problems?	5284	12.7	1446	11.6	1608	12.9	776	12.6
Lifetime hospitalization	8. Have you <u>ever</u> been in a hospital for emotional or mental health problems?	3903	9.4	1198	9.6	1137	9.1	594	9.7
<b>Decision Rules</b>	<b>Criteria</b>								
Current psychiatric medication	Yes to item 7.	5284	12.7	1446	11.6	1608	12.9	776	12.6
Lifetime hospitalization	Yes to item 8.	3903	9.4	1198	9.6	1137	9.1	594	9.6
Current symptoms	Yes to 2 of items 1 through 6.	969	2.3	340	2.7	275	2.2	132	2.1
Referral for any other reason	Referral for reasons other than above criteria.	3048	7.3	830	6.6	891	7.1	428	6.9

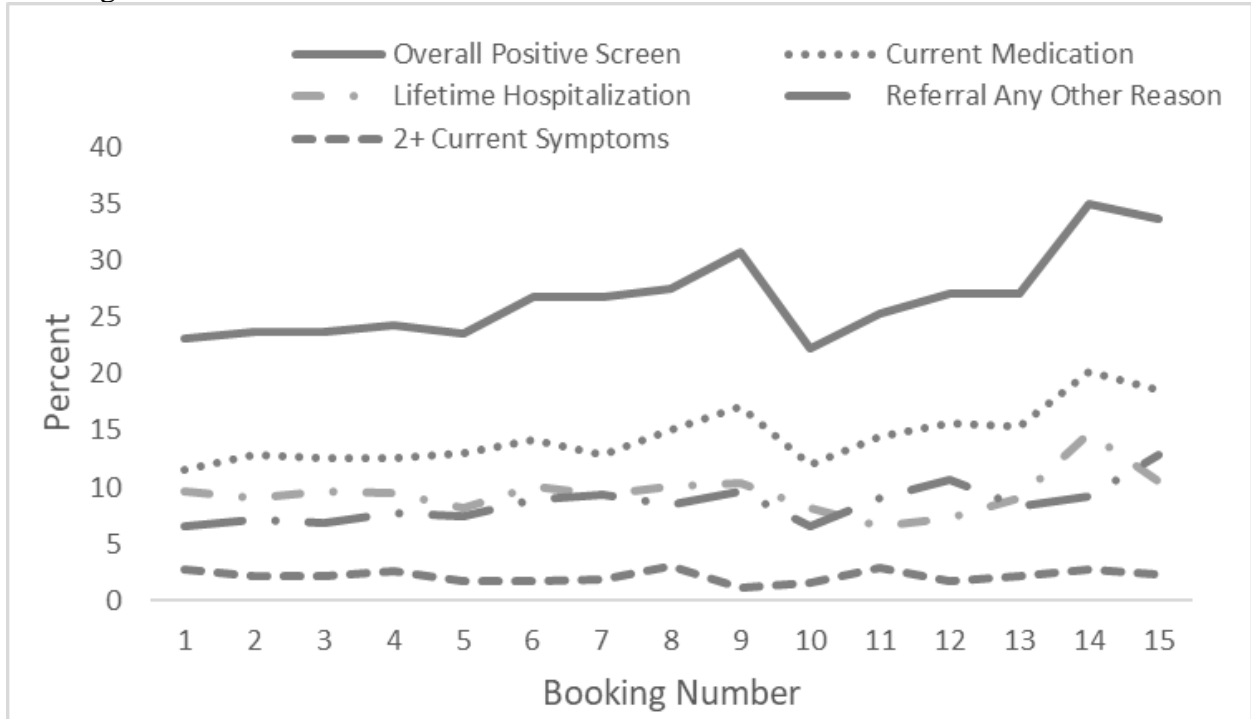
Notes. % = valid percentage of affirmative responses. n = number of affirmative responses.

**Table 2.** Multilevel Logistic Models of Booking Number Predicting Brief Jail Mental Health Screening Results

Model	Model Estimates						
	-2LL	Estimate	SE	<i>t</i>	<i>df</i>	OR	95% CI
1. Met criteria for a positive screen overall	194522.6	0.025	0.006	4.34	41686	1.02	1.01-1.04
2. Currently taking psychiatric medication	209701.6	0.010	0.007	1.49	41833	1.01	1.00-1.02
3. Experienced any prior hospitalization	216054.0	-0.037	0.008	-4.48	41807	0.96	0.95-0.98
4. 2 or more current psychiatric symptoms	257560.7	-0.049	0.014	-3.55	33298	0.95	0.93-0.98
5. Referral for any other reason	220830.3	0.030	0.006	5.12	29143	1.03	1.02-1.04

*Notes.* -2LL = negative two log likelihood; SE = standard error for the estimate; *t* = *t* value; *df* = degrees of freedom; OR = odds ratio; 95% CI = 95% confidence interval for the odds ratio.

**Figure 1.** Percent of Jail Detainees who Screened Positive Overall and by Decision Rule across Bookings



## CHAPTER 3

### **Bias in Bail Assignments: An Examination of the Interactions Between Social Categories and Legal Factors**

Across the United States, efforts are underway to reduce bias in bail assignments. However, few studies have considered the simultaneous impact of multiple social categories on bail assignments. Additionally, existing studies have focused primarily on felony defendants. To address these gaps in knowledge, our goal was to examine main and interactive effects of legally relevant variables (i.e., charge count, charge severity, and booking number), and race, gender, and age on bail amounts assigned across repeated jail bookings. We used multilevel modeling in a heterogeneous sample of defendants ( $N = 24,060$ ) booked into a large, urban county jail over a three-and-half-year period. All legally relevant variables demonstrated main effects in the expected direction. However, 2-way and 3-way interactions with race, gender, and age qualified these main effects. One 3-way interaction between charge severity, age, and race demonstrated that older and Black defendants with less serious charges were given higher bail amounts than younger and White defendants with more serious charges. Another 3-way interaction between race, gender, and age showed that older defendants were assigned higher bail amounts for all race and gender combinations except Black men: younger Black men were assigned higher bail than older Black men. Taken together, findings show bias in bail assignments that are at the intersections of multiple legal factors and social categories. These findings emphasize the importance of examining the interactions of legal factors and social categories to reach a nuanced understanding of bias in bail assignments that can inform evidence-based bail reform efforts.

*In preparation*



## Introduction

Across the United States, we find ourselves in the third period of bail reform in 60 years, with the goal of maximizing pretrial liberty and reducing disparities in pretrial decisions, while maintaining public safety and court appearance rates (Desmarais & Lowder, 2019; Pretrial Justice Institute, 2017). Toward this effort, lawsuits are being argued to challenge the constitutionality of current bail practices, which are often punitive rather than preventative. In particular, there is a great deal of focus on reforming the money bail system (The Justice Collaborative, n.d.). Judicial orders and amendments to state constitutions now require judges to find that a defendant is able to pay the amount of bail that is set. Prosecutors around the country are revising their policies and *not* requesting money bail for defendants with low-level offenses (e.g. stolen property, disorderly conduct, or trespassing). Non-profits and advocacy groups help pay bail and advocate for those detained pretrial. Finally, many states are implementing pretrial risk assessment tools to help promote more transparent, accurate, and fair pretrial decision-making, and some states are moving away from the use of money bail altogether (Hopkins & Doyle, 2018). A majority of these efforts center on reducing disparities in pretrial detention rates, specifically, and bail is one path to being detained pretrial (Wooldredge et al., 2015). Indeed, the inability to pay bail contributes largely to whether an individual is held in pretrial detention (Cohen & Reaves, 2007; New York City Criminal Justice Agency, 2016; Rabuy & Kopf, 2016).

Without question, the efforts of the third wave of bail reform are laudable and sorely needed. Almost two decades of research demonstrate racial biases in bail assignments and detention rates, as well as the downstream negative effects of pretrial detention on case processing, and even, community reentry after disposition (Ayres & Waldfogel, 1994; Demuth & Steffensmeier, 2004; Free, 2002; Jones, 2013; Wooldredge, 2012; Wooldredge, et al., 2015).

Yet, this emphasis on racial bias has been largely to the exclusion of consideration of the ways that bail amounts are disproportionately assigned across multiple, intersecting social categories. To that end, the purpose of this study is to examine potential biases at the intersection of race, gender, and age in bail amounts assigned over time in a jurisdiction that has not yet engaged in bail reform. In the sections that follow, we briefly review the correlates and consequences of bail, highlighting some current gaps in the literature.

### **Consequences and Determinants of Bail Amount**

The amount of bail a defendant is assigned can directly impact whether they are held in pretrial detention in one of two ways. First, the defendant cannot afford to pay bail, even at low amounts, due to restricted financial resources (New York City Criminal Justice Agency, 2016). Second, the bail amount is intentionally set unattainably high with the purpose of preventing the defendant from being able to make bail (Liu et al., 2018). Indeed, financial resources have become one of the dominant factors determining whether someone is held pretrial or released (Sawyer & Wagner, 2020; Subramanian et al., 2015). Studies demonstrate that in some jurisdictions, as many as 40-87% of people held pretrial are held because they could not afford bail (Human Rights Watch, 2010; New York City Criminal Justice Agency, 2018). Regardless of the reason a defendant cannot pay bail, the end result is pretrial detention which has numerous consequences.

Defendants held pretrial are more likely to engage in plea negotiations and to plead guilty than those who are released, possibly due to detained defendants' inability to meet with their lawyers or their desire to secure release from jail (Kellough & Wortley, 2002; Heaton, et al., 2017; McCoy, 2007; Sacks, et al., 2015; Ottone & Scott-Hayward, 2018). Further, pretrial detention is associated with increased rates of conviction (Stevenson, 2018; Philips, 2012; Cohen

& Reaves, 2007), likelihood of being sentenced to prison or jail, and increased sentence length (Heaton, et al., 2017; Philips, 2012; Stevenson, 2018), even after controlling for legal (e.g., offense seriousness) and extralegal (e.g., race, gender) factors (Williams, 2003). Finally, defendants held pretrial experience damaged family bonds, poorer mental and physical health, lost income and employment, and lost property due to their inability to work while detained (Bak, 2002; Ottone & Scott-Hayward, 2018; Rabinowitz, 2010). The direct impact of bail amount on pretrial detention is concerning in light of the fact that the use and amounts of money bail are not consistently applied across defendants (Demuth & Steffensmeier, 2004; Freiburger, et al., 2010; Freiburger & Hilinski, 2010; Wooldredge, 2012; Wooldredge, et al., 2015).

When it comes to assigning bail, the Bail Reform Act of 1984 set a broad range of legally permissible considerations in determining the amount that can be set. This act permits judges to consider the nature and seriousness of the offense, the weight of evidence against a defendant, the defendant's background and characteristics (e.g. financial resources, family ties, community ties), and the level of danger the defendant poses to the community (Bail Reform Act of 1984, 1984; Spohn, 2009). Bail decisions are made during rapid and informal hearings, often only a few minutes long and commonly occurring via videoconferencing without lawyers present (Stevenson & Mayson, 2017). Most jurisdictions rely on bail schedules or statutory laws that provide guidelines to set bail as a function of charge severity and other legally permissible factors (Gouldin, 2016; Pretrial Justice Institute, 2010). While these methods represent efforts to increase uniformity and fairness in bail amounts, they fail to provide guidance regarding how factors should be weighted, and which are most relevant to a defendant's likelihood of failure to appear versus dangerousness to community (Stevenson & Mayson, 2017).

As a result of the combination of lack of time and limited guidance, judicial decision makers may fall back on heuristic principles (Tversky & Kahneman, 1974) and racialized crime scripts (Harris et al., 2011) when setting bail. This could result in biased decisions. For example, according to the illusion of validity heuristic, a judicial decision maker may assign a higher bail if they know a defendant comes from a neighborhood with a high violent crime rate. Racialized crime scripts occur when an identified group (e.g., Black people) are associated with a particular crime (e.g., illegal drug distribution). If a judicial decision maker is operating with this racialized crime script, they may assume that Black defendants are more likely to be guilty of a drug charge and assign a higher bail amount. Experts, including judicial decision makers with years of experience, are as prone to these mental shortcuts as non-experts (Tversky & Kahneman, 1974). Evidence of heuristics and racialized crime scripts in bail assignment can be seen in studies that have found disparities in bail amounts across different social categories. Specifically, women are assigned lower bail amounts than men (Demuth & Steffensmeier, 2004; Ball & Bostaph, 2009; Pinchevsky & Steiner, 2016) while Black and Hispanic defendants are assigned higher bail amounts than their White counterparts, all after considering legally relevant factors (Arnold, et al., 2018; Demuth, 2003; Schlesinger, 2005; Wooldredge et al., 2015).

### **The Importance of Examining Interactions**

The impact of individual characteristics on the amount of bail a defendant is assigned has been documented. However, judicial decision makers do not consider defendants in a unidimensional way, rather they consider multiple social categories (e.g., race *and* gender *and* age) simultaneously when setting bail (Kang & Bodenhausen, 2015). If we only consider single identities when examining differences in bail amount, we will conflate the outcomes for Black men with Black women or the outcomes for Black women and White women, and oversimplify

the complex interaction of factors that are affecting bail assignments (Crenshaw, 1991; 1989). The concept of intersectionality emphasizes the importance of examining oppression at the nexus of multiple social categories because people experience greater or lesser degrees of oppression based on all their identities simultaneously (Carbado, 2013; Crenshaw, 1991; Cho et al., 2013; McCall, 2005; Potter, 2013). By examining just one social category, we reproduce the subordination of those that have not been considered (Crenshaw, 1991). Examining bail amounts across different combinations of social categories will clarify how they contribute to the inequality different defendants experience in this context (Cole, 2009). Doing so is critical to ensuring that bail reform efforts do not unintentionally reproduce or exacerbate those inequalities.

Findings from studies that have examined the simultaneous influence of multiple social categories on bail amounts are limited but support the importance of the approach for clarifying patterns of disparity. To demonstrate, one study of 39,435 felony defendants from 75 counties throughout the United States showed that Black and Hispanic male defendants are assigned higher bail than White male defendants (Demuth & Steffensmeier, 2004). Other studies have found that young, Black men, specifically, are assigned higher bail amounts than White men, older Black men, and Black and White women of any age (Wooldredge, 2012; Wooldredge et al., 2015). Prior research also suggests that Black, White, and Hispanic women are assigned lower bail amounts than men in the same racial/ethnic groups (Demuth & Steffensmeier, 2004). Some studies have found differences in other pretrial decisions but no differences in bail amount (Freiburger et al., 2010; Freiburger & Hilinski, 2010; Pinchevsky & Steiner, 2016). Further, several of these studies failed to find a main effect of race on bail amount, but did find interactional effects when examining race, gender, and age together (Demuth & Steffensmeier,

2004; Wooldredge, 2012; Wooldredge et al., 2015). Consequently, in the absence of testing interactions, we may inaccurately conclude there is a lack of racial bias in bail assignments.

While these studies have advanced our understanding of the impact of multiple social categories on decisions about bail amount, they do have some limitations. In particular, several of these studies (Demuth & Steffensmeier, 2004; Freiburger & Hilinski, 2010; Pinchevsky & Steiner, 2016; Wooldridge, 2012; Wooldredge et al., 2015) focused exclusively on felony defendants. As such, there is a need to examine bias experienced by defendants booked for charges with lower levels of severity, particularly as these defendants make up a larger proportion of individuals booked into jail (Subramanian et al., 2015). Additionally, only one prior study examined interactions between social category and legally relevant factors (Pinchevsky & Steiner, 2016), and they only considered gender for these interactions. Further, none of these studies followed defendants over time. Examining the changes in bail amounts over time will help clarify the ways in which inequalities may be exacerbated as a result of repeated involvement with the criminal justice system. Finally, attending to intersections of multiple social categories with each other and with legal factors is critical to our comprehensive understanding of who is the most marginalized and disenfranchised by current pretrial practices (Crenshaw, 1991). Doing so will help guide reform with the greatest potential to effectively reduce the bias in bail decisions where that bias has the greatest impact.

### **The Current Study**

Little is known about the intersection of multiple social categories as they relate to decisions about bail. Yet, bail reform efforts are moving forward across the nation with great momentum (Baughman, 2019). For these efforts to be successful in promoting more equitable pretrial decisions, a clear understanding of exactly who is impacted by these practices and how

they are impacted is critical. Thus, we drew data from a jail that has not yet begun reform. First, we examined the impact of legally relevant variables. We considered booking number, in order to examine changes in bail amount assigned across repeated jail bookings, along with charge count and charge severity. Then, we examined social categories, specifically the main effects and interactions of race, gender, and age in their influence on the amount of bail assigned. We examined interactions because considering main effects alone could obscure the outcomes for subgroups within each category (Cole, 2009). In fact, in the case of a cross-over interaction, we would fail to find a main effect, but the interaction would provide important information about how bail is assigned for each of the subgroups within the interaction. Finally, we examined interactions between these social categories and legally relevant factors to explore whether the impact of legally relevant factors differs by race, gender, and age.

### **Methods**

Analyses were conducted retrospectively in a large, secondary dataset. All study procedures were approved by the university's Institutional Review Board.

#### **Data**

Data were drawn from administrative and clinical records maintained by the study jail. Data were linked across records using booking IDs, which correspond to a unique jail booking. We then used the unique identifiers assigned to defendants (i.e., name IDs) to identify bookings for each individual defendant. Thus, the data were in a nested format with bookings nested within defendants.

#### **Participants**

The study sample included all defendants booked into a large, urban county jail between January 1, 2013 and June 30, 2016 who were assigned a bail amount. Race was a variable of

primary interest, thus we excluded bookings for which data on race was missing (0.1%, n=24). We also excluded bookings for which defendants were identified as Asian (0.2%, n=57) and Native American (0.0%, n=14), because these sample sizes could not support analysis. Thus, our race comparisons focus on bookings for Black and White defendants, which represented 99.8% of the study sample. Our final study sample included 24,060 defendants across 32,399 bookings.

## Measures

**Outcome.** *Bail amount* was measured at the booking level as the sum of the bail amounts for all charges at each booking during the study period. Total bail amounts were not normally distributed (skewness = 22.57) so a log<sub>10</sub>-base transformation was used to correct the skewness. One booking entry was excluded after transforming bail amount because the bail amount of \$0.10 resulted in a log<sub>10</sub> bail amount of -1.00. There was no missingness for this variable.

**Level 1 predictors.** *Booking number* was measured as a count of defendants' bookings into the jail during the study period. We subtracted 1 from each booking number in order to create a meaningful intercept. So, booking 0 represented a defendant's index booking into the jail during the study period. Booking number 1 was a defendant's first time being rebooked into jail (i.e. recidivism), booking number 2 was their second time being rebooked into jail, and so on. This variable allowed us to ascertain change in bail amounts across repeated jail bookings for an individual defendant. There was no missingness for this variable.

*Charge count* was measured as the number of distinct charges at each booking during the study period and was obtained from the jail intake records based upon the information recorded by the arresting officer. There was no missingness for this variable.

*Charge severity* was measured as either A-level or B-level, operationalized in accordance with the Federal Bureau of Investigation's Uniform Crime Reporting Program. Serious, often



felony-level offenses (e.g., assault, robbery, etc.) were categorized as A-level. Less serious, often misdemeanor-level offenses (e.g., drunkenness, disorderly conduct, etc.) were categorized as B-level. This was done because the study jail has a unique charge classification system that is unlikely to generalize outside this jurisdiction. This variable was coded for each booking into the jail during the study period based upon the information recorded by the arresting officer. For analyses, defendants were coded to indicate the highest charge level for each booking; to demonstrate, for a defendant charged with a combination of A- and B-level offenses at one booking, charge severity would be coded as A-level for that booking. This variable was missing for 5.8% ( $n = 1,891$ ) of the bookings in the sample.

*Age* was measured as a defendants' reported age upon each individual instance of booking into the jail. This variable was centered around the mean age of the sample across all bookings which was 31.75 years. There was no missingness for this variable.

**Level 2 predictors.** *Race* (Black, White) and *Gender* (male, female) were measured at the person level, based upon the data recorded at the defendant's first booking into the jail during the study period. Neither race nor gender were self-reported. Both of these variables were recorded by jail intake staff based on a defendant's appearance. These data came from an administrative jail database in which the only options available for gender were man and woman. There was no missingness for either of these variables.

## **Data Analysis**

We performed a non-randomly varying slopes multilevel model (Raudenbush & Bryk, 2002) using SAS 9.4 PROC MIXED. This technique allowed us to examine differences in bail amount across repeated jail bookings. It also allowed us to use all available data from each defendant and control for the differences in timing of each repeated measure of bail (i.e., time

lapse between bookings). Our analysis is organized as a two level, hierarchical model with bookings, Level 1, nested within defendants, Level 2. The model equation can be seen below.

$$\text{Level 1: } \text{LOG OF BAIL AMOUNT}_{it} = \beta_{0it} + \beta_{1it} (\text{BOOKING NUMBER}) + \beta_{2it} (\text{CHARGE COUNT}) + \beta_{3it} (\text{CHARGE SEVERITY}) + \beta_{4it} (\text{AGE}) + \beta_{5it} (\text{BOOKING NUMBER} * \text{AGE}) + \beta_{6it} (\text{CHARGE COUNT} * \text{AGE}) + \beta_{7it} (\text{CHARGE SEVERITY} * \text{AGE}) + r_{it}$$

$$\begin{aligned} \text{Level 2: } \beta_{0i} &= \gamma_{00} + \gamma_{01} (\text{GENDER}) + \gamma_{02} (\text{RACE}) + \gamma_{03} (\text{GENDER} * \text{RACE}) + u_{0i} \\ \beta_{1i} &= \gamma_{10} + \gamma_{11} (\text{GENDER}) + \gamma_{12} (\text{RACE}) + \gamma_{13} (\text{GENDER} * \text{RACE}) \\ \beta_{2i} &= \gamma_{20} + \gamma_{21} (\text{GENDER}) + \gamma_{22} (\text{RACE}) + \gamma_{23} (\text{GENDER} * \text{RACE}) \\ \beta_{3i} &= \gamma_{30} + \gamma_{31} (\text{GENDER}) + \gamma_{32} (\text{RACE}) + \gamma_{33} (\text{GENDER} * \text{RACE}) \\ \beta_{4i} &= \gamma_{40} + \gamma_{41} (\text{GENDER}) + \gamma_{42} (\text{RACE}) + \gamma_{43} (\text{GENDER} * \text{RACE}) \\ \beta_{5i} &= \gamma_{50} + \gamma_{51} (\text{GENDER}) + \gamma_{52} (\text{RACE}) \\ \beta_{6i} &= \gamma_{60} + \gamma_{61} (\text{GENDER}) + \gamma_{62} (\text{RACE}) \\ \beta_{7i} &= \gamma_{70} + \gamma_{71} (\text{GENDER}) + \gamma_{72} (\text{RACE}) \end{aligned}$$

At Level 1, log of bail amount is the outcome that is assessed for defendants,  $i$ , at multiple bookings,  $t$ . This outcome is the function of the intercept,  $\beta_{0it}$ , and each of the Level 1 variables. The intercept,  $\beta_{0it}$ , is defined as the amount of bail a defendant,  $i$ , is assigned when all variables are equal to 0. Each of the Level 1 variables are represented by  $\beta_{xit}$  numbered in sequential order.  $\beta_{1it}$  is the expected change in bail amount as the number of bookings increases,  $\beta_{2it}$  is the expected change in bail amount based on the number of charges,  $\beta_{3it}$  is the expected change in bail amount based on the charge level, and so on.  $\beta_{1it} - \beta_{4it}$  represent main effects.  $\beta_{5it} - \beta_{7it}$  represent interaction effects between Level 1 variables. Finally,  $r_{it}$  and  $u_{0i}$ , are error terms that represent the within-person and between-person fluctuation (i.e., residual variance), respectively, in bail amount after accounting for all variables.

In the first Level 2 equation,  $\gamma_{00}$  represents the grand mean (i.e. the mean bail amount for the entire sample when all variables are 0),  $\gamma_{01}$  is the main effect of gender on bail amount,  $\gamma_{02}$  is the main effect of race on bail amount, and  $\gamma_{03}$  is the effect of the interaction of gender and race on bail amount. In subsequent Level 2 equations, the initial gammas (e.g.  $\gamma_{10}$ ,  $\gamma_{20}$ ,  $\gamma_{30}$ , etc.)

represent the average change in bail amount for each of the corresponding Level 1 variables. So,  $\gamma_{10}$  represents the average change in bail amount for the entire sample as the number of bookings increases,  $\gamma_{20}$  represents the average change in bail amount for the sample as the number of charges increases, and so on. Cross-level interactions are represented by the second, third, and fourth gammas in each Level 2 equation. These interactions test whether the within-person relationship between bail amount and each of the Level 1 variables depends on gender, race, or the interaction of gender and race. These gammas represent change in bail amount for the interactions of Level 1 and Level 2 variables. We performed our analysis as one model in which we examined main and interaction effects (up to 3-way interactions) of all variables on bail amount. We constrained all Level 2 slopes besides the intercept because the model would not converge when the slopes were allowed to vary.

### **Results**

Our analytic sample was 59.0% Black ( $n = 14,204$ ) and the majority of the sample were men (77.4%). The mean bail amount across bookings was \$24,033 ( $SD = \$142,217$ ; range = \$1.00 –\$7,510,000). Table 1 provides descriptive information about bail amounts assigned for each of the defendant subgroups based on race, gender, and age. Mean age (31.75,  $SD = 11.26$ ) was used to divide defendants into younger and older categories for the purpose of illustrating differences in bail amounts across subgroups. The mean bail amount for women falls between \$12,172 for younger, White women and \$12,950 for older, Black women. For men, mean bail amount ranges from \$21,432 for younger, White men and \$30,417 for younger, Black men.

We began by running a fully unconditional model (i.e. a model without any predictors included). This is necessary to determine whether there is enough variability in the dependent variable at Level 1, within people, and Level 2, between people, to warrant further analysis.

Results from the fully unconditional model indicated that 36.4% of the variability in bail amount was between people ( $\tau_{00} = 0.168$ ,  $z = 33.40$ ,  $p < .001$ ) and 63.6% was within people ( $\sigma^2 = 0.294$ ,  $z = 66.87$ ,  $p < .001$ ). The unconditional model results indicate that there is sufficient variability at both levels to continue our analysis.

Results for the model examining bail amounts can be seen in Table 2. There were significant main effects for all three legally relevant variables. Specifically, bail amount increased significantly as a defendant's number of bookings increased ( $\gamma_{10} = 0.041$ ,  $t = 4.93$ ,  $p < .001$ ). Bail amount also increased significantly as charge count increased ( $\gamma_{20} = 0.202$ ,  $t = 22.53$ ,  $p < .001$ ) and bookings with A-level (i.e. more serious) charges received higher bail amounts than bookings with B-level (i.e. less serious) charges ( $\gamma_{30} = 0.000$ ,  $t = 1.98$ ,  $p = .048$ ). Of the variables that measured social categories, there was a main effect for gender. Men were assigned higher bail amounts than women ( $\gamma_{01} = 0.089$ ,  $t = 3.63$ ,  $p < .001$ ). The main effects for age and race were not significant (see Table 2).

Next, we considered two-way interactions between legally relevant and social category variables and between each of the social category variables. There was a significant interaction between booking number and age ( $\gamma_{50} = -0.004$ ,  $t = -6.01$ ,  $p < .001$ ). Younger and older defendants had similar bail amounts when their number of bookings was low. However, as number of bookings increased, younger defendant's bail amounts increased much more rapidly than older defendant's bail amounts. There were also significant interactions between charge severity and gender ( $\gamma_{31} = -0.000$ ,  $t = -2.32$ ,  $p = .020$ ) and between number of charges and gender ( $\gamma_{21} = -0.024$ ,  $t = -2.37$ ,  $p = .018$ ). Men were assigned higher bail amounts than women for both A- and B-level charges. In fact, men with B-level charges received higher bail amounts than women with A-level charges. The interaction between charge count and gender revealed that

men received higher bail amounts for a low number of charges, but women received higher bail amounts for a high number of charges. The remaining two-way interactions were not significant (see Table 2).

Finally, we examined three-way interactions between legally relevant variables and social category variables; we also examined interactions between each of the social category variables with each other. There was a significant interaction between charge severity, age, and race ( $\gamma_{72} = -0.000$ ,  $t = -2.49$ ,  $p = .013$ ). Older, Black defendants with either A- or B-level charges received the highest bail amounts and younger, White defendants with either A- or B-level charges received the lowest bail amounts in this interaction. Within each charge level older people of both races received higher bail amounts than younger people and, however, older people with B-level charges received higher bail than younger people with A-level charges. Further, Black people received higher bail amounts than White people but again, Black people with B-level charges received higher bail amounts than White people with A-level charges.

There was also a significant interaction between booking number, age, and gender ( $\gamma_{51} = 0.001$ ,  $t = 2.39$ ,  $p = .017$ ). Bail amount increased as the number of bookings increased at all ages and for both genders. However, bail amount increased slightly faster for older men than for younger men and women of all ages. Finally, there was a significant, three-way interaction between age, gender, and race ( $\gamma_{43} = -0.005$ ,  $t = -3.01$ ,  $p = .003$ ). White women received the lowest bail amounts followed by Black women, White men, and then Black men. Among White women, Black women, and White men, older defendants received slightly higher amounts than younger defendants. However, this pattern was reversed among Black men, with younger defendants receiving higher bail than older defendants (see Figure 1). The remaining three-way interactions were not significant (see Table 2).

## Discussion

In this third wave of bail reform, law makers, advocates, and researchers are challenged to develop, implement, and evaluate procedures that reduce bias in bail decisions. Yet, our understanding of bias in bail is still limited to primarily felony defendants (Freiburger & Hilinski, 2010; Wooldredge, et al., 2015; Wooldredge, 2012), many of which come from the same dataset compiled by the State Court Processing Statistics program (Ball & Bostaph, 2009; Demuth, 2003; Demuth & Steffensmeier, 2004; Pinchevsky & Steiner, 2016; Schlesinger, 2005). Additionally, only a handful of studies have examined bail decisions across intersecting social categories (Demuth & Steffensmeier, 2004; Freiburger & Hilinski, 2010; Pinchevsky & Steiner, 2016; Wooldredge, 2012; Wooldredge, et al., 2015). To help expand this literature, we examined how bail amounts vary across legal factors and social categories in a large heterogeneous sample of defendants. Overall, our findings demonstrate that bail amounts are often assigned in ways that reflect biases at the intersections of race, gender, and age.

### Summary of Findings

Consistent with prior research (Pinchevsky & Steiner, 2016; Schlesinger, 2005), all three legally-relevant variables demonstrated significant effects on bail amounts in the expected and legally appropriate directions. Specifically, bail amount increases as number of bookings, charge count, and charge severity increase. The influence of number of charges and charge severity on bail amount is consistent with the Bail Reform Act of 1984, which guides judges to consider factors such as the nature and seriousness of the offense (Spohn, 2009; Bail Reform Act of 1984, 1984). However, number of charges and charge severity *should* have the same impact on bail amounts for all defendants, which was not the case in the current study. Instead, we found that all

three legally-relevant variables interacted with race, gender, and age to affect bail amounts. These interactions will be discussed in further detail later.

Our finding that bail amount increased as a defendant's number of bookings increased may or may not be consistent with the Bail Reform Act of 1984. Under this act, judges are permitted to consider past conduct and criminal history, but it is not clear whether this only includes prior convictions or if it also includes prior arrests (Bail Reform Act of 1984, 1984). We measured bookings into jail and many people who are booked into county jails subsequently have their charges dropped or are found not guilty. So, the increases in bail amounts seen across repeated bookings do not necessarily reflect increased bail due to *actual* increases criminal histories, but rather increased likelihood of being arrested. Further examination is needed to determine whether bail increases across arrests in addition to increasing across convictions. If bail does increase across arrests, policymakers must specify in future written reform that prior arrests cannot be considered in the setting of pretrial conditions, including bail. Otherwise, the systemic bias against some groups of defendants, such as Black defendants who are more likely to be arrested (Kochel, et al., 2011), will continue to be perpetuated in bail practices.

With respect to race, we failed to find a main effect of race on bail amount. At first glance, this finding appears to be in contrast with the narrative regarding racial bias that is driving widespread bail reform efforts. However, prior research has similarly been mixed regarding a main effect of race on bail amounts (Arnold, et al., 2018; Demuth, 2003; Freiburger, et al., 2010; Wooldridge, 2012; Wooldridge et al., 2015; McIntyre & Baradaran 2013). However, we did find two significant three-way interaction effects that included race. The first three-way interaction between age, gender, and race, revealed that overall, White women received the lowest bail followed by Black women, White men, and then Black men. However, older women,

regardless of race, and older *White* men received slightly higher bail amounts than their younger counterparts, while young, Black men received *higher* bail than older Black men. The complexity of this interaction and reversal of the impact of age for Black men compared to other groups highlights the importance of examining bail using an approach that considers the simultaneous influence of multiple social categories.

Ours is not the only study to consider interactions between race and other social identity, and demographic variables (Wooldridge, 2012; Wooldridge et al., 2015). Across two studies, Wooldridge and colleagues (2015) similarly found that young, Black men experience the worst bail outcomes, overall. Research examining the 3-way interaction of race, gender, and age in relation to other criminal justice decisions, namely sentencing, also shows that young, Black men experience the worst outcomes (Steffensmeier, et al., 2017; Steffensmeier, et al., 1998). In sum, our findings add to a body of literature demonstrating complex biases that exist at the nexus of social identities and demographic characteristics in the criminal justice system.

While the effect size for the 3-way interaction between age, gender, and race may seem small, our analysis was conducted on a logarithmic scale. The actual differences in bail amounts for each subgroup are meaningful (see Table 1). For example, the average bail amount assigned to older, Black women was \$550 more than the average amount assigned to older, White women. Even the smaller difference of \$56 between the average bail assigned to younger, Black women versus younger, White women could mean the difference between freedom and detention. Black women who have been incarcerated have been estimated to earn a median monthly income of just \$1,064<sup>1</sup>, or \$266 per week prior to that incarceration (Rabuy & Kopf, 2015). So, what appears to be a small effect size represents a difference that is just over 20% of younger, Black

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<sup>1</sup> Median monthly incomes were reported from 2004 in Rabuy & Kopf (2015) and have been inflation-adjusted to represent 2014 values since these bail amounts range from January 2013-June 2016.



women's weekly income and almost two weeks of older, Black women's weekly income. Even more striking is the \$8,985 difference in average bail amounts between young, White men, who are assigned an average bail of \$21,432 and young, Black men who are assigned an average bail of \$30,417. Considering that Black men who have been incarcerated earn a median monthly income of \$1,473, this average difference represents six full months of income. All of the differences in bail reported here represent differences in the *average* bail assigned to each group. Thus, in some cases, these differences in assigned bail are larger and the disparity is greater between Black and White women and Black and White men.

Clarifying the complex biases that exist in bail assignments is critical to developing bail reform efforts that will reduce, rather than exacerbate, bias. Yet, legal policies and social justice movements alike tend to focus on one dimension of the populations they serve, often to the detriment of subgroups within that population (Cho, Crenshaw, & McCall, 2013). For example, pretrial risk assessments are being adopted across the country as part of bail reform and much of the discussion around their use has been focused on whether they exacerbate racial disparities (Angwin et al., 2019; Barabas et al., 2017; Mayson, 2018; Pretrial Justice Institute, 2020). To be sure, this discussion is important and sorely needed. Tools that exacerbate racial bias cannot be used in efforts to reform a racially biased system. Notably, studies have not found strong evidence of racial bias in existing pretrial risk assessment tools (Desmarais et al., in press). However, this singular focus on racial bias has resulted in research that is limited to examinations of the main effects of race on different aspects of a tool's performance. Three studies have examined whether implementation of a pretrial risk assessment tool reduces racial disparities in nonfinancial release rates (Redcross et al., 2019; Schaefer & Hughes, 2019; Stevenson, 2018) and the findings of these studies are mixed; perhaps in part because the use of

risk assessment tools has varying impacts on subgroups within racial groups (e.g., Black women vs Black men). Indeed, our findings, along with those of Wooldridge and colleagues (2015), indicate the importance of considering multiple social categories simultaneously. Of course, it will not always be possible to include many different social categories and in those cases, it is better to at least examine the influence of race. However, as bail reform efforts are implemented, we must strive to evaluate their effect on intersecting social categories to determine whether these efforts are truly reducing the disparities that currently exist.

We found another three-way interaction between charge severity, age, and race. This interaction revealed that, older and Black defendants with B-level charges received higher bail than younger and White defendants with A-level charges. This finding indicates that older and Black defendants received higher bail amounts for charges such as misdemeanor drug possession than younger and White defendants received for charges such as felony assault. Prior studies at other stages of the criminal justice process, have found greater evidence of racial bias when charge severity is low and less evidence of racial bias when charge severity is high (Chen, 2008; Hester & Hartman, 2017; Mitchell & MacKenzie, 2004). Judicial decision makers are seemingly more influenced by bias when determining outcomes for less serious charges, perhaps because there is more room for discretion in setting outcomes (Mitchell & MacKenzie, 2004). Our finding of bias for less severe charge levels differs from prior research (Ball, 2006; Warren et al. 2012) indicating that context and other social identity or demographic characteristics can influence the impact of charge severity on bail amount.

The 3-way interaction between charge severity, age, and race highlights the role that heuristic principles and racialized crime scripts (Harris et al., 2011; Tversky & Kahneman, 1974) may play in determining the amount of bail an individual is assigned. Specifically, judicial

decision makers seem to determine that Black defendants require higher bail than White defendants to assure their appearance in court. Alternatively, judicial decision makers may view Black defendants as greater threats to public safety (implicitly or explicitly) and use high bail amounts as a way to assure that Black defendants remain in pretrial detention. Older, Black men receiving higher bail amounts than young, Black men at both charge levels is a reversal from the previous 3-way interaction involving race. It is also out of line with the narrative that judicial decision makers view young, Black men as being particularly dangerous (Steffensmeier et al., 1998). This discrepancy indicates that the relationship between race, age, charge severity and bail may be even more complicated. Perhaps other social categories, like socioeconomic status, are also influencing the relationship between race, age, charge severity, and bail amount. Overall, this interaction suggests the need for clarification of bail guidelines. Defendants with less serious charges should not be receiving higher bail than those with serious charges. Existing guidelines may need amending or rewriting to clearly state that bail amount should be commensurate with charge severity. Additionally, increased oversight of adherence to guidelines may be warranted. Judicial decision makers have a great deal of discretion when it comes to setting bail (Digard & Swavola, 2019; Schlesinger, 2005) and they make these decisions rapidly and with little supporting information (Worden et al., 2019). Providing clear guidelines and establishing a mechanism for ensuring these guidelines are followed could help to correct situations in which defendants with low severity charges are being assigned disproportionately high bail amounts.

We found one other significant three-way interaction that did not involve race, but rather gender, age, and booking number. Specifically, this interaction revealed that bail amount increases for men and women of all ages over repeated bookings; however, older men experience a slightly more rapid increase in bail amount across repeated bookings than other age and gender

combinations. This is counter to prior studies that have found evidence of leniency for older defendants. For example, older defendants are more likely to be granted release on recognizance and less likely to be denied bail (Demuth, 2003; Pinchevsky & Steiner, 2016). However, the finding that bail amount increases the same for women of all ages is in line with prior studies which have found evidence that differences by age, for pretrial and sentencing outcomes, are stronger for male defendants or offenders than for their female counterparts (Freiburger & Hilinski, 2013; Freiburger & Hilinski, 2010; Pinchevsky & Steiner, 2016). Notably, this study looks at bail across repeated bookings where the studies mentioned above all use a cross-sectional design. It may be true that the longitudinal nature of this analysis reveals an increased punitiveness toward older defendants not seen in cross-sectional designs. This finding emphasizes the need for further examination of patterns in bail assignment over repeated bookings. The purpose of bail is to ensure a defendant returns to court for subsequent hearings (Adair, 2006). Thus, the only amount of bail that should be assigned is an amount that a defendant is capable of paying and that will incentivize them to return to court (Stack v. Boyle, 1951). It is not appropriate to assign increasingly higher bail as a punitive measure against a defendant across repeated bookings unless those bookings resulted in convictions. If bail is increasing across repeated bookings in the absence of convictions, greater oversight into bail setting practices is needed.

Finally, one last two-way interaction between gender and legal variables merits discussion. We found an interaction between gender and charge count which showed that men were assigned higher bail amounts than women when the number of charges associated with each booking was low; however, as the number of charges increased, women began to receive higher bail amounts. In fact, women who had a high number of charges associated with a given

booking received higher bail amounts than men charged with a similarly high number of charges. These women may be treated more harshly because their behavior is seen as unexpected and therefore problematic (Chesney-Lind, 1987; Kruttschnitt, 1981; Pinchevsky & Steiner, 2016). Indeed, this punitive reaction to women who fall outside of gender norms has been demonstrated in other pretrial outcomes (Pinchevsky & Steiner, 2016). Judicial decision makers may set a higher bail because they perceive women with a high number of charges to be less likely to return for their court dates or greater threats to public safety. Alternatively, there has been recent speculation that bail is sometimes used as a form of punishment (Johnson, 2019), though this is not an appropriate use of bail, and judicial decision makers may assign a high bail amount to women they believe to be deserving of a punitive outcome. While women generally receive lower bail amounts than men, even over repeated bookings, there may be instances where women are at risk for unfairly receiving higher bail.

### **Limitations**

A few limitations of the study design should be noted. First, we were unable to separate out defendants who were identified as Hispanic because ethnicity was not consistently documented by the jail staff. Unfortunately, this is a common limitation of criminal justice research that uses administrative data as only 15 states report ethnicity in arrest records (Eppler-Epstein et al., 2016). Our findings are important because they provide a more nuanced look at Black/White disparities than most prior research and Black people are the most overrepresented racial or ethnic minority group in US correctional populations (Wagner, 2012). Further, defendants who are Hispanic or Latinx are most likely identified as White meaning that, if they were correctly identified, the White/Black disparities demonstrated here would likely be even larger (Eppler-Epstein et al., 2016). Similarly, we excluded Asian and Native American defendants from

analyses due to very small sample sizes. The degree to which there is bias in bail amounts across these racial and ethnic identities must be examined by future work. Second, we were limited to an examination of gender as a binary category (men, women) that was recorded by jail staff, presumably based upon defendants' appearances. Again, this is similar to the methods of establishing gender in most prior studies of pretrial decisions and outcomes. However, research indicates that gender minorities experience discrimination at other points within the justice system (James et al., 2016). Future research must examine how gender minorities are impacted during the pretrial period as well. Third, we did not have access to information about defendants' criminal history prior to the study period and were therefore unable to control for criminal history. However, our study was longitudinal, covering three and a half years, and many defendants in our sample were booked multiple times during this period. In this way, we incorporated some measure of criminal history. Further, research indicates that judges place varying degrees of emphasis on the importance of criminal history when setting bail. In fact, some studies have found criminal history to be a weak predictor of bail decisions (Ebbesen & Konecni, 1975) while others have found the nature of the current charge to be a strong predictor (Beatty et al., 2014; Goldkamp & Gottfredson, 1979). Finally, we investigated bail amounts assigned to defendants in one jurisdiction; generalizability of findings, particularly as bail reforms are implemented, must be examined in future research.

Despite these limitations, this study also has many strengths. This study is one of only a handful to examine the effects of simultaneous social categories on bail amounts including up to 3-way interactions (Demuth & Steffensmeier, 2004; Freiburger & Hilinski, 2010; Pinchevsky & Steiner, 2016; Wooldredge, 2012; Wooldredge, et al., 2015). Studies that examine the impact of race on bail and other pretrial decisions have mixed findings in part because interactions between

multiple social categories are not always considered. Interactional studies can help to clarify these mixed findings with a more nuanced examination of exactly who is affected and how. Furthermore, to our knowledge, this is the first study to examine changes in bail amounts across repeated bookings over a three-and-a-half-year period. In this way, findings likely speak to systemic patterns and biases, rather than being reflective of particularly problematic decision makers or an unusual period in time.

### **Conclusion**

Taken together, the findings in this study highlight the fact that legal factors – namely, booking number, number of charges, and charge severity – do not affect judicial decisions regarding bail amount in the same way for all defendants. Rather, the effect of legal factors is moderated by characteristics of the defendant, including their race, gender, and age. Furthermore, bail is not assigned equally across these social categories. Specific groups, most notably young, Black men in the current study, experience harsher bail assignments than other groups. Failure to consider the multiple, intersecting identities that contribute to disparities in bail amounts obscures identification of the underlying inequities in bail assignments and may hamper the effectiveness of bail reform. Indeed, reform efforts that aim to reduce discriminatory practices against larger groups defined by a single marker, such as race, may not achieve the reform goal of promoting equitable, non-carceral, bail practices (Mason, 2010). Instead, such a narrow approach may actually contribute to the persistence of discriminatory bail practices within certain groups of people who experience multiple forms of oppression (Crenshaw, 2016). Researchers, policymakers, and advocates, alike, must consider the ways in which simultaneous social categories influence reform outcomes at local, state, and national levels.

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**Table 1.** Descriptive Statistics for Bail Amounts Assigned to Subgroups

<b>Subgroup</b>	<b><i>n</i> (%)</b>	<b>Mean (SD)</b>	<b>Range</b>
Younger White women	1677 (5.2%)	\$12,172 (\$44,017)	\$50 - \$1,060,000
Younger Black women	2397 (7.5%)	\$12,228 (\$46,179)	\$25 - \$1,130,000
Older White women	1320 (4.0%)	\$12,300 (\$42,356)	\$100 - \$750,000
Older Black women	1553 (4.8%)	\$12,950 (\$55,179)	\$100 - \$1,280,000
Younger White men	5277 (16.3%)	\$21,432 (\$136,162)	\$75 - \$4,400,000
Older White men	4216 (13.0%)	\$26,945 (\$153,079)	\$5 - \$4,000,000
Older Black men	6526 (20.1%)	\$27,466 (\$161,575)	\$25 - \$5,100,000
Younger Black men	9433 (29.1%)	\$30,417 (\$169,778)	\$1 - \$7,510,000

*Notes.* *n* = bookings. SD = standard deviation.

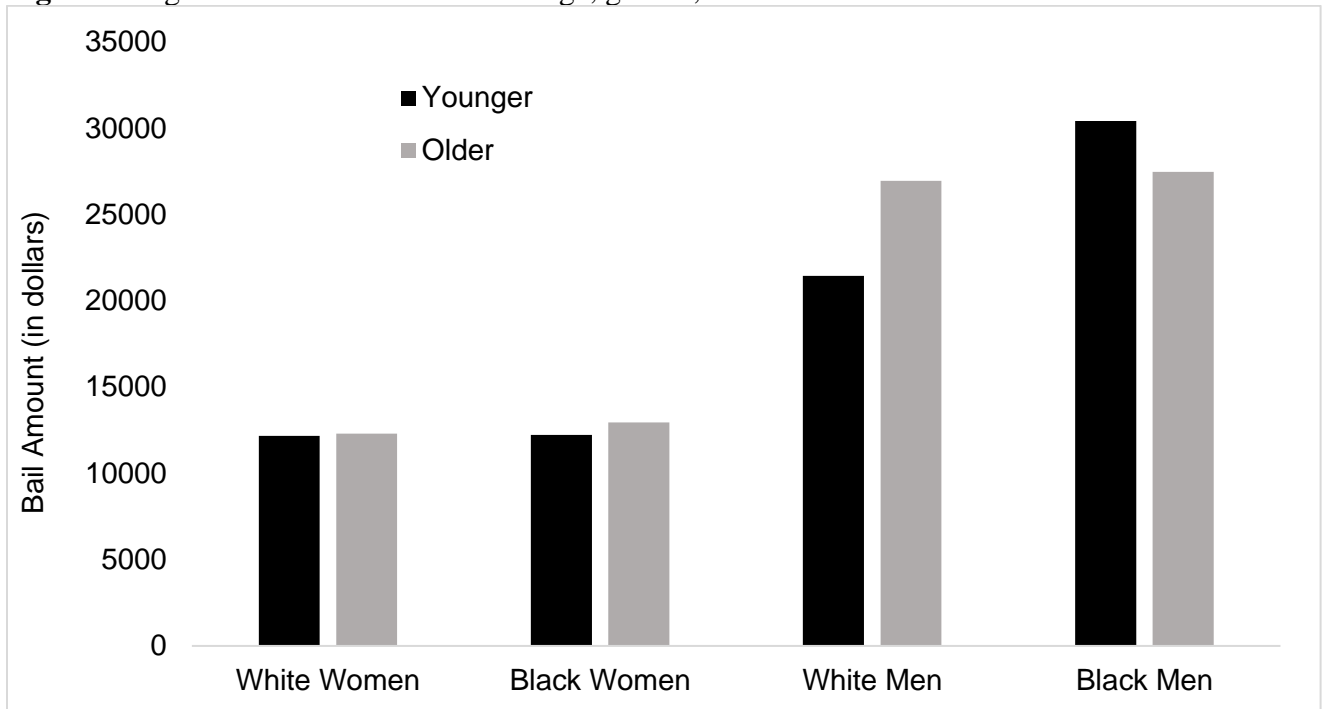
**Table 2.** Results of Main and Interaction Effects on Bail Amount

<b>Variables</b>	<b>Model Estimates</b>			
	<b>Estimate</b>	<b>SE</b>	<b><i>t</i></b>	<b><i>p</i></b>
Booking Number	<b>0.042</b>	0.008	4.93	<.001
Charge Count	<b>0.202</b>	0.009	22.53	<.001
Charge Severity	<b>0.000</b>	0.000	1.98	.048
Age	0.003	0.002	1.71	.086
Race	0.042	0.030	1.39	.164
Gender	<b>0.089</b>	0.027	3.36	.001
Age*Gender	0.000	0.002	0.00	.996
Age*Race	0.001	0.002	0.65	.518
Race*Gender	0.033	0.034	0.95	.341
Charge Count*Age	0.001	0.001	1.10	.270
Charge Severity*Age	0.000	0.000	0.04	.968
Booking Number *Age	<b>-0.004</b>	0.001	-6.01	<.001
Charge Count*Gender	<b>-0.024</b>	0.010	-2.37	.018
Charge Severity*Gender	<b>-0.000</b>	0.000	-2.32	.020
Booking Number*Gender	0.012	0.010	1.22	.224
Charge Count*Race	0.000	0.012	0.03	.977
Charge Severity*Race	-0.000	0.000	-0.76	.450
Booking Number*Race	-0.009	0.011	-0.78	.435
Charge Severity*Race*Gender	0.000	0.000	0.72	0.471
Charge Count* Race*Gender	0.019	0.013	1.44	0.149
Booking Number* Race*Gender	-0.010	0.012	-0.83	0.409
Charge Severity*Age*Race	<b>-0.000</b>	0.000	-2.49	0.013
Charge Count* Age*Race	-0.000	0.001	-0.1	0.919

**Table 2.** (Continued)

Booking Number* Age*Race	0.001	0.000	1.58	0.113
Charge Severity*Age*Gender	0.000	0.000	0.15	0.879
Charge Count* Age*Gender	0.000	0.001	0.22	0.823
Booking Number* Age*Gender	<b>0.001</b>	0.001	2.39	0.017
Age*Race*Gender	<b>-0.004</b>	0.002	-3.01	0.003

*Notes.* Significant effects are bolded.  $N = 24060$ .  $SE$  = standard error.

**Figure 1.** Significant interaction between age, gender, and race

## CHAPTER 4

### **Predictive Validity of Pretrial Risk Assessments: A Systematic Review of the Literature**

Bail reform is sweeping the nation and many jurisdictions are looking to pretrial risk assessment as one potential strategy to support these efforts. This paper summarizes the findings of a systematic review of research examining the predictive validity of pretrial risk assessments. We reviewed 11 studies (13 publications) examining the predictive validity of six pretrial risk assessment instruments reported in the grey and peer-reviewed literature as of December 2018. Findings typically show good to excellent predictive validity. Differences in predictive validity for men and women were mixed and small. When it could be examined, predictive validity was generally comparable across racial/ethnic subgroups; however, three comparisons revealed somewhat lower—but still fair to good—predictive validity for defendants of color than white defendants. Findings suggest that pretrial risk assessments predict pretrial outcomes with acceptable accuracy, but also emphasize the need for continued investigation of predictive validity across gender and racial/ethnic subgroups.

## Introduction

Across the United States, efforts are underway to maximize pretrial release rates while minimizing pretrial misconduct, including failure to appear in court and perpetration of new crime during the pretrial period. Since 2012, every state in the United States has enacted some form of pretrial legal reform (National Conference on State Legislatures, 2018), with many states implementing pretrial risk assessment instruments as part of these efforts. Briefly, pretrial risk assessment instruments are designed to forecast the likelihood of failure to appear in court and/or perpetration of new crime during the pretrial period through consideration of items that have been shown in research to be associated with these outcomes. In this way, pretrial risk assessment instruments may provide some empirical evidence to inform pretrial decisions (Desmarais & Lowder, 2019). A recent scan of pretrial practices across the United States found that approximately two-thirds of surveyed counties reported using a pretrial risk assessment instrument (Pretrial Justice Institute, 2019a). Despite their widespread implementation, pretrial risk assessment is the subject of considerable controversy.

Some of the controversy centers on whether pretrial risk assessment instruments are inherently biased because they are developed using data that reflect biased policing practices that target people of color (Eckhouse et al., 2019; Mayson, 2019). Other critiques reflect beliefs that pretrial risk assessments cannot predict pretrial outcomes, that they increase rates of detention, and that they exacerbate biases in pretrial decision-making (e.g., Barabas et al., 2019; Pretrial Justice Institute, 2020; Scurich & Krauss, 2020). Behind these critiques, at least in part, are analyses of risk assessments completed using a single instrument in a single jurisdiction, including ProPublica's analysis of the COMPAS in Broward County, Florida (Angwin et al., 2016) and a subsequent study that concluded COMPAS assessments performed no better than

lay persons at predicting recidivism (Dressel & Farid, 2018). While these two particular studies have been criticized and rebutted in the academic literature (see, for example, Bansak, 2019; Flores et al., 2016; Holsinger et al., 2018; Lin et al., 2020), they are among the most prominent sources cited in calls to abolish risk assessment instruments from the criminal justice system.

Discussion of bias in risk assessment—specific to pretrial or otherwise—often conflates several issues (Desmarais & Zottola, 2020), a few of which we touch on here. A first issue is whether certain groups of people receive higher risk scores or classifications than others. A second issue is whether certain groups have higher rates of criminal behavior than others (actual or as the result of unfair police practices). A third issue is whether some groups are over classified at higher risk levels and under classified at lower risk levels relative to their actual rates of criminal behavior. It is this third issue that speaks to predictive validity. In particular, pretrial risk assessments should be able to forecast pretrial outcomes for different racial/ethnic groups with comparable predictive validity, regardless of the base rate of offending in each group (Lowder et al., 2019). To our knowledge, there has been no effort to date that examines and compares the predictive validity of pretrial risk assessments as a function of race/ethnicity, or gender for that matter, across instruments and outcomes. We sought to address this gap.

### **Pretrial Risk Assessment Instruments**

Pretrial risk assessment instruments were designed as a strategy to help overcome some of the limitations of human decision-making that may contribute to biased and unfair decisions in the pretrial context (Desmarais & Lowder, 2019). It is well accepted that human judgment is influenced by our personal beliefs and that increasing structure can reduce reliance on heuristics in the decision-making process, thereby producing less biased and more accurate decisions (Tversky & Kahneman, 1974). Consequently, increasing structure through the use of risk



assessment instruments may contribute to more accurate and less biased pretrial decisions. Indeed, meta-analyses of research conducted over the past 60 years show that predictions of future behavior completed using statistical methods produce more accurate assessments of future violent and criminal behavior compared to unstructured human judgments (Ægisdóttir et al., 2006; Grove, Zald, Lebow, Snitz, & Nelson, 2000). While a similar body of work does not exist in the pretrial context specifically, findings of these studies suggest that we may anticipate such gains in the accuracy of predictions regarding future behavior during the pretrial period, as well.

The first pretrial-specific risk assessment instrument, the Vera Point Scale, was created and adopted in New York City more than 50 years ago (Ares et al., 1963). Since then, additional pretrial risk assessment instruments have been introduced, some of which we highlight here. Several pretrial risk assessment instruments were designed for use in a specific state and then adopted by other jurisdictions, such as the Virginia Pretrial Risk Assessment Instrument (VPRAI; VanNostrand, 2003) and the Ohio Risk Assessment System-Pretrial Assessment Tool (ORAS-PAT; Latessa et al., 2009). Another pretrial risk assessment instrument, the U.S. Federal Pretrial Services Risk Assessment Instrument (PTRA; Lowenkamp & Whetzel, 2009), was developed for use with individuals charged with federal offenses. Most recently, the Public Safety Assessment (PSA; VanNostrand & Lowenkamp, 2013) was created as a publicly available pretrial risk assessment instrument. The PSA is now used statewide in Arizona, Kentucky, Utah, Rhode Island, and New Jersey, as well as in major cities (e.g., Chicago, Houston, Phoenix, and Los Angeles) and smaller jurisdictions across the United States (Laura and John Arnold Foundation, 2020). Many other pretrial risk assessment instruments have been developed locally without being implemented as widely (Myburgh et al., 2015).

Pretrial risk assessment instruments are primarily comprised of risk factors (i.e.,

characteristics of a defendant or their environment that may increase likelihood of pretrial failure). Pretrial risk assessment instruments also may include protective factors (i.e., characteristics that may mitigate the likelihood of pretrial failure; Monahan & Skeem, 2016). Both risk and protective factors may be static (i.e., historical or otherwise unchangeable) or dynamic (i.e., able to change; Douglas & Skeem, 2005). Pretrial risk assessment instruments typically use an actuarial approach; that is, numeric item ratings are weighted and combined into a total score that is cross-referenced with a table describing outcome rates or probabilities. In practice, pretrial risk assessments should be used to inform but not replace judicial discretion (Desmarais & Lowder, 2019). *Wisconsin v. Eric Loomis* (2016) asserts that scores produced by risk assessment instruments may not be the determinative factor in decisions of release.

### **Predictive Validity of Pretrial Risk Assessments**

The landscape of pretrial risk assessment practice and policy has changed dramatically in recent years, as jurisdictions across the nation have ramped up their pretrial reform efforts (Pretrial Justice Institute, 2019a). While other fields have experienced long delays from research and development of new technologies to adoption in practice (Green et al., 2009), the implementation of pretrial risk assessment instruments has outpaced the peer-reviewed research in many ways. Peer-reviewed studies examining predictive validity in the context of pretrial risk assessment are relatively few and far between (Desmarais & Lowder, 2019). Instead, the body of research largely reflects the findings of local validation efforts disseminated in reports to government, non-profits, and other outlets, including mass media. Over the past 10 years, a few systematic reviews and meta-analyses have summarized this work and set the stage for the current review (Mamalian, 2011; Myburgh et al., 2015; Bechtel et al., 2011; Bechtel et al., 2017). In the sections that follow, we briefly review the findings of the two meta-analyses,

specifically, highlighting the aspects of these efforts upon which we seek to expand.

The first meta-analysis was conducted about a decade ago (Bechtel et al., 2011) and examined associations of risk factors and risk assessments with pretrial outcomes, including failure to appear, re-arrest, new crime, and any pretrial failure (i.e., one or more of the individual measures), across 13 studies. In general, the strength of association of individual factors with pretrial outcomes was low but tended to be higher for static than dynamic factors. Global estimated effect sizes showed that pretrial risk assessments, in aggregate, demonstrated correlations moderate in size with re-arrest, failure to appear, and any pretrial failure, but not with new crime. Predictive validity was not reported by instrument. More recently, Bechtel and colleagues (2017) examined correlations between pretrial risk scores and failure to appear, pretrial arrest, or a composite measure of pretrial failure across 16 studies of various instruments. Analyses revealed a mean effect size representing “fair” validity for failure to appear and “good” validity in relation to re-arrest and any pretrial failure. While the authors reported and compared effect sizes for each study, information on predictive validity by instrument was not provided.

The findings of the two meta-analytic investigations advanced the scientific literature addressing the degree to which pretrial risk assessments, and their components, are associated with pretrial outcomes; however, gaps remain that merit an updated review and synthesis of the research. First, as noted earlier, the pretrial risk assessment landscape has changed in recent years, including the implementation of the PSA in dozens of jurisdictions across the United States (VanNostrand & Lowenkamp, 2013). At the time of Bechtel and colleagues’ most recent meta-analysis, only one study examining the predictive validity of PSA assessments was available for analysis. Second, these meta-analyses reported results in aggregate across instruments and defendants to present an overall state of the science, rather than examining and

comparing predictive validity by instrument or within specific subgroups. As such, our understanding of the empirical evidence regarding predictive validity of pretrial risk assessments for individual instruments and within subgroups of defendants, most notably those defined by race/ethnicity and gender, is limited. Third, the authors examined correlations, which measure the direction and strength of association, but are greatly constrained by base rates.<sup>1</sup> Fourth, prior reviews have not examined validity of pretrial risk assessments in predicting new violent crime, which is arguably the most relevant outcome for informing pretrial decisions. In fact, statutes and guidelines generally emphasize public safety over flight risk and interpret threat to public safety narrowly as reflecting new violent criminal activity (as opposed to any new criminal activity; *United States v. Salerno*, 1987; American Bar Association, 2007). For these four reasons, a re-examination and synthesis of the empirical evidence is due.

### **The Current Review**

The aims of the current review are to: 1) describe pretrial risk assessment instruments used in jurisdictions across the United States; 2) summarize the characteristics of studies that have examined their predictive validity; and 3) synthesize findings regarding their validity in predicting failure to appear in court, new criminal activity, new violent criminal activity, and technical violations during the pretrial period overall and for subgroups of defendants. Our goal is to provide a summary of the empirical evidence regarding predictive validity that not only adds to the scientific literature, but also informs decisions regarding the selection and implementation of a pretrial risk assessment instrument, if any, to support pretrial reform efforts.

## **Method**

### **Review Protocol**

We followed the preferred reporting items for systematic review and meta-analysis

protocols (PRISMA-P) statement (Moher et al., 2015), a 17-item checklist to promote transparent and consistent reporting of our systematic review protocol and findings.

### **Search Strategy**

**Identification of pretrial risk assessment instruments.** We identified risk assessment instruments used to predict the likelihood of failure to appear in court, new crime involvement, new violent crime involvement, and technical violations during the pretrial period. We searched PsycINFO, PsycArticles, Web of Science, National Criminal Justice Reference Service Abstracts, ProQuest Dissertation & Theses electronic databases, Google Scholar, and Google using all possible combinations of the following keywords: “pretrial”, “risk assessment”, “risk assessment instrument”, “risk assessment tool”, “failure to appear”, “crime”, “criminal activity”, “bail”, “bond”, and “defendant”. We also examined the references of papers, reports, reviews, and online resources that we identified through our database searches (Bechtel et al., 2011, 2017; Bureau of Justice Assistance, n.d.; Mamalian, 2011; Myburgh et al., 2015; Pretrial Justice Institute, 2019b; Summers & Willis, 2010) and consulted with experts in the field. We limited our search to instruments whose source material (e.g., manual, calibration study) had been produced by December 31, 2018. We included instruments if they were: a) designed to predict outcomes during the pretrial period (i.e., failure to appear, new criminal activity, new violent criminal activity, and/or technical violation); and b) used in multiple jurisdictions in the United States. We identified 10 pretrial risk assessment instruments that met inclusion criteria:

1. Colorado Pretrial Assessment Tool (CPAT; Pretrial Justice Institute, 2012);
2. Connecticut Risk Assessment for Pretrial Decision Making (Connecticut Decision Aid; Hedlund et al., 2005);
3. Correctional Offender Management Profile for Alternative Sanctions – Pretrial

- Release Risk Scale (COMPAS-PRRS; Dieterich, 2010);
4. Florida Pretrial Risk Assessment Instrument (FPRAI; Austin et al., 2011);
  5. Indiana Risk Assessment System – Pretrial Assessment Tool (IRAS-PAT; Latessa et al., 2013);
  6. Ohio Risk Assessment System – Pretrial Assessment Tool (ORAS-PAT; Latessa et al., 2009);
  7. Public Safety Assessment (PSA; VanNostrand & Lowenkamp, 2013);
  8. U.S. Federal Pretrial Services Risk Assessment Instrument (PTRA; Lowenkamp & Wetzel, 2009);
  9. Vera Point Scale (Ares et al., 1964); and
  10. Virginia Pretrial Risk Assessment Instrument (VPRAI; VanNostrand, 2003).

**Identification of predictive validity studies.** Our next step was to identify studies that examined the predictive validity of these 10 instruments. We used the same search databases and sources listed above, this time using the full names and acronyms of the instruments as keywords. Again, we limited our search to studies that had been conducted by December 31, 2018. We included studies if they: b) examined validity in predicting pretrial outcomes (i.e., failure to appear, new criminal activity, new violent criminal activity, and/or technical violation); b) were reported in peer-reviewed journals, dissertations, theses, conference presentations, government or other reports available online, or book chapters; and c) were written in English or a reliable translation was available. In the case of overlapping samples, we included unique analyses once for each sample. If samples overlapped and the same analytic technique was applied, we included the predictive validity estimate from the sample with the most participants.

We conducted our search in January 2019, which returned an initial total of 1,998,701

hits. We reviewed 32,398 records<sup>2</sup> over the next several months to arrive at a final count of 11 studies across 13 publications<sup>3</sup> (marked with an asterisk in the reference list), including three journal articles and 10 government/technical reports (see figure). Predictive validity was examined in one study each for COMPAS-PRRS, CPAT, and ORAS-PAT assessments and two or more studies each for PSA, PTRA, and VPRAI assessments. Across studies, data were collected between 1998 and 2016. The unit of analysis was at the person level for five studies, at the case level for four, and unclear for two.<sup>4</sup> Our search did not identify any studies of predictive validity that met our inclusion criteria and reported the necessary information for four instruments: the Connecticut Decision Aid, FPRAI, IRAS-PAT, and Vera Point Scale. These four instruments are excluded from the remainder of this review.

**Data extraction.** Two of members of the research team extracted the following information from each study using a standardized data extraction protocol developed for this project (available upon request): 1) demographics of the study samples (e.g., sample size, gender, race/ethnicity, age) study design characteristics (e.g., study source, tool authorship, duplicate sample, research or practice context, length of follow-up), and the assessment process (e.g., setting, format, assessor, sources of information used to fill out assessment); 2) characteristics of the risk assessment instruments (e.g., assessment approach, number of items, types of items, and predicted outcome); and 3) predictive validity estimates or the information needed to calculate them (e.g., frequencies, failure rates) overall and as a function of gender and race/ethnicity, when reported. Table 1 provides full details on all variables for which data were extracted. There was an acceptable level of inter-rater reliability ( $\kappa = 0.79$ ) for all studies coded by two researchers. We settled coding disagreements through discussion with the research team.

We followed the Risk Assessment Guidelines for Evaluation of Efficacy (RAGEE; Singh

et al., 2015), a 50-item checklist detailing the information that should be included in papers reporting on predictive validity of risk assessments, to describe study quality. We elected to use the RAGEE over other measures for two reasons. First, the RAGEE was developed and validated through a Delphi process and reflects expert consensus regarding information that should be reported in papers describing research on the predictive validity of risk assessments (Singh et al., 2015). Second, recent methodological discussions have emphasized reporting study design features that may affect the interpretation of findings rather than reporting a particular scale value or rating (Hohn et al., 2019; Widman et al., 2020).

**Data analysis.** We summarized sample, study design, and instrument characteristics using measures of central tendency, when appropriate. We then summarized the item type and content of the pretrial risk assessment instruments included in our review. Next, we extracted frequencies and predictive validity estimates for total scores and risk levels across instruments, where possible. Predictive validity was assessed for failure to appear, new criminal activity, new violent criminal activity, and technical violations, as well as combinations of these outcomes. Extracted estimates included the area under the receiver operating characteristic curve (AUC), correlation coefficient ( $r$ ), the odds ratio (OR), and the Dispersion Index for Risk (DIF-R), which were the indices most commonly reported. Briefly, AUC represents the probability that a randomly selected defendant who engaged in one of the pretrial outcomes would have received a higher risk rating than a randomly selected defendant who experienced pretrial success;  $r$  represents the direction and strength of association between risk rating and pretrial failure; OR represents the ratio of the odds of a lower risk rating in those who experienced pretrial success to the odds of a higher risk rating in those who experienced pretrial failure; and DIF-R represents the extent to which risk classifications produce reasonably sized groups of defendants with



maximally different rates of pretrial failure (Singh, 2013; Silver et al., 2000).

We first examined predictive validity of total scores and risk levels by calculating, when necessary, and comparing AUC values. The AUC is a suitable metric because its values are not influenced by base rates, which are anticipated to differ across groups and studies, to the same degree as other effect sizes (e.g., correlations; Smith, 1996). Moreover, AUCs are the most commonly reported effect size measure both in studies included herein and in risk assessment research more generally (Singh et al., 2013). For three studies, AUC values were not reported but could be calculated from correlations and chi-square values (see Rice & Harris, 2005). We imputed missing standard error values from confidence intervals and *p*-values (Higgins & Green, 2011). Although DIF-R has gained in popularity as a measure of predictive validity, it was only reported in two studies we identified. Further, there are no formulae for estimating its standard errors and its values are dependent upon base rates and sample size. For these reasons, we excluded DIF-R from further review.

We also examined predictive validity of the risk levels via proportional odds ratios, which represents a strategy for pooling and comparing heterogeneous odds ratios across studies of multiple tests that used different cut-offs for response categories (Siadaty & Shu, 2004). Briefly, the proportional odds ratios assume that the effect associated with each dichotomization of an ordered categorical variable, such as risk levels, is the same; for example, that the increased likelihood of pretrial failure from low to moderate/high risk is the same as from low/moderate to high risk. First, we calculated odds ratios based upon the risk level, outcome frequencies, and sample size data extracted from each study. Second, we collapsed the number of risk levels to three (Singh et al., 2011).<sup>5</sup> Third, we ran two logistic regressions for each instrument and pretrial outcome to examine the assumption of proportionality. In the first logistic regression we

compared risk level 1 (i.e., “low” risk) to levels 2 and 3 (i.e., “moderate” and “high” risk). In the second logistic regression we compared risk levels 1 and 2 (i.e., “low” and “moderate” risk) vs 3 (i.e., high “risk”). Visual examination and statistical comparison of meta-analyzed odds ratios for each study and pretrial outcome indicated that the assumption of proportionality was met in all cases. For the fourth and final step, we calculated proportional odds models examining the association between the 3-level risk variables for each instrument and each pretrial outcome. (Data and results available upon request.)

When these extraction and computational steps resulted in multiple AUCs or proportional odds for an outcome, instrument, or subgroup of interest, we computed an estimate of the average effect size using random effects model (Riley et al., 2011). When multiple effect sizes were reported in a study, we averaged the effects at the study level prior to meta-analysis to remove bias from correlated outcomes (Cooper, 1998). Due to the small number of studies, we do not statistically compare AUCs or proportional odds across outcomes, instruments, or subgroups. Instead, we describe effect sizes in relation to their practical significance, or more accurately, interpretation of the strength of association between the pretrial risk assessments and outcomes. Anchored to Cohen’s *d* (1988), AUC values < .55 were considered poor, .55-.63 fair, .64 - .71 good, and .71 - 1.00 excellent (Rice & Harris, 2005). Based upon the calculations of Chen and colleagues (2010), proportional odds ratios < 1.50 were considered poor, 1.50 – 2.99 fair, 3.00 – 4.99 good, and 5.00 or greater excellent.

## **Results**

### **Characteristics and Content of Instruments**

Most instruments were relatively short, ranging in length from seven items in the ORAS-PAT to 14 items in the COMPAS-PRRS, with an average of 10.17 (*SD* =2.64) items per

instrument. The type and content of items included in the six pretrial risk assessment instruments are summarized in the first online supplemental table. All instruments were comprised of risk factors, both static and dynamic, to the exclusion of protective factors. All instruments included items that represented at least three of four possible content domains (i.e., demographic, personal/social, criminal justice, and clinical). The COMPAS-PRRS and the PTRA included items across all content domains. All instruments included items assessing some form of criminal history, though the operational definitions varied. The CPAT was the only instrument that did not include an item assessing history of failure to appear. Four of the six instruments included items that queried employment status; such items were not included in the CPAT or the PSA. In fact, the PSA did not include items assessing personal/social or clinical characteristics. The CPAT, ORAS-PAT, and VPRAI did not include items assessing demographic characteristics.

### **Study and Sample Characteristics**

Aggregate summaries of the characteristics of the studies and samples included in our review can be found in Table 1. Further information on each study is available in the second online supplemental table. Pretrial risk assessments included in our review were completed by professionals, such as pretrial service staff, in five studies and by research staff in two studies; in the other four studies it was not clear who had completed the assessments. Information used to complete the assessments was typically obtained via a combination of review of official or administrative records, interviews with defendant, and interviews with others. Most studies were conducted by the authors of the pretrial risk assessment instrument under investigation. For four instruments—COMPAS-PRRS, CPAT, ORAS-PAT, and PTRA—all studies were completed by an author of the instrument. Only two instruments—PSA and VPRAI—were examined in studies completed by researchers who did not create the instrument; however, in both cases, the

developers of the instrument commissioned the research. In just over one-third of the studies, assessments were completed as part of routine practice following implementation; in the other studies, assessments were completed for the purpose of research (see Table 1).

Three studies used a prospective design, while six studies used a retrospective design; the research design was not clearly described in the other two. The average length of follow-up was reported in two studies ( $M = 11.50$  months,  $SD = 0.71$ ). All studies included failure to appear and new arrests during the pretrial period as outcome measures; two studies examined new arrest for a violent crime and five studies included technical violations. The types of offenses included in the operational definition of new arrest included felonies or misdemeanors in four studies; two studies also included traffic offenses. The study of COMPAS-PRRS assessments (Dieterich, 2010) focused on felony offenses, specifically. Ten studies reported frequencies and eight studies reported AUC values, which were the two most frequently reported statistics (see Table 1). Only one type of statistic was reported for the COMPAS-PRRS and CPAT assessments: AUC values and frequencies, respectively. Studies of the other four instruments reported multiple different statistics. On average, studies reported information for 25 of the 50 RAGEE items, ranging from a low of 20 items for studies of PSA (Lowenkamp & VanNostrand, 2013) and PTRA (Lowenkamp & Whetzel, 2009) assessments to a high of 34 items for studies of ORAS-PAT (Latessa et al., 2009) and PTRA (Cohen & Lowenkamp, 2019) assessments.

Across studies, the average analytic sample size was 55,431 ( $SD = 73,376.63$ ; Range = 452- 200,583). When reported, the mean defendant age was 33.22 years ( $SD = 2.39$ ) and the majority of defendants (74.8%) were male. For studies in which information on race/ethnicity was reported, just over half of the assessments were completed on White defendants (59.1%) and close to one-third on Black defendants (31.1%), with 15.4% completed on defendants identified

as Latinx<sup>6</sup> (see Table 1). Outcomes were reported within subsamples for three instruments across four studies: PSA assessments by gender and race (DeMichele et al., 2018); PTRAs assessments by gender, race, and ethnicity (Cohen & Lowenkamp, 2019); and VPRAI assessments by gender, race, and income (Danner et al., 2015, 2016; VanNostrand, 2003).

### **Predictive Validity**

Table 2 presents the AUC values for total and subscale scores predicting pretrial outcomes. AUC values were reported or could be calculated for pretrial risk assessments completed using four instruments: the ORAS-PAT, PSA, PTRAs, and VPRAI. Generally speaking, only one AUC value was available for each instrument and outcome, with the exception of the PTRAs (see Table 2). Across instruments and outcomes, predictive validity was typically good, and sometimes excellent. AUC values ranged from .644 for VPRAI assessments predicting the combined measure of new criminal activity, failure to appear, and/or technical violations to .730 for PTRAs assessments predicting technical violations (see Table 2). PTRAs assessments demonstrated the highest AUC values across all outcomes.

AUC values for total and subscale scores within subgroups defined by gender and race/ethnicity were provided for PSA and PTRAs assessments predicting new criminal activity and new violent criminal activity, as well as for PSA assessments predicting failure to appear. On the whole, predictive validity was comparable across subgroups; even when AUC values differed slightly between groups, they were still indicative of good predictive validity. For instance, AUC values for PSA assessments were slightly higher for men but still good for women in the prediction of new criminal activity (men = .653 vs. women = .637), whereas the opposite was true for PTRAs assessments (men = .670 vs. women = .690). As another example, AUC values for PSA assessments were slightly higher for defendants of color but still good for

white defendants in the prediction of new criminal activity (defendants of color = .659 vs. white defendants = .647), while the opposite was true for PTRAs assessments (defendants of color = .663 vs. white defendants = .675). There were two instances in which the AUC values suggested differing levels of predictive validity, both involving PSA assessments: AUC values for new violent criminal activity and technical violations indicated good predictive validity for white defendants (AUCs = .666 and .655), but fair predictive validity for defendants of color (AUCs = .631 and .612). That said, the absolute differences were quite small: .035 and .039, respectively.

Table 3 presents the AUC values for risk levels predicting pretrial outcomes. AUC values were reported or could be calculated for risk assessments completed using five instruments: the COMPAS-PRRS, ORAS-PAT, PSA, PTRAs, and VPRAI. With the exception of the VPRAI, only one AUC value was available for each instrument and outcome (see Table 3). Across instruments and outcomes, predictive validity varied from fair to excellent, with AUC values ranging from .620 for PSA risk levels predicting the combined measure of new criminal activity and/or failure to appear to .726 for PTRAs risk levels predicting the combined measure of new criminal activity, failure to appear, and/or technical violations. PSA risk levels demonstrated the highest AUC values for new criminal activity and failure to appear, while COMPAS-PRRS risk levels demonstrated the highest AUC value for the combined measure of new criminal activity and/or failure to appear. No AUC values for risk levels were reported or could be calculated for new violent criminal activity.

AUC values for risk levels within subgroups defined by gender and race/ethnicity were available only for VPRAI assessments predicting the combined measure of new criminal activity, failure to appear, and/or technical violations. While the AUC values differed slightly across subgroups, they indicated good predictive validity overall (see Table 3).

Table 4 presents the proportional odds ratios for risk levels predicting pretrial outcomes. Proportional odds ratios could be calculated for risk assessments completed using five instruments (i.e., CPAT, ORAS-PAT, PSA, PTRA, and VPRAI). There were one or two proportional odds ratios for each instrument and outcome (see Table 4). Predictive validity of the risk levels varied from fair to excellent, with proportional odds ratios ranging from 2.267 for VPRAI assessments predicting the combined measure of new criminal activity and/or failure to appear to 5.282 for CPAT assessments predicting failure to appear. CPAT assessments also demonstrated the highest proportional odds ratios for new criminal activity and the combined measure of new criminal activity and/or failure to appear, while PTRA demonstrated the highest proportional odds ratios for new violent criminal activity and the combined measure of new criminal activity, failure to appear, and/or technical violations (see Table 4).

Proportional odds ratios could be calculated within subgroups defined by gender and race/ethnicity for PSA and PTRA risk levels predicting new criminal activity and new violent criminal activity, for PSA risk levels predicting failure to appear, and for VPRAI risk levels predicting any pretrial misconduct (defined both ways). More often than not, the proportional odds ratios suggested comparable predictive validity across subgroups, even if the exact values differed slightly. For example, the proportional odds ratios for VPRAI risk levels were slightly higher for men (2.602) than for women (2.526) in the prediction of the combined measure of new criminal activity and/or failure to appear and/or technical violation, whereas the reverse was found for PSA risk levels predicting new violent criminal activity (men = 2.744 vs. women = 2.938). As another example, the proportional odds ratios for PTRA risk levels were slightly higher for white defendants (2.888) than defendants of color (2.717) in the prediction of new criminal activity, but the absolute value of the difference is very small (i.e., 0.171).

There were a handful of instances in which the proportional odds ratios suggested differing levels of predictive validity as a function of gender and race/ethnicity. Comparing results for male and female defendants, there is no consistent pattern in the directionality of the differences. For example, PTRAs risk levels demonstrated good validity in predicting new criminal activity among women (3.012) and only fair validity in predicting this outcome among men (2.880), but the trend was reversed for the prediction of new violent criminal activity (men = 3.090 vs. women = 2.379). In contrast, when differences were seen in the magnitude of proportional odds ratios for white defendants and defendants of color, they usually demonstrated greater predictive validity among white defendants. An exception to this pattern was seen for the PTRAs risk levels predicting new violent criminal activity for which predictive validity was slightly greater among defendants of color (3.070) than white defendants (2.953). In most cases, the absolute differences in the proportional odds ratio values for defendants of color and white defendants were quite small. In three comparisons, however, the proportional odds ratios for PSA risk levels predicting failure to appear, new criminal activity, and new violent criminal activity were markedly higher for white defendants than defendants of color (see Table 4).

### **Discussion**

With bail reform sweeping the nation, many jurisdictions are looking to risk assessment instruments as one potential—and controversial—strategy to support their efforts. We conducted a systematic review of research examining the validity with which six pretrial risk assessment instruments used in multiple jurisdictions across the United States forecast key pretrial outcomes overall and by race/ethnicity and gender, when possible. In doing so, we provide the most up-to-date information on the predictive validity of pretrial risk assessments by instrument and by outcome, and the first synthesis and comparison of findings across studies regarding predictive



validity for defendants of color and white defendants, as well as for men and women. We also present the first application of a risk assessment-specific checklist, the RAGEE, to examine the quality of studies included in systematic reviews of the predictive validity of risk assessments, pretrial or otherwise. Additionally, we introduce a new method for pooling and comparing odds ratios across studies of risk assessment instruments that use different cutoffs and risk levels: proportional odds ratios. While this analytic strategy has been used in other fields, we are not aware of prior reviews that have used it in the context of risk assessment research.

### **Summary of Findings**

Overall, findings of our review generally showed good to excellent predictive validity across outcomes for these six pretrial risk assessment instruments, with effect sizes in keeping with those seen in reviews of risk assessments used in other criminal justice contexts. Also consistent with those reviews, our findings did not reveal one pretrial risk instrument that produced assessments with the greatest predictive validity overall. Instead, our findings suggest that instruments produce pretrial risk assessments with somewhat greater predictive validity for some outcomes compared to others, but also that conclusions regarding the predictive validity of a given instrument may differ depending upon the predictors, outcomes, and effect sizes examined. For total scores, AUC values were highest for the PTRA. There was greater variability in the AUC values for the risk levels, with PSA risk levels producing slightly higher AUC values than VPRAI risk levels for new criminal activity and failure to appear, while COMPAS-PRRS risk levels produced the highest AUC values for the combined measure of new criminal activity and/or failure to appear and PTRA risk levels for the combined measure of new criminal activity, failure to appear, and/or technical violations. Looking at the proportional odds ratios, the CPAT risk levels performed especially well in predicting new criminal activity and

failure to appear. PTRAs risk levels demonstrated the highest proportional odds ratios for new violent criminal activity. PSA risk levels demonstrated good validity for new violent criminal activity, as well.

A primary aim of this review was to provide empirical evidence regarding the predictive validity of pretrial risk assessments across subgroups of defendants defined by race/ethnicity and gender. Only four studies of three instruments (i.e., the PSA, PTRAs, and VPRAI) reported results for racial/ethnicity subgroups and they defined those groups in different ways. The comparisons we could make suggested comparable predictive validity of PSA, PTRAs, and VPRAI assessments for defendants of color and white defendants in general. In a few comparisons, however, we did see lower levels of predictive validity among defendants of color than white defendants, particularly in the proportional odds ratios for PSA risk levels. Such findings not only raise potential concerns regarding the use of pretrial risk assessment instruments in practice, but also raise questions regarding the source and implications of these differences. Specifically, the pattern of results observed for the predictive validity of PSA assessments among defendants of color and white defendants may reflect the fact that PSA assessments are completed solely based upon record review and rely more heavily on criminal history and prior failures to appear in court compared to the other instruments. On the one hand, removing the step of interviewing defendants eliminates barriers to implementation that renders the PSA appealing to many jurisdictions, such as the staffing, space, and time required to conduct the defendant interviews (VanNostrand & Lowenkamp, 2013). On the other hand, variables that reflect prior contact with the criminal justice system have come under particular scrutiny due to their susceptibility to biased policing practices and prosecutorial decisions, as well as recordkeeping errors (Eckhouse et al., 2019; Lowder et al., 2019; Mayson, 2019).

That said, all PSA assessments examined herein were drawn from Kentucky and generalizability to other jurisdictions is unknown. PSA assessments conducted in a different jurisdiction with different input data may very well produce a different pattern of results. Beyond identifying whether racial bias exists, research is needed to understand why there are differences in predictive validity (Schmidt et al., 2020). Although the subject of much speculation, this issue has not received much empirical attention (Shepherd & Lewis-Fernandez, 2016). Further, racial bias in pretrial decisions made without risk assessment results is well documented, as are the gains in accuracy produced by structured methods for predicting future violent and criminal behavior. Consequently, we may find greater racial bias in Kentucky pretrial decisions in the absence of the PSA or another pretrial risk assessment instrument (Desmarais, 2020). Indeed, a recent meta-analytic review found reductions in pretrial detention rates associated with the use of pretrial risk assessments for both black and white defendants (Viljoen et al., 2019). In aggregate, findings do not provide empirical evidence that pretrial risk assessments systematically demonstrate racial bias. Because the number of studies reporting predictive validity data for racial/ethnic subgroups was relatively low, this remains a high priority avenue for future research.

Only a few studies provided the data necessary to compare predictive validity as a function of gender and, when they did, there was no consistent pattern of findings. Most often, we saw slightly higher levels of predictive validity among assessments of women than men, including for PTRAs assessing predicting new criminal activity, PSA assessments predicting new violent criminal activity and failure to appear, and VPRAI assessments predicting the combined measure of new criminal activity and/or failure to appear. However, predictive validity was slightly greater among men than women for PTRAs assessing predicting new violent

criminal activity and VPRAI assessments predicting the combined measure of new criminal activity, failure to appear, and/or technical violations. For most of these comparisons, the absolute value of the differences were so small as to suggest similar levels of predictive validity for men and women. In other words, based upon the findings of this review, we also do not find compelling evidence to suggest that pretrial risk assessments demonstrate predictive bias as a function of gender. At the same time, the numbers of women in U.S. jails continues to rise (Kajstura, 2019) and some work suggests there may be benefits to examining gender-specific factors in pretrial assessment (Gehring & Voorhis, 2014). More research on the predictive validity of pretrial risk assessments as a function of gender is needed.

Whether pretrial risk assessments completed using the six instruments included in our review demonstrate consistency across assessors was not addressed in the extant literature. Yet, increasing consistency in pretrial decision-making is one of the goals of current reform efforts (National Task Force on Fines, Fees, and Bail Practices, 2019). As such, the lack of information on the inter-rater reliability represents a limitation that should be addressed in future evaluations of pretrial risk assessment instruments. Inter-rater reliability is relevant to any assessment that requires rating or coding items, even without an interview (Douglas et al., 2012), as reliability is a necessary criteria for validity (Douglas et al., 2011; Gottfredson & Moriarty, 2006). Even if a large proportion of the items are completed using official records, there may be differences in how that information is interpreted by assessors and/or errors that occur in the coding. The RAGEE statement (Singh et al., 2015) specifies that inter-rater reliability must be reported or an explanation for lack of reporting must be given.

The lack of information on inter-rater reliability was not the only way in which many of the reviewed studies fell short of standards for conducting and reporting risk assessment

validation studies. Individual studies generally reported on about half to two-thirds of the 50 items included in the RAGEE statement. For instance, the length of follow-up data collection at the person or case level was reported in only two studies; a couple of other studies reported a total period during which data was collected overall. Follow-up length or time at risk was not controlled for in any analyses. Moreover, none of the studies accounted for pretrial conditions that could have affected outcomes. As a final example, other than race and gender, sample characteristics (e.g., behavioral health, socioeconomic status) were rarely provided. At least one of the reports included in our review (DeMichele et al., 2018) has now been published in a peer-reviewed outlet (DeMichele et al., 2020). As more studies are published in peer-reviewed outlets, we may see greater adherence to accepted methodological practices, reporting guidelines, and testing standards (AERA et al., 2014; Douglas et al., 2011; Singh et al., 2015).

Finally, many studies used arrests to operationalize the measurement of new (violent) criminal activity. Reliance on arrests—as both predictor and outcome—has been criticized in the debate surrounding the use of risk assessment instruments to inform pretrial decision-making (e.g., Barabas et al., 2019). People of color are more likely to be arrested for behaviors compared to white people (Kochel et al., 2011), yet arrests remain a common metric against which criminal justice decision-makers operationalize threat to public safety. Efforts are needed to move both the research and criminal justice system away from this focus on re-arrest to potentially less biased measures, such as convictions. Using convictions, however, is not a perfect solution due to bias in the winnowing process from arrest through prosecution, as well as in case disposition and sentencing (e.g., Kutateladze et al., 2014; Stolzenberg et al., 2013). Another strategy is to use filed charges, which could reduce false positives associated with arrest records and false negatives associated with convictions (Spohn & Holleran, 2002), but may still be subject to

biases. One more strategy is to include self-report, which has been shown to increase accuracy in the measurement of violence and crime (Crisanti et al., 2003; Johnson et al., 2019), but is time consuming and may have limited feasibility. Given the strengths and limitations of different strategies, future research should examine predictive validity across multiple operational definitions of pretrial outcomes using data collected from multiple sources.

### **Limitations**

The methodology of our review limits its findings in at least three ways. First, our intent was to present a comprehensive review of the validation research available on a sample of pretrial risk assessment instruments. For this reason, we used an inclusive selection strategy. One of our key findings is that studies examining the predictive validity of pretrial assessments often did not adhere to the methodological practices and reporting standards that are accepted in the field. As such, exclusion of studies that did not meet certain quality standards would likely have eliminated many sources from our review. Due to the small number of studies, we did not statistically compare results, but rather compared the effect sizes descriptively. Second, we strove to identify and include all studies examining the predictive validity of pretrial risk assessments that are commonly used in multiple jurisdictions. There may be studies that met inclusion criteria but were missed by our search strategy or that were produced after our cutoff date. We welcome efforts that build upon our findings as new research becomes available. Third, we were only able to examine predictive validity findings as a function of race/ethnicity for four studies and our comparisons across subgroups were limited by the ways in which studies defined those groups (e.g., grouping all people of color together). Given the widespread implementation of pretrial risk assessment instruments, continued evaluation of fairness and racial bias is needed.

## **Conclusion**

Despite these limitations, this review sheds light on the predictive validity of six widely used pretrial risk assessment instruments overall and across various subgroups of defendants and outcomes. Like many reviews of risk assessment instruments before this one, our findings do not identify one instrument that produces the most (or least) accurate pretrial risk assessments. To some extent, our findings suggest a tradeoff between efficiency and predictive validity: Pretrial risk assessments produced using instruments that comprised more items and that required defendant interviews often outperformed those produced using shorter instruments that could be completed solely based on records. Findings also suggest that pretrial risk assessments forecast outcomes with acceptable predictive validity. However, findings underscore the need for investigations of predictive validity across racial/ethnic subgroups and vis-à-vis new violent criminal activity. Beyond predictive validity, more research is needed on the impact of pretrial risk assessments, towards the reform goals of more equitable and less carceral pretrial decisions.

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### Endnotes

1. The magnitude of a correlation is greatly reduced for dichotomous variables that have a base rate of less than 50% and pretrial outcomes have base rates well below this level (see Babchishin & Helmus, 2016, for further discussion).
2. In keeping with practices used in prior reviews, we restricted our review of Google results to the first 300 hits of each search (see, for example, Bramer et al., 2017; Haddaway et al., 2015; Piasecki et al., 2018; Viljoen et al., 2019).
3. Results from the one study of ORAS-PAT assessments were published across a paper and a report and, similarly, results for one of the VPRAI studies were published across two separate technical reports.
4. Because it was not possible to identify and exclude repeated assessments from the data available to us, and given that all instruments included at least some dynamic factors that could change over time, we treat assessments themselves as the unit of analysis, acknowledging that we may have multiple assessments for one individual.
5. The PSA uses a binary flag for risk of new violent criminal activity; we used the raw scores to create a 3-level variable for analysis.
6. In keeping with this journal's publication guidelines, we use the term Latinx here. However, studies included in our review reported on Hispanic ethnicity, specifically.

**Table 1.** Summary of Assessment, Study, and Sample Characteristics

CHARACTERISTIC	GROUP	FREQUENCY
<b>Assessment Process</b>		
Risk assessor	Researcher	2
	Professional	5
	Not reported/unclear	4
Source of information	Official records	5
	Interview with defendant	5
	Interview with others	1
	Not reported/unclear	4
Assessment format	Assessor-completed, computer format	4
	Self-administered, format not specified	1
	Assessor-administered, format not specified	5
	Not reported/unclear	2
Study context	Research	7
	Practice/implementation	4
<b>Study Characteristics</b>		
Publication type <sup>a</sup>	Journal article	3
	Government/technical report	10
Tool authorship	Author of tool was study author	9
	Study author was not author of tool	2
Study jurisdiction	Single site/jurisdiction	2
	Multi-site/jurisdiction	9
Duplicate or overlapping sample	Yes	6
	No	5
Temporal design	Prospective	3
	Retrospective	6
	Not reported/unclear	2
Length of follow-up in months ( $k = 2$ )	$M (SD)$	11.50 (0.71)
Predictor(s) tested	Individual item scores	2
	Total scores	7
	Subscale scores	1
	Risk levels	8
	Not reported/unclear	1
Pretrial failure outcome measured	Failure to appear	11
	New criminal activity	11
	Violent offending	2
	Violation or breach of conditions	5

**Table 1.** (continued)

Source of outcome data	Official records	5
	Not reported/unclear	6
Statistics reported	Frequencies	10
	Correlations	4
	Chi square	2
	Odds ratios	3
	AUC	8
	DIF-R	2
RAGEE items reported (out of 50)	<i>M (SD)</i>	25.00 (4.78)
<b>Sample Characteristics</b>		
Sample characteristics calculated on	Full sample, no attrition	6
	Final sample, after attrition	2
	Not reported/unclear	3
Sample size at assessment ( <i>k</i> = 11)	<i>M (SD)</i>	61,229.09 (79,563.05)
Male participants ( <i>k</i> = 9)	<i>M (SD)</i>	30,189.64 (39,676.04)
White participants ( <i>k</i> = 9)	<i>M (SD)</i>	28,581.60 (44,853.75)
Black participants ( <i>k</i> = 8)	<i>M (SD)</i>	10,660.40 (10,477.57)
Latinx participants ( <i>k</i> = 4) <sup>b</sup>	<i>M (SD)</i>	5,596.08 (9,687.37)
Other race participants ( <i>k</i> = 9) <sup>c</sup>	<i>M (SD)</i>	1,615.40 (2,266.47)
Age (in years) at assessment ( <i>k</i> = 6)	<i>M (SD)</i>	33.22 (2.39)

Notes. RAGEE = Risk Assessment Guidelines for Evaluation of Efficacy. *M* = Mean. *SD* = standard deviation. Some category counts add to more than 11 studies because some categories are not mutually exclusive. <sup>a</sup> There are 13 publication types because we drew information from a peer-reviewed paper and a technical report that reported on the same study for the ORAS-PAT, and the results for one of the VPRAI studies was published across two separate technical reports. <sup>b</sup> In most studies, race/ethnicity were recorded in one variable; however, in one study, race and ethnicity were coded separately. <sup>c</sup> Includes Asian, Native American, Pacific Islander, unidentified races, and in one study, Black defendants.

**Table 2.** Areas Under the Curve for Total and Subscale Scores Predicting Pretrial Outcomes

	ALL DEFENDANTS			GENDER						RACE/ETHNICITY					
				Male Defendants			Female Defendants			White Defendants			Defendants of Color		
	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )
<b>NCA</b>															
All instruments	2	.664	(.643-.685)	2	.665	(.644-.686)	2	.675	(.645-.705)	3	.671	(.655-.687)	3	.662	(.593-.731)
PSA	1	.650	--	1	.653	--	1	.637	--	1	.647	--	1	.659	--
PTRA	1	.680	(.660-.690)	1	.670	(.650-.700)	1	.690	(.660-.710)	2	.675	(.657-.693)	2	.663	(.583-.743)
<b>NVCA</b>															
All instruments	2	.673	(.649-698)	2	.668	(.634-.702)	2	.658	(.616-.700)	3	.674	(.644-.705)	3	.656	(.571-.741)
PSA	1	.664	--	1	.654	--	1	.657	--	1	.666	--	1	.631	--
PTRA	1	.690	(.660-.720)	1	.690	(.640-.740)	1	.660	(.590-.740)	2	.682	(.639-.726)	2	.671	(.563-.780)
<b>FTA</b>															
All instruments	2	.655	(.639-.671)	--	--	--	--	--	--	--	--	--	--	--	--
PSA	1	.646	--	1	.642	--	1	.655	--	1	.655	--	1	.612	--
PTRA	1	.670	(.650-.690)	--	--	--	--	--	--	--	--	--	--	--	--
<b>TV</b>															
All instruments	2	.710	(.680-.739)	--	--	--	--	--	--	--	--	--	--	--	--
PTRA	1	.730	(.720-.740)	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI*	1	.688	(.674-.703)	--	--	--	--	--	--	--	--	--	--	--	--
<b>NCA or FTA</b>															
All instruments	5	.676	(.659-.693)	--	--	--	--	--	--	--	--	--	--	--	--
ORAS-PAT	1	.675	--	--	--	--	--	--	--	--	--	--	--	--	--
PSA	1	.638	--	--	--	--	--	--	--	--	--	--	--	--	--
PTRA	3	.689	(.684-.695)	--	--	--	--	--	--	--	--	--	--	--	--
<b>NCA or FTA or TV</b>															
All instruments	5	.690	(.652-.727)	--	--	--	--	--	--	--	--	--	--	--	--
PTRA	3	.721	(.712-.729)	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI	2	.644	(.599-.689)	--	--	--	--	--	--	--	--	--	--	--	--

Notes. NCA = New Criminal Activity; NVCA = New Violent Criminal Activity; FTA = Failure to Appear; ORAS-PAT = Ohio Risk Assessment System-Pretrial Assessment Tool; PSA = Public Safety Assessment; PTRA = Pretrial Risk Assessment Instrument; VPRAI = Virginia Pretrial Risk Assessment Instrument. *k* = number of effect sizes; *AUC* = area under the receiver operating characteristic curve; 95% *CI* = 95% confidence interval. When multiple effect sizes were coded for a given predictor and outcome, the mean effect sizes were estimated using the random effects model. -- not reported and could not be calculated.

**Table 3.** Areas Under the Curve for Risk Levels Predicting Pretrial Outcomes

	ALL DEFENDANTS			GENDER						RACE/ETHNICITY					
	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	Male Defendants			Female Defendants			White Defendants			Defendants of Color		
				<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )	<i>k</i>	<i>AUC</i>	(95% <i>CI</i> )
<b>NCA</b>															
All instruments	2	.626	(.617-.634)	--	--	--	--	--	--	--	--	--	--	--	--
PSA	1	.630	--	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI	1	.621	--	--	--	--	--	--	--	--	--	--	--	--	--
<b>FTA</b>															
All instruments	2	.631	(.619-.643)	--	--	--	--	--	--	--	--	--	--	--	--
PSA	1	.640	--	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI	1	.622	--	--	--	--	--	--	--	--	--	--	--	--	--
<b>TV</b>															
VPRAI	1	.655	--	--	--	--	--	--	--	--	--	--	--	--	--
<b>NCA or FTA</b>															
All instruments	4	.669	(.633-.705)	--	--	--	--	--	--	--	--	--	--	--	--
COMPAS-PRRS	1	.715	--	--	--	--	--	--	--	--	--	--	--	--	--
ORAS-PAT	1	.650	--	--	--	--	--	--	--	--	--	--	--	--	--
PSA	1	.620	--	--	--	--	--	--	--	--	--	--	--	--	--
PTRA	1	.692	(.687-.696)	--	--	--	--	--	--	--	--	--	--	--	--
<b>NCA or FTA or TV</b>															
All instruments	4	.663	(.621-.705)	--	--	--	--	--	--	--	--	--	--	--	--
PTRA	1	.726	(.722-.729)	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI	3	.642	(.613-.671)	2	.657	(.630-.686)	2	.665	(.630-.700)	2	.678	(.648-.709)	2	.637	(.540-.735)

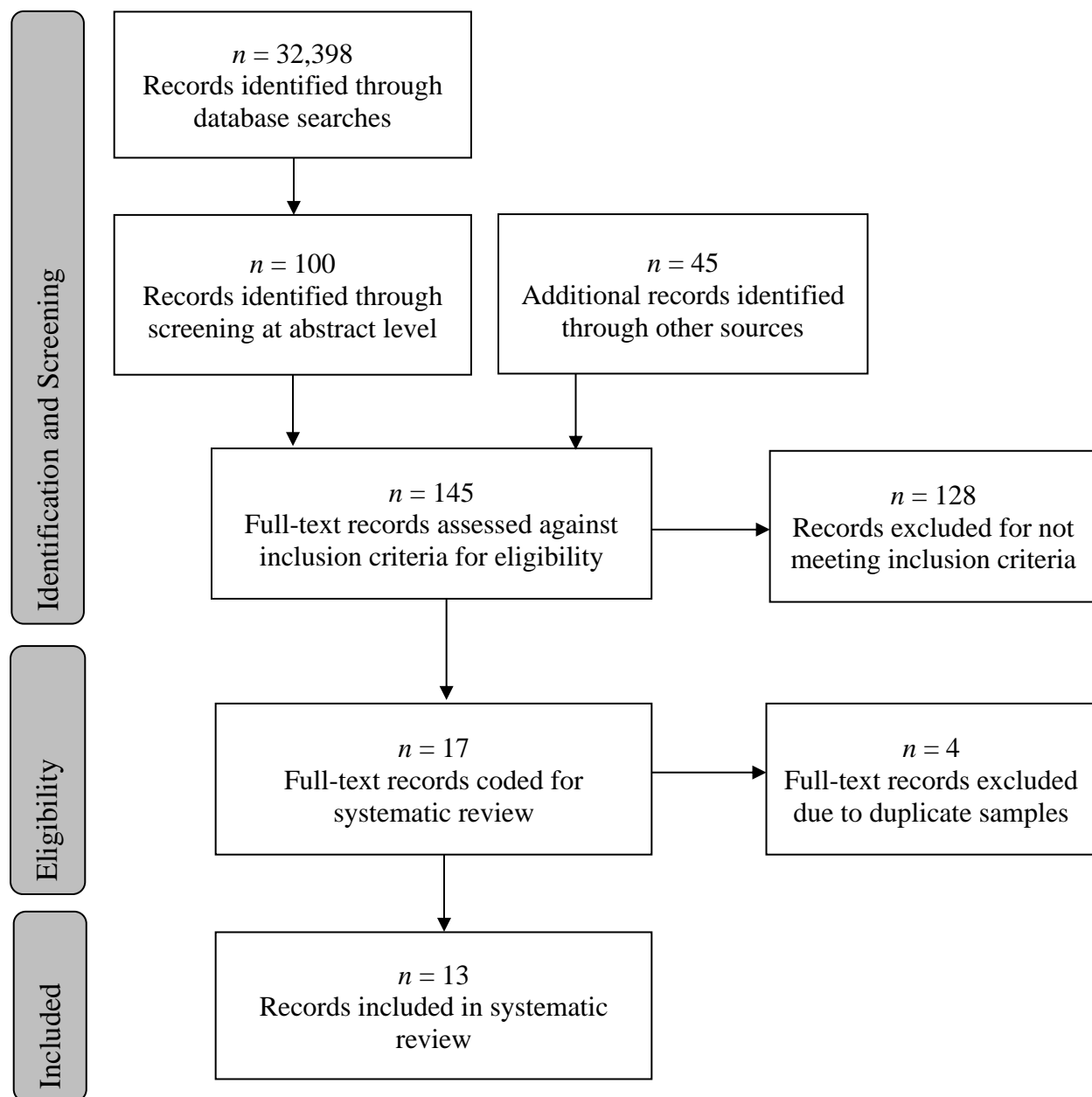
Notes. NCA = New Criminal Activity; NVCA = New Violent Criminal Activity; FTA = Failure to Appear; COMPAS = Correctional Offender Management Profiling for Alternative Sanctions; ORAS-PAT = Ohio Risk Assessment System-Pretrial Assessment Tool; PSA = Public Safety Assessment; PTRA = Pretrial Risk Assessment Instrument; VPRAI = Virginia Pretrial Risk Assessment Instrument. *k* = number of effect sizes; *AUC* = area under the receiver operating characteristic curve; 95% *CI* = 95% confidence interval. When multiple effect sizes were coded for a given predictor and outcome, the mean effect sizes were estimated using the random effects model. -- not reported and could not be calculated.

**Table 4.** Proportional Odds Ratios for Risk Levels Predicting Pretrial Outcomes

	ALL DEFENDANTS			GENDER						RACE/ETHNICITY					
				Male Defendants			Female Defendants			White Defendants			Defendants of Color		
	<i>k</i>	<i>POR</i>	(95% <i>CI</i> )	<i>k</i>	<i>POR</i>	(95% <i>CI</i> )	<i>k</i>	<i>POR</i>	(95% <i>CI</i> )	<i>k</i>	<i>POR</i>	(95% <i>CI</i> )	<i>k</i>	<i>POR</i>	(95% <i>CI</i> )
<b>NCA</b>															
All instruments	6	2.673	(2.379-3.004)	2	2.744	(2.596-2.900)	2	2.707	(2.222-3.299)	3	3.100	(2.652-3.623)	3	2.730	(2.593-2.874)
CPAT	1	3.068	(2.240-4.203)	--	--	--	--	--	--	--	--	--	--	--	--
PSA	2	2.459	(2.091-2.891)	1	2.699	(2.604-2.797)	1	2.461	(2.324-2.606)	1	3.516	(3.400-3.636)	1	2.722	(2.532-2.927)
PTRA	1	2.967	(2.819-3.122)	1	2.880	(2.616-3.171)	1	3.012	(2.700-3.361)	2	2.888	(2.678-3.113)	2	2.717	(2.459-3.003)
VPRAI	2	2.774	(1.719-4.476)	--	--	--	--	--	--	--	--	--	--	--	--
<b>NVCA</b>															
All instruments	2	3.075	(2.860-3.306)	2	2.795	(2.557-3.056)	2	2.763	(2.289-3.334)	3	3.023	(2.757-3.314)	3	2.658	(2.355-2.999)
PSA	1	2.964	(2.714-3.236)	1	2.744	(2.490-3.023)	1	2.938	(2.386-3.617)	1	3.042	(2.743-3.373)	1	2.305	(1.944-2.733)
PTRA	1	3.323	(2.924-3.776)	1	3.090	(2.464-3.875)	1	2.379	(1.696-3.337)	2	2.953	(2.414-3.612)	2	3.070	(2.587-3.644)
<b>FTA</b>															
All instruments	6	2.512	(2.308-2.735)	--	--	--	--	--	--	--	--	--	--	--	--
CPAT	1	5.282	(3.701-7.539)	--	--	--	--	--	--	--	--	--	--	--	--
PSA	2	2.459	(2.247-2.690)	1	2.517	(2.449-2.607)	1	2.702	(2.579-2.831)	1	2.711	(2.633-2.792)	1	1.751	(1.648-1.860)
PTRA	1	2.587	(2.355-2.841)	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI	2	2.172	(1.880-2.509)	--	--	--	--	--	--	--	--	--	--	--	--
<b>NCA or FTA</b>															
All instruments	6	2.920	(2.511-3.396)	--	--	--	--	--	--	--	--	--	--	--	--
CPAT	1	4.333	(3.204-5.859)	--	--	--	--	--	--	--	--	--	--	--	--
ORAS-PAT	1	3.831	(2.108-6.962)	--	--	--	--	--	--	--	--	--	--	--	--
PSA	1	2.429	(2.360-2.500)	--	--	--	--	--	--	--	--	--	--	--	--
PTRA	2	3.073	(2.831-3.336)	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI	1	2.267	(1.858-2.767)	1	3.540	(2.862-4.378)	1	3.751	(2.374-5.926)	--	--	--	--	--	--
<b>NCA or FTA or TV</b>															
All instruments	4	3.240	(2.767-3.794)	--	--	--	--	--	--	--	--	--	--	--	--
PTRA	2	3.910	(3.645-4.195)	--	--	--	--	--	--	--	--	--	--	--	--
VPRAI	2	2.532	(2.253-2.846)	1	2.602	(2.338-2.895)	1	2.526	(2.102-3.036)	1	2.898	(2.548-3.297)	1	2.280	(1.994-2.607)

Notes. NCA = New Criminal Activity; NVCA = New Violent Criminal Activity; FTA = Failure to Appear; CPAT = Colorado Pretrial Assessment Tool; ORAS-PAT = Ohio Risk Assessment System-Pretrial Assessment Tool; PSA = Public Safety Assessment; PTRA = Pretrial Risk Assessment Instrument; VPRAI = Virginia Pretrial Risk Assessment Instrument. *k* = number of effect sizes; *POR* = *proportional odds ratio*; *95% CI* = 95% confidence interval. When multiple effect sizes were coded for a given predictor and outcome, the mean effect sizes were estimated using the random effects model. -- not reported and could not be calculated.



**Figure 1.** Results of Systematic Literature Search for Predictive Validity Studies

**Online Supplemental Table 1.** Type and Content of Items included in Reviewed Pretrial Risk Assessment Instruments

	COMPAS-PPRS	CPAT	ORAS-PAT	PSA	PTRA	VPRAI
<b>TYPES OF FACTORS</b>						
Risk	X	X	X	X	X	X
Protective						
Static	X	X	X	X	X	X
Dynamic	X	X	X	X	X	X
<b>CONTENT DOMAIN</b>						
<b>Demographic Characteristics</b>						
Age	X			X	X	
Marital status						
Citizenship					X	
Education					X	
<b>Personal/Social Characteristics</b>						
Living situation	X	X	X		X	X
Employment	X		X		X	X
Financial resources		X				
Local family relationships	X					
<b>Criminal Justice Characteristics</b>						
Current charge	X			X	X	X
Other pending charges	X	X		X	X	X
Time in community	X					
History of pretrial failure	X		X	X	X	X
Prior revoked bail or suspension		X				
Prior misdemeanor offenses				X		X
Prior felony offenses				X	X	X
Prior violent convictions				X		X
Age at first arrest		X	X			
Prior incarceration	X	X	X	X		
<b>Clinical Characteristics</b>						
Substance use	X	X	X		X	X
Mental health		X				

*Notes.* COMPAS-PPRS = Correctional Offender Management Profiling for Alternative Sanctions-Pretrial Risk Release Scale; CPAT = Colorado Pretrial Assessment Tool; ORAS-PAT = Ohio Risk Assessment System: Pretrial Assessment Tool; PSA = Public Safety Assessment; PTRA = Pretrial Risk Assessment Instrument; VPRAI = Virginia Pretrial Risk Assessment Instrument.

**Online Supplemental Table 2.** Characteristics of Studies Examining the Predictive Validity of Pretrial Risk Assessment Instruments

	Analytic Sample Size	Unit of Analysis	Jurisdictions	Study Years	Offense Types	Sample Characteristics	Predictor(s)	Subgroups Examined	Outcome Data Source(s)	Pretrial Outcome(s) Assessed	Follow-up Period(s)	RAGEE Items Reported
<b>COMPAS-PRRS</b>												
Dieterich (2010)	2,831	person	Kent County, FL	2008	felony	not reported	not clear	none	official records	FTA, felony arrest	not reported	21
<b>CPAT</b>												
The Pretrial Justice Institute (2012)	1,315	person	10 Colorado counties	unclear	felony, misdemeanor, traffic	76.8% male 85.6% White 11.5% Black 1.4% Native American 1.2% Asian 0.2% Pacific Islander	risk levels	none	official records	FTA, new filing (violent & non-violent)	up to 16-month timeframe	26
<b>ORAS-PAT</b>												
Latessa et al., (2009); Latessa, et al. (2010)	452	person	7 Ohio counties plus some from "another state"	2006-2009	not specified	79.3% male 50.2% White 46.5% Black 3.3% race not specified <i>M</i> age = 32.7	total scores and risk levels	none	official records	FTA, new arrest (violent & non-violent)	12 months	34; 23
<b>PSA</b>												
DeMichele et al. (2018)	164,597	case	statewide in Kentucky	2013-2014	not specified	68.9% male 81.1% White 16.8% Black 2.1% race not specified	subscale scores, violence flag	gender (men, women) race (Black, White)	official records	FTA, new arrest (violent & non-violent), new violent arrest	not reported	26
Lowenkamp & VanNostrand (2013)	109,633	case	statewide in Kentucky	2009-2011	not specified	74% male 80% White 18% Black 2% other race <i>M</i> age = 33.5	risk levels	none	unclear	FTA, new arrest (violent & non-violent), bail revocation	not reported	20

Online Supplemental Table 2. (continued)

	Analytic Sample Size	Unit of Analysis	Jurisdictions	Study Years	Offense Types	Sample Characteristics	Predictor(s)	Subgroups Examined	Outcome Data Source(s)	Pretrial Outcome(s) Assessed	Follow-up Period(s)	RAGEE Items Reported
<b>PTRA</b>												
Cohen & Lowenkamp (2019)	85,369	unclear	federal system	2009-2015	not specified	71.7% men 42.8% White 25.6% Black 24.2% Hispanic 7.4% race not specified <i>M</i> age = 37.8	total scores, risk levels	race (non-Hispanic White, non-Hispanic Black, Hispanic) gender (men, women)	official records	FTA, new arrest (violent & non-violent), pretrial revocation (technical violation & arrest)	11 months	34
Lowenkamp & Whetzel (2009)	200,582.50*	unclear	federal system	2001-2007	all	not reported	individual item scores, total scores, risk levels	none	not reported	FTA, new arrest (violent & non-violent), technical violation	not reported	20
<b>VPRAI</b>												
Danner et al. (2015, 2016)	14,382	case	statewide in Virginia (29 pretrial service agencies)	2013-2014	felony, misdemeanor, traffic	74.4% male 51.5% White 43.2% Black 3.6% Hispanic 0.1% Native American 1% Asian <i>M</i> age = 32.3	individual item scores, risk levels	race (White, Persons of Color) gender (men, women)	not reported	FTA, new arrest (violent & non-violent), technical violation	not reported	25; 23
Lovins & Lovins (2016)	568	person	Riverside County, CA	2014-2015	not specified	71.8% male 36.6% White 63.4% Persons of Color	total scores, risk levels	none	not reported	FTA, new arrest (violent & non-violent), technical violation	not reported	21
VanNostrand (2003)	1,971	person	7 Virginia localities	1998-1999	felony, misdemeanor	78.5% male 57.7% White 39.8% Black 2.4% race not specified	total scores, risk levels	race (White, Black) gender (men, women) income	official records	FTA, new arrest (violent & non-violent)	up to 1 year 10 months	29

**Online Supplemental Table 2.** (continued)

	Analytic Sample Size	Unit of Analysis	Jurisdictions	Study Years	Offense Types	Sample Characteristics	Predictor(s)	Subgroups Examined	Outcome Data Source(s)	Pretrial Outcome(s) Assessed	Follow-up Period(s)	RAGEE Items Reported
VanNostrand et al. (2011)	28,044	case	statewide in Virginia (30 pretrial service agencies)	2005-2008	felony, misdemeanor	78% male 46% White 48% Black 4.5% Hispanic 1.5% race not specified M age = 32.0	risk levels	none	interview with defendant	FTA, new arrest (violent & non-violent), technical violation	not reported	23

Notes. COMPAS-PPRS = Correctional Offender Management Profiling for Alternative Sanctions-Pretrial Release Risk Scale; CPAT = Colorado Pretrial Assessment Tool; ORAS-PAT = Ohio Risk Assessment System-Pretrial Assessment Tool; PSA = Public Safety Assessment; PTRR = Pretrial Risk Assessment Instrument; VPRAI = Virginia Pretrial Risk Assessment Instrument. RAGEE = Risk Assessment Guidelines for Evaluation of Efficacy. FTA = failure to appear. \*Lowenkamp & Whetzel (2009) reported a range of sample sizes from 185,827 - 215,338 so we used the average of the lower end and upper end of the range as the reported sample size.

## CHAPTER 5

### Integrative Review

The United States has the highest rate of incarceration in the world. In fact, when each state is considered separately, 31 states individually detain more people than any other country. While state prisons detain more people each year on a longer-term basis, jails see a higher volume of people every year (Bronson & Carson, 2019; Zeng, 2020). Roughly 4.9 million people are booked into jail each year, many of them more than one time (Sawyer & Wagner, 2020), and some sources suggest that US jail populations are increasing (Kang-Brown et al., 2019). Despite the large, and possibly growing, number of people that are impacted by jails every year, the amount of research focused on jails is limited. Therefore, many jail practices lack empirical support and evaluation. This is true of long-standing jail practices (Peters et al., 2008), and newer practices that have been developed in line with recent reform efforts (Desmarais et al., In Press).

Increasing the evidence base for jail practices is critical at a time when jail reform is receiving a lot of national attention and therefore increased momentum. During the period between jail booking and first appearance in court, dozens of decisions are made about a person that have major implications for their trajectory through the rest of the criminal justice process. Currently, many of these decisions are made with limited guidance or transparency (Kubiak et al., 2020; Stevenson & Mayson, 2017) and as a result, there is evidence of disparate decision making across subgroups (Wooldredge et al., 2015). Changes to existing practices and the implementation of new practices for this period are needed but they should be evidence-based and supported by rigorous research. The manuscripts in this study sought to build the evidence base for jail practices between booking and first appearance, including the critical decisions that are made and instruments used to help make those decisions.

## Summary of Findings

**Study 1.** In Chapter 2, I sought to contribute to the limited evidence base for the BJMHS. The BJMHS is a widely used mental health screening instrument that helps determine whether a person should undergo a more in-depth mental health evaluation (Callahan & Noether, 2018). Overall, findings from this study showed that people's results on the BJMHS change across repeated jail bookings. Specifically, the odds of a positive screen increased with each jail booking, as did the odds of referral on the basis of staff discretion. In contrast, the odds of screening positive for two or more current symptoms and prior hospitalization decreased. There was no change in the odds of screening positive for current psychiatric medication across bookings. It is possible that some of these changes may reflect improvements to mental health or natural fluctuations in the presentation of mental illness (Rogers et al., 2010). However, changes may also reflect issues with the administration or recording of items, or decreased reporting if people grow tired of responding to the same questions multiple times (Meehan et al., 2012).

The increases in an overall positive BJMHS seem to be driven by the increases in the odds of discretionary referral. This suggests that jail staff may become familiar with people across repeated bookings and refer them even if they otherwise screen negative. Perhaps staff do not trust the BJMHS results either because of the self-report nature of items or because there is evidence that the BJMHS does not detect some mental health problems (e.g., anxiety) very well (Steadman et al., 2007). Finally, it is possible that changes to screening results reflect a lack of fidelity in screening administration or poor reliability of the BJMHS items. Indeed, a recent review of BJMHS studies demonstrated highly variable findings across studies (Martin et al., 2013). Overall, this study's findings highlight the urgent need for closer examination of the mechanisms for change in BJMHS results. Some change across bookings is reasonable (e.g., a

reduction of symptoms) while other change is not (e.g., a reduction of prior hospitalization). Mental health screening results can have serious implications for jail detainees and the instruments used to conduct these screens must be thoroughly validated and reliable.

**Study 2.** In Chapter 3, I examined factors that contribute to the amount of bail a person is assigned at first appearance. Specifically, I used multi-level modeling to examine main and interactive effects of legally relevant variables (i.e., charge count, charge severity, and booking number), race, gender, and age on bail amounts assigned across repeated jail bookings. Findings showed that all legally relevant variables were significant such that more charges, more bookings, and a more serious charge were all associated with a higher bail amount. However, 2-way and 3-way interactions with race, gender, and age qualified these main effects. One 3-way interaction between charge severity, age, and race demonstrated that being older and being Black seemed to outweigh charge level in influencing bail amount. Indeed, older people with B-level charges received higher bail than younger people with A-level charges. Similarly, Black people with B-level charges received higher bail than White people with A-level charges. Another 3-way interaction between race, gender, and age showed White women received the lowest bail amounts followed by Black women, White men, and then Black men. Among White women, Black women, and White men, older defendants received slightly higher amounts than younger defendants. However, this pattern was reversed among Black men, with younger defendants receiving higher bail than older defendants.

Findings from this study demonstrate that the way some legal factors influence assigned bail amount is moderated by social categories. Furthermore, bail is not assigned equally for everyone. Some groups, like young, Black men receive higher bail amounts than other groups even when the variables that should influence bail are considered. This study highlights the need



to examine the simultaneous impact of multiple social categories to understand how current bail practices influence specific groups of people (e.g., young, Black men) in different ways. Reform efforts must be informed by this understanding to assure that efforts truly reduce discriminatory practices for the specific groups who are currently experiencing unfair treatment.

**Study 3.** In Chapter 4, I contribute to the discussion around pretrial reform with a systematic review that examines the predictive validity of pretrial risk assessment instruments overall and by subgroup. It is the first systematic review to examine validity in predicting new violent crime which has been emphasized by recent statutes relevant to decisions about bail and detainment (United States v. Salerno, 1987; American Bar Association, 2007). Our literature review identified 11 studies (in 13 publications) that examined the predictive validity of six pretrial risk assessment instruments. Overall, the risk assessment instruments showed good to excellent predictive validity for the full samples of each study. Differences in predictive validity by gender were mixed. Some instruments demonstrated better validity for women (e.g., the PTRAI predictions for new criminal activity) while others demonstrated better validity for men (e.g., the VPRAI predictions for any pretrial failure).

Differences in predictive validity by race were generally comparable across People of Color and White people. However, we did see lower levels of predictive validity for People of Color for a few comparisons, namely, the proportional odds ratios examining outcomes for the Public Safety Assessment. Our examination of gender and race differences was limited by the fact that only four studies examined the performance of risk assessment instruments across gender and race. Overall, we did not find that any one pretrial risk assessment instrument stood out as being the most accurate. However, there is a critical need for more studies examining the predictive validity of these instruments across subgroups. This includes a need for more

comparisons of race and gender as well as a need for comparisons across other subgroups such as socioeconomic status. These instruments cannot be included with complete confidence among pretrial reform efforts without more thorough validation research.

### **Implications**

The findings from these three studies have several implications that are relevant in this time of national focus on jail reform in general and pretrial reform specifically. Most of these implications have to do with the speed with which reform efforts are moving. For example, some reform efforts that have outpaced the research and are being implemented without a strong evidence-base. Other reform efforts are being implemented without a clear understanding of the problems that exist with current jail practices. These efforts require more research to build up the evidence-base supporting them. The final implication of this work focuses on the fact that, while more research is needed, the screening and assessment instruments studied herein do show success as options that can be employed toward achieving reform. Overall, this dissertation aims to capitalize on the jail reform momentum and to contribute to the body of literature that comprises the evidence base for the various reform efforts that are currently underway.

One reform effort that needs a stronger evidence-base is the use of the BJMHS in jails. To be sure, the practice of administering mental health screens at booking is critical due to the high prevalence of mental illness in jails (Steadman et al., 2009). Many organizations (e.g., National Commission on Correctional Health Care (NCCHC), Stepping Up Initiative, SAMSHA, National Conference of State Legislatures), recommend administration of a mental health screen at booking and some recommend the BJMHS specifically (NCCHC, 2018; Peters et al., 2008; The Stepping Up Initiative, 2018; Widgery, 2018). Yet, only a few studies have validated this screening instrument in jails with somewhat mixed results (Evans et al., 2010; Prins et al., 2012;

Steadman et al., 2007). Chapter 2 of this dissertation also demonstrates mixed results regarding the BJMHS's effectiveness. The rate of mental health problems identified by the screening instrument was well below what is typically reported (Steadman et al., 2005). Further, endorsement of many items decreased across repeated bookings; even for one item that could not have decreased because it measured a static historic experience (i.e., ever hospitalized). These results suggest that more research is needed to ensure that the BJMHS accurately identifies people in need of further mental health evaluation. Results also suggest that more training may be necessary to ensure the instrument is administered with fidelity. The BJMHS continues to be recommended for use in efforts to reduce the number of people with mental illness in jails (The Stepping Up Initiative, n.d.). Perhaps these recommendations could be expanded to include more thorough training to ensure proper administration. Additionally, the data generated from these efforts could be used to develop a stronger, more updated evidence base for this tool.

A second reform effort that needs a stronger evidence base is the use of pretrial risk assessment instruments. Civil rights groups, lawyers, journalists, and government officials have called into question the ability of pretrial risk assessment instruments to produce fair predictions of risk and some have gone as far as to say that these instruments should not be used in pretrial reform efforts (Angwin et al., 2016; Holder, 2014; Mayson, 2018; PJI, 2020; The Leadership Conference on Civil and Human Rights, 2018). In response to these concerns, researchers must rigorously examine pretrial risk assessment instruments in all aspects of their performance, particularly in terms of their ability to reduce racial disparities. Instruments that exacerbate racism in criminal justice decision making cannot be part of the efforts to reform this system. Yet, as the systematic review in Chapter 4 of this dissertation demonstrates, there is an incredibly limited amount of research focused on this concern. Only four studies have examined the

presence of racial bias in the prediction of pretrial risk assessment instruments. Further, these studies found limited evidence of bias in predictions of pretrial outcomes. To be sure, this is a topic that warrants a great deal of further examination, but the existing validation studies demonstrate promising results. Pretrial risk assessment instruments are being rapidly adopted across the United States (Advancing Pretrial Policy and Research, n.d.). Thus, researchers must continue to engage in thorough examination of the performance of these tools in all domains of their functioning. Particular attention should be paid to the instruments' ability to produce fair predictions of risk to contribute to equitable reductions in pretrial incarceration rates.

While there are disagreements about exactly how reform should be enacted, there is consensus across academic, legal, activist, and government fields that reform is needed. Some of the disagreement over specific policies and procedures may be due in part to gaps that still exist in our understanding of the effects of current practices, especially those related to bail. Chapter 3 of this dissertation helped to fill a gap in the literature regarding bail assignment. This study was the first to examine interactions between social categories and legal factors in a sample that included people charged with felonies and misdemeanors. It was also the first to use a longitudinal design. Findings from this study demonstrated a concerning pattern of racial disparities in bail assignment. These findings highlight the critical need to implement procedures that provide guidance for decisions about bail and that will allow for transparency around those decisions. With guidelines and transparency in place, it will be possible to monitor whether these decisions are made in the same way for everyone. Additionally, if there is reason to suspect a decision was not made fairly, a person's counsel, who is assigned after decisions about bail are made (Worden et al., 2018), will be able to retroactively call that decision into question. Enacting guidelines and transparency will promote fairness in decision making and will allow for

more oversight of this process by defendants' counsel and other parties seeking to promote defendant rights during this process.

Currently, some jurisdictions across the country are seeking to put in place clearer guidelines and policies to promote fairness and transparency in decisions made at first appearance (National Conference on State Legislators, 2018). According to a report from the National Conference of State Legislatures (2018), lawmakers in 46 states enacted 182 laws related to pretrial reform in 2017. While some states passed laws directing studies of risk assessment instruments, there were nine states that enacted laws allowing or requiring the use of these instruments. It is unclear whether the enactment of those laws was preceded by a period of research or information gathering. Yet as Chapter 3 demonstrates, it is necessary for jurisdictions to determine the impact of current practices so that they know what changes their reform efforts must target. Otherwise, these efforts may just perpetuate existing inequalities. Further, it is not clear whether any states required evaluations to validate the risk assessment instruments in their jurisdiction or follow up studies to examine whether the assessment instruments are being implemented with fidelity (National Conference of State Legislatures, 2018). States that implement assessment instruments should perform validation studies prior to implementation or within the first year of implementation to ensure that the tools are accurately, and fairly, predicting risk in that jurisdiction. Follow up evaluations should also be required to ensure that the risk assessment tools are being completed and applied appropriately. Findings from the study in Chapter 2 demonstrate the fact that screening tools used in jails do not always function as they should. This is a problem because all of these screening and assessment instruments are used to make decisions about people that will impact the rest of their trajectory through the criminal justice system and through their lives (Kurlychek & Johnson, 2019).

Therefore, all reform efforts should include research and evaluation to determine which current practices are most problematic, whether the chosen reform efforts are valid in a jurisdiction, and whether the new practices and policies are being implemented correctly and appropriately.

While there is certainly a need for continued research of the screening and assessment instruments included across these dissertation studies, findings also show that these instruments provide important information that can be used to inform the decisions made between booking and first appearance. Findings from Chapter 2 showed that people were more likely to screen positive on the BJMHS overall across repeated bookings. This finding could mean that people were more likely to be identified as having mental health problems and to be flagged for further evaluation. Ultimately, identifying mental health problems at this early stage in the process can contribute to people receiving needed mental health care and reduced involvement with the criminal justice system in the future (Lamberti et al., 2001). Findings from Chapter 4 indicate that there are several options available for pretrial risk assessment instruments that show good predictive validity for risk of pretrial failure (i.e., failure to appear in court and new criminal arrest). Further, there was limited evidence of racial bias in these predictions. These findings support the use of risk assessment instruments which have also been shown to reduce the number of people held in pretrial detention (Redcross et al., 2019). Thus, there is promising evidence that, mental health screens and risk assessment instruments can be used in jail reform efforts aimed at reducing the number of people who are incarcerated in jails throughout the United States.

### **Future Directions**

The studies included in this dissertation highlight two important directions for future research focused on jail and pretrial reform. The first direction is the use of longitudinal study

designs. Chapter 2 incorporated a longitudinal design to study changes in a mental health screening instrument across repeated jail bookings. This is not a common study design in jail research even though, in practice, people often experience repeated bookings and therefore repeated screenings. It is important for future work to employ research designs that examine changes to screening and assessment instrument results, decisions made by jail staff or judges, and outcomes experienced by people. Longitudinal study designs will allow for an understanding of how things change with repeated jail contact. This is particularly important for screening and assessment instruments. If instruments change in ways that indicate they become inaccurate, it may be necessary to alter the instruments themselves or the ways in which they are administered so that produce accurate information across repeated bookings.

The second direction highlighted by this dissertation is the need for further examination into whether pretrial risk assessment instruments perpetuate the racial bias that is already present in the criminal justice system. This includes bias both in the assessments produced by the instruments as well as bias in their application. The systematic review conducted for Chapter 4 revealed that only three pretrial risk assessment instruments have been evaluated for evidence of bias in predictive validity across four studies (Cohen & Lowenkamp, 2019; Danner, VanNostrand, Spruance, 2016; DeMichele, et al., 2018; VanNostrand, 2003). While these studies generally did not find evidence of racial bias, four studies are not enough to ensure there is no bias in these instruments. Where possible, it would be useful to employ longitudinal designs or interaction effects when examining the possibility of racial bias in predictive validity. Perhaps bias becomes more evident across repeated bookings or is more prevalent for specific subgroups of people (e.g., Black men). Further, we must look beyond predictive validity to examine whether there is evidence of disparate impact in the application of risk assessment results. It is

necessary to examine whether race interacts with the format in which risk is communicated to develop best practices for pretrial risk communication. It is also necessary to examine whether there is an interaction between race and risk score when it comes to how likely a person is to have different pretrial conditions assigned. For example, at low risk, are White and Black people equally likely to be released without conditions? Alternatively, at high risk, are White and Black people equally likely to be detained or to receive the same bail amount? We must rigorously examine pretrial risk assessment instruments for evidence of racial bias and disparate impact so that we can determine whether to confidently include them in pretrial reform efforts.

### **Conclusion**

Jail reform in general and pretrial reform specifically are in the national spotlight. Major news outlets like The Atlantic, Propublica, The New York Times, and The Washington Post have published articles and op-eds covering the need for reform as well as sharing views supporting or refuting various reform efforts (Angwin et al., 2016; Barabas et al., 2019; Bellware, 2020; Carlisle, 2018). This national attention brings with it the momentum and resources that are needed to implement reform. But, for these reform efforts to be truly effective, they must be rooted in a strong evidence-base. This dissertation helps build that evidence-base by examining a commonly used jail mental health screen in a novel way, helping to develop a more nuanced understanding of bias in current bail practices, and summarizing the state of the evidence on pretrial risk assessment instruments. Now is the time to capitalize on national support and implement evidence-based practices that will promote transparent, equitable, non-punitive decision making from jail booking to first appearance in court.



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