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The Pretrial Detention Penalty: A Systematic Review and Meta-Analysis of Pretrial Detention and Case Outcomes

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ABSTRACT

It has long been argued that defendants detained pretrial face more severe case outcomes than released defendants. Considering the magnitude, directionality, and significance of these findings, this article uses systematic review and meta-analysis to examine the average effect of pretrial detention on a series of case processing outcomes: conviction, guilty plea, dismissal, charge reduction, incarceration, and sentence length. Assessing 143 effect sizes across 57 studies that met the inclusion criteria, findings indicated that detained defendants face more severe outcomes, with the strongest effect on their likelihood of incarceration. Pretrial detention had a medium effect on convictions, guilty pleas, and dismissals, a smaller effect on sentence length, and a non-significant, small effect on charge reductions. The studies' effect sizes were heterogeneous, highlighting the importance of jurisdictional differences in policies and practices. Moderator analyses were used to assess this variation. Future research should examine how disparity cumulates via the pretrial detention penalty.

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In their study on bail and sentencing, Sacks and Ackerman (2014) asserted that pretrial detention “holds the potential to steer the course of the criminal case, with incarceration as the potential outcome” (p. 64). Researchers drew a similar conclusion nearly 60 years ago when the Vera Institute of Justice reported that New York felony defendants held in pretrial detention were more likely to be convicted and incarcerated than released defendants (Ares et al., 1963; Rankin, 1964). Examining misdemeanor defendants, Feeley (1979) similarly asserted that the “process is the punishment” as he examined how detained defendants are at a disadvantage through their case processing. Since then, the pretrial process has shifted substantially across the United States alongside the Bail Reform Act of 1966 and the Bail Reform Act of 1984. The 1966 Act created a presumption of release for defendants charged with non-capital crimes unless the court had concerns about a defendant’s court attendance. When defendants did require pretrial supervision, the least restrictive conditions were to be used. The 1984 Act, however, allowed courts to consider a defendant’s safety risk

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while making pretrial decisions in addition to their risk of missing future court proceedings. Defendants perceived as dangerous could now be denied bail and held in pretrial detention throughout their case processing. Even as pretrial policies shifted, studies have continued to find that detained defendants face more severe case outcomes than released defendants. These relationships persist in the U.S. and abroad, in state and federal jurisdictions, at the misdemeanor and felony levels, and across various statistical tests, contributing partly to the observation that “pretrial decisions determine mostly everything” (McCoy, 2007).

While many studies have identified a significant effect of pretrial detention on defendants’ case outcomes, there are some exceptions to this overall trend, and across these works, the magnitude of the pretrial detention penalty varies. Thus, it is unclear the extent to which pretrial detention impacts defendants’ case outcomes and, further, which case outcomes are most impacted by the pretrial detention penalty. The current study sheds light on this by identifying the average effect of pretrial detention on defendants’ adjudication and sentencing outcomes. I conduct a systematic review and meta-analysis of studies examining the relationship between pretrial detention and conviction, pleas, dismissals, charge reductions, incarceration, and sentence length. Systematic review and meta-analysis resolve potential issues of non-empirical reviews, such as selective inclusion of studies, subjective weighting of studies, and misleading interpretations of studies’ findings (Lipsey & Wilson, 2001). Empirical reviews reduce research biases by considering the findings’ magnitude, directionality, and significance. As the United States is currently in the third wave of bail reform, implementing risk assessments and reducing reliance on cash bail, this review can inform current pretrial reforms.

Background

Understanding the Pretrial Detention Penalty

Focal concerns theory helps frame the role of stereotypes in judicial decision-making and why pretrial detention is often associated with more severe case outcomes (Steffensmeier et al., 1998). This theory suggests that three concerns of court actors contribute to more severe punishments: (1) the blameworthiness of the defendant, (2) the protection of the community, and (3) the practical implications of incarceration (e.g., cost, bed space, removing a breadwinner from his or her family). Compared to released defendants, detained defendants may appear more blameworthy and as more of a danger if sentenced to probation. Detained defendants may wear jail clothing to court proceedings, which some court actors may associate with danger (Klein, 1997). In response, bail bond agents have informed their potential clients that they “might look ‘more guilty’ [i.e., blameworthy] appearing in court in an orange jumpsuit rather than their own clothes” (Page et al., 2019). Even decades later, defendants can struggle to maintain their hygiene while detained, which can contribute to outward or implicit bias during sentencing (Chevrier, 2021). Pretrial detention may also signal “danger and culpability” as defendants’ cases progress through the legal system, leading to harsher punishments (Sutton, 2013). Tartaro and Sedelmaier (2009) explain that the “more dangerous the offender is perceived by the judge, the harsher the sentence and the greater the likelihood of custodial sentence.”

From a logistical standpoint, as emphasized by the United States Supreme Court, “pretrial confinement may imperil the suspect’s job, interrupt his source of income, and impair his family relationships” (“Gerstein v. Pugh,” 1975, p. 114). Released defendants, however, can show the court that they can contribute to the community, maintain a job, abstain from criminal activity, be responsible by attending court, and overall, be a good candidate for a probation sentence. Discussed in St. Louis (2022), a United States Sentencing Commission (2010) survey of 639 federal district court judges highlights how these efforts are relevant during sentencing. As judges sentence within-range or depart from guidelines, most report the following factors as relevant in their decision-making: a defendant’s “post-offense rehabilitative efforts” (61% of judges for within-range sentencing, 70% departures), employment record (62%, 65%), family ties/responsibilities (57%, 62%), and community ties (46%, 49%). Being released pretrial helps produce this mitigating evidence used by sentencing judges to predict success in the community (Didwania, 2020), with court observation research emphasizing that defendants’ “efforts to rehabilitate” lead to less severe sanctions (Scott-Hayward & Ireland, 2022). Thus, by function of being incarcerated, pretrial detainees are less able to show the court that they can ‘turn their life around’ through rehabilitative efforts (Williams, 2003).

Being detained pretrial can also inhibit defendants’ defense preparations, impacting their subsequent case outcomes (Foote, 1954). The U.S. Supreme Court recognized this in *Barker v. Wingo* & 407 U.S. 514 (1972) when they stated that a detained defendant is “hindered by his ability to gather evidence, contact witnesses, or otherwise prepare his defense” (p. 533). Problems of this nature still exist today. In her article “Hell Is Trying to Visit My Jail Client” (2017), attorney Rebecca Boucher emphasized the many difficulties of working with detained clients. Chevrier (2021) further discussed the difficulty of building a case while detained, including when attorneys are assigned, physical access to defendants, building trust, and gathering and reviewing evidence. Accordingly, attorneys spend less time meeting with detained defendants than released defendants (Allan et al., 2005), with pretrial detainees often indigent and relying on overworked public defenders to argue their cases (Holmes et al., 1987). The difficulties surrounding representation and the coercive nature of time-served plea offers can lead to more severe case outcomes for detained defendants. As Feeley (1979) emphasized, “when the choice is between freedom for those who plead guilty and jail for those who want to invoke their right to trial, there really is no choice at all” (p. 206).

Finally, it is also plausible that previously identified effects of pretrial detention on defendants’ case outcomes overestimate the extent to which a pretrial penalty exists, suffering from selection bias. In other words, underlying factors may influence both the pretrial detention decision and subsequent decisions that scholars often cannot account for in their analyses. Recent research has used instrumental variable analysis (e.g., judge randomization) and propensity score matching to begin addressing concerns over causality and has also found that pretrial detention is associated with more severe case outcomes (e.g., Campbell et al., 2020; Dobbie et al., 2018). Nonetheless, additional research is needed to understand whether the pretrial detention penalty is causal or whether this robust finding is often over-estimated—an area the current study aims to explore.

Advantages of Systematic Review and Meta-Analysis

While scholars have published literature reviews and book chapters summarizing research on the pretrial detention penalty, non-empirical reviews are inherently limited. These reviews often focus on prior findings' significance and directionality as opposed to their magnitude, the sample size of the analyses, and the methodological rigor of the analyses. Scholarship on the pretrial detention penalty, for example, often discusses multiple studies that found significant effects of pretrial detention on later case outcomes—but those studies rely on the same dataset to draw their conclusions. For example, in the current review, 48% ($n=74$) of the 155 eligible studies analyzed the same two datasets. Moreover, non-empirical reviews often include only peer-reviewed journal articles, which may overestimate significant findings due to publication bias (Lipsey & Wilson, 2001).

Meta-analysis, on the other hand, generally considers published and unpublished studies in addition to the effects' significance, magnitude, and direction across studies.¹ Typically paired with a systematic review of the literature, meta-analysts use rigorous empirical methods to aggregate the research on a specific topic. This is a particularly useful tool for understanding the pretrial detention penalty. When identifying the average effect of pretrial detention on each case outcome, I can avoid “double-counting” findings that rely on the same dataset and give more weight to studies with larger sample sizes. I can also move beyond significance/directionality and explore the magnitude of the pretrial detention penalty. Is the overall effect of pretrial detention on each case outcome small, medium, or large? How much of an issue is the pretrial detention penalty, and how urgent is the need for reform? I am also able to compare the magnitude of the pretrial detention penalty across the various case outcomes to determine which outcomes are most impacted. Finally, I can conduct exploratory analyses of different factors that help explain why there is variation in effect sizes across studies. For instance, do studies using instrumental variable analysis identify weaker effects than studies using regression analyses, or have the effects of pretrial detention become stronger/weaker over time? The answers to these questions are important and can help inform current policy reforms surrounding pretrial justice. These questions, however, can only be answered through an empirical review of the prior literature. In short, systematic reviews and meta-analyses help overcome research bias when summarizing prior literature and can be replicated as additional studies are completed.

Objectives

The current review examines the average direct effects of pretrial detention on defendants' adjudication and sentencing outcomes. To my knowledge, this is the first

¹Unpublished studies, particularly theses and dissertations, may still suffer from publication bias. However, this bias is typically of a lesser extent than that of peer reviewed articles as evident by related meta-analyses. For example, in his review of the impact of race on sentencing outcomes, Mitchell (2005) found that effect sizes from published studies “exhibited substantively and statistically larger effect sizes” than those from unpublished studies. While including unpublished works begins to address publication bias, it does not resolve it entirely.

systematic review and meta-analysis on this topic. It is essential to learn more about the effects of pretrial detention on defendants, particularly given the millions of people's lives impacted and pretrial detention's high cost to taxpayers (Wagner & Rabuy, 2017; Zeng, 2019). As such, I assess if detained defendants are subject to more severe case outcomes than released defendants when controlling for offense type/severity and criminal history. These outcomes include conviction, guilty plea, dismissal, charge reduction, incarceration, and sentence length. I hypothesize that across the eligible studies:

H1: Detained defendants will be significantly more likely to be convicted than released defendants.

H2: Detained defendants will be significantly more likely to plead guilty than released defendants.

H3: Detained defendants will be significantly less likely to have their cases dismissed than released defendants.

H4: Detained defendants will be significantly less likely to have a severity-based charge reduction than released defendants.

H5: Detained defendants will be significantly more likely to be incarcerated than released defendants.

H6: Detained defendants will be sentenced to prison/jail for significantly longer than released defendants.

Finally, as the downstream consequences of pretrial detention may cumulate through adjudication and sentencing decisions, I also hypothesize that across the eligible studies:

H7: The average effect of pretrial detention will be larger for sentence outcomes than for adjudication outcomes.

Methodology

Inclusion Criteria

To be included in the current review, studies must have met the following criteria:

1. *Pretrial detention*. This is treated as a binary measure in the current study based on its operationalization in most of this literature. The few studies that measure the length of pretrial detention were omitted from the current review.
2. *Case outcomes*. Studies must assess any of the following outcomes: conviction, guilty plea, dismissal, charge reduction, incarceration, and sentence length. Conviction, guilty plea, dismissal, and incarceration are binary measures. Studies that trichotomize incarceration are included if the reference category

is non-custodial/probation sentences. Both binary and continuous measures of charge reduction are included.

3. *Analysis.* Following prior meta-analyses on the effect of race on case outcomes (Mitchell, 2005; Zane & Pupo, 2021), the studies must use multivariate regression and control for offense type/severity and criminal history. This reduces the risk of spurious findings related to pretrial detention and its effect on case outcomes.
4. *Sample.* Only studies focusing on United States criminal courts (state and federal) are included in the current review. Studies that combine juvenile and criminal courts are also excluded.
5. *Time period.* Studies using samples post-1990 were included. If a study included multiple years, with at least one being post-1990, it was included; however, only effect sizes of post-1990 data were analyzed.
6. *Language.* All studies must be published in English to be included in the current review.

Search Strategy

Studies were identified across multiple sources, with the final searches concluded in the fall of 2021. I began by screening abstract databases, including Criminal Justice (ProQuest); Criminal Justice Abstracts (EBSCOhost); National Criminal Justice Reference Service Abstracts; Sociological Abstracts (ProQuest); Social Science Premium Collection (ProQuest); PubMed Central; Dissertations and Theses Global (ProQuest); and Google Scholar.² Aligning with the eligibility criteria, the searches were restricted to studies published after 1990 and in English (when the options were available). I exported the .ris for all citations and imported them to DistillerSR, a software for automating literature searches.³

I next checked the websites of various criminal justice agencies and research institutes to identify more studies that met the eligibility criteria, particularly those not peer-reviewed. Some examples include the Vera Institute of Justice, Urban Institute, Sentencing Project, Pretrial Justice Institute, Arnold Ventures, Prison Policy Initiative, and Marshall Project. While some researchers may argue that only published, peer-reviewed studies should be included in meta-analyses, this raises concerns about

²I used the following to search for studies across each of these databases. Search 1 of 2: ("pretrial detention" OR "pre-trial detention" OR "pre-sentence detention" OR "presentence detention") AND (adjudicat* OR convict* OR plea OR "charge reduction" OR dismiss* OR sentenc* OR cumulative OR processing OR downstream OR "case outcomes"). Search 2 of 2: ("pretrial release" OR "pre-trial release" OR "pre-sentence release" OR "presentence release") AND (adjudicat* OR convict* OR plea OR "charge reduction" OR dismiss* OR sentenc* OR cumulative OR processing OR downstream OR "case outcomes").

³Despite extensive efforts to identify studies that met the eligibility criteria, it is possible that some works were unintentionally excluded. Authors may have used phrases such as "held in custody pretrial" rather than "pretrial detention," which would not have appeared in the search. The exclusion of these papers is unlikely given the entire papers were screened (e.g., literature review), and other steps were taken to identify eligible studies (e.g., eligible studies' references, experts). If relevant studies were excluded from the search, it is possible that given the overlap of data sources, they used the same data as identified studies and would have been omitted later in the process.

publication bias related to significant findings (Lipsey & Wilson, 2001). As such, both types of publications are included in the current review. After identifying studies that met the eligibility criteria, prior to removing studies using the same underlying data, I screened their references for any studies not identified through the database searches. Finally, I sent the list of eligible studies to experts in the field for review, including members of the American Society of Criminology's Pretrial Justice Working Group. All additional citations were also imported into DistillerSR.

Coding

The included studies were coded on the following criteria: author and date; dataset; outlet; location; study time period; sample size; type of analysis; case outcomes examined; and the operationalization of pretrial detention and other variables (e.g., criminal history). Outlet is comprised of four categories—journal article, book/book chapter, dissertation/thesis, and technical report—and also dichotomized into journal article or other. Location includes regions of the US: Midwest, Northeast, South, West, and multiple regions. Study time period consists of four categories: 1990s, 2000s, 2010s, and multiple decades. The type of analysis was coded as (1) single-level or multi-level and (2) regression, propensity score matching, or instrumental variable. Offense type/severity is comprised of three categories: type, severity, and both type and severity. Criminal history is coded as binary measures, continuous measures, and both measures. Finally, studies assessing sentence length were coded based on the operationalization of the dependent variable; this includes (1) sentence in days, (2) sentence in months, and (3) transformed sentence length (e.g., natural log). As a single coder, I coded a random sample of 15 studies twice to assess coding reliability. The studies were coded two months apart, and I did not reference the first coding when coding a second time (Lipsey & Wilson, 2001). I found no discrepancies between the time one and time two codings, so intra-rater reliability was 1. Distiller SR's Artificial Intelligence Screening identified articles that might have been miscoded; after screening their suggestions, I found that all articles were appropriately coded.

Analytic Strategy

To better understand the impact of pretrial detention on defendants' case outcomes, I conduct a systematic review and meta-analysis to examine the magnitude, direction, and significance of the average effect of pretrial detention. Specifically, I conduct six separate meta-analyses focusing on the following outcomes of interest: (1) conviction, (2) guilty plea, (3) dismissal, (4) charge reduction, (5) incarceration sentence, and (6) custodial sentence length.⁴ Across all meta-analyses, the unit of analysis is the effect size.

⁴While many studies examine the 'in/out' decision, where both prison and jail sentences are considered 'in,' other scholars have argued that prison and jail sentences are fundamentally different and should be analyzed separately (Holleran & Spohn, 2004). As such, I conduct sub-analyses for both sentence type and sentence length to examine only prison sentences and only jail sentences. However, the results of these analyses were largely similar to those of the combined prison/jail analyses (see Appendix D). A meta-analysis

An independent set of effect sizes is a crucial assumption of meta-analysis. While some studies may contain multiple analyses of the same sample of defendants (e.g., stepwise regression), the most sophisticated statistical analysis was selected for effect size calculation. This could include a more methodologically advanced analysis, such as instrumental variable, or a regression analysis with more variables included. Some studies include multiple regression analyses of the same sample with the same number of variables; in these instances, the two effect sizes were averaged.⁵ Other studies include multiple analyses of different sub-samples, such as white defendants and black defendants. I included both effect sizes in these instances as they meet the independence assumption. Finally, many studies analyze the same underlying data, such as the State Court Processing Statistics. To avoid statistical dependence, only one of the studies was included.⁶ To select the studies, I first removed dissertations and theses later published as peer-reviewed journal articles. Next, following a similar meta-analysis by Zane and Pupo (2021), I selected studies with a larger sample size. If two studies examined the same number of defendants, I included the study with more variables included. I used this “reductionist” approach of selecting one effect size per repeated samples as opposed to the “integrative” approach of including multiple effect sizes per repeated samples (López-López et al., 2018).

Odds ratios were chosen as the preferred effect size for the dichotomous case outcomes (e.g., conviction, incarceration). Nearly all included studies reported either the logged odds or odds ratios and the standard errors or 95% confidence intervals. A few studies reported coefficients from probit regression models, and some used OLS regression (or related analyses) to assess a dichotomous dependent variable. These coefficients and standard errors were converted to odds ratios (and related standard errors) following the steps outlined in a prior meta-analysis (Kochel et al., 2011; also see Hasselblad & Hedges, 1995). The standard mean difference effect size was used for the continuous dependent variables (e.g., sentence length). Similar to a related meta-analysis (Mitchell, 2005), the standard mean difference effect sizes were converted to the odds ratio scale (Lipsey & Wilson, 2001, p. 198; also see Hasselblad & Hedges, 1995).⁷ While most studies measure pretrial detention, some measure pretrial release; the latter were reverse-coded when calculating the effect size.⁸ All effect sizes were weighted using inverse variance weights, which account

focusing on jail sentence length could not be conducted as only three effect sizes from six studies were available.

⁵For example, Freiburger (2011) conducts two logistic regression analyses predicting the ‘in/out’ decision. These analyses feature the same sample and include the same number of variables. The first includes three variables related to caretaker roles and the second replaces those with three variables related to financial roles. I averaged the effect size of pretrial detention on the incarceration decision across the two models.

⁶However, if the studies analyze different years of the data, both were included (as long as there were no overlapping years). Moreover, if one study focused on only male defendants and a second study focused on only female defendants, both were included. If one study focused on drug defendants and another on male defendants, only one would be included as there would be overlap between the two samples (involving some statistical dependency). In short, multiple studies analyzing the same underlying data could be included if they focused on entirely different samples of this data.

⁷One study that used a Poisson count model to examine sentence length in months was therefore excluded as the estimates could not be converted to Cohen’s *d* or odds ratios with the information available.

⁸This involved converting the odds ratios and 95% confidence intervals to logged odds and standard errors, reversing the directionality of the coefficient, and converting the reversed logged odds and standard errors

for the variation in sample size between studies (Tau squared) by allowing larger-scale studies to carry more weight in the meta-analysis (Lipsey & Wilson, 2001). Following Olivier and Bell (2013), in the current review, odds ratios of approximately 0.82 or 1.22 are considered small, approximately 0.55 or 1.86 are considered medium, and less than 0.33 or greater than 3.00 are considered large.

After obtaining an independent set of effect sizes and their inverse variance weight, I conducted a homogeneity analysis, calculating a Q statistic to test the assumption that all effect sizes estimate the same population. If heterogeneous, I can assume the variation between effect sizes can be attributed to sampling error and differences between the studies' methodological rigor and populations of focus (Lipsey & Wilson, 2001). As the tests were significant, I used random-effects models to calculate pretrial detention's weighted mean effect on the outcomes. With a random-effects model, the weights account for both within- and across-study variance. The effect sizes are then screened for outliers that may distort the meta-analytic findings. This includes repeatedly re-running each analysis and removing one study each time to assess if the average effect sizes' magnitude, directionality, or significance change (Lipsey & Wilson, 2001).

The effect sizes were also screened for publication bias through a trim-and-fill procedure. This involves plotting each effect size by its logged odds ratio (X-axis) and standard error (Y-axis). The plotted effect sizes should resemble a funnel distributed around the mean effect size. If the effect sizes are not balanced on the left and right of the mean effect size, additional effect sizes can be imputed, thereby adjusting the average effect size (Lipsey & Wilson, 2001). Finally, I conducted a moderator analysis to examine the variation of effect sizes across the included studies. The moderators include the coded characteristics previously discussed (e.g., location, type of analysis), and the analysis includes a series of *t*-tests for each moderator. These analyses are all completed in Biostat Inc.'s Comprehensive Meta-Analysis v3 and v4 software.

Results

Systematic Review of Included Studies

Using the search strategy outlined above, I identified 21,708 papers across various databases and websites. DistillerSR screened for duplicate studies pulled from multiple databases using its Smart Quarantine feature. Any papers that matched the title, authors, and abstract with over 85% confidence were flagged as duplicates.⁹ A random sample of 50 studies was manually screened, and all were duplicates. As such, all studies flagged by DistillerSR were quarantined, or in other words, removed from the screening process. Additional duplicate studies were identified through the screening process and also quarantined. 13,745 distinct works remained for further review.

back to odds ratios and 95% confidence intervals.

⁹References below 85% tended to have excluded abstracts and were therefore only matched by title and author. To err on the side of caution, 85% was selected so that only references with duplicate titles, authors, and abstracts, were flagged.

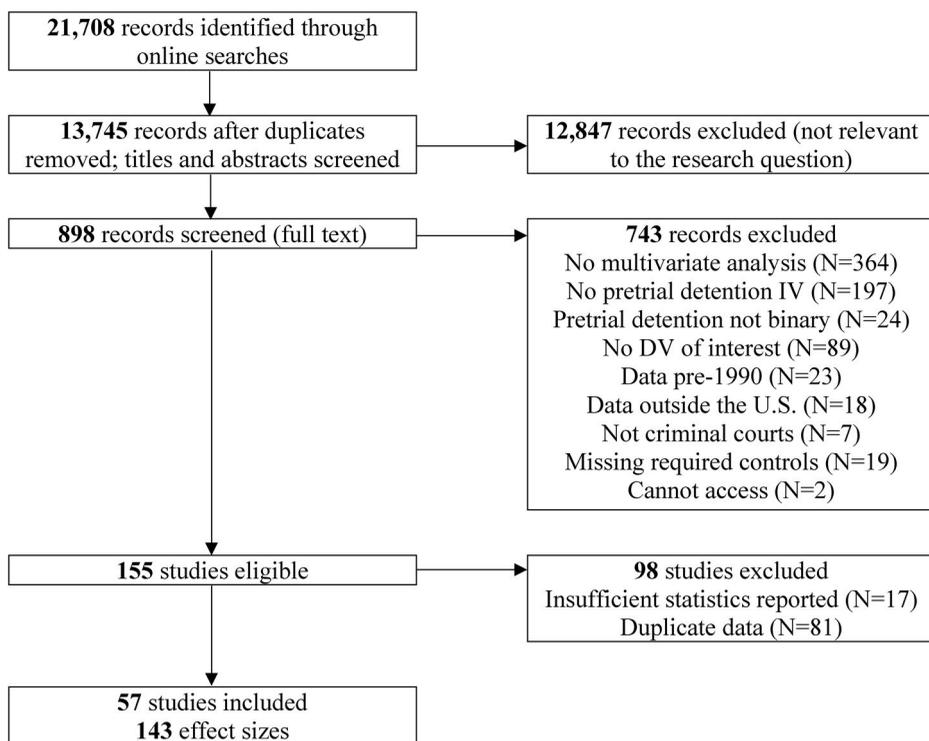


Figure 1. Study selection flowchart.

As seen in [Figure 1](#), the 13,745 works were reviewed in multiple stages to identify the 57 included studies.¹⁰ First, I screened the titles and abstracts of the 13,745 works to remove any irrelevant to the research question. This typically included studies that discussed a different topic (e.g., inmate misconduct), relied on non-U.S. data, analyzed responses to vignettes, were not published in English, or were non-empirical works. Of the 13,745 works, 898 potentially relevant studies remained, of which I screened their full text. Many of these works did not meet the eligibility criteria, such as not measuring pretrial detention, missing key control variables, focusing only on the juvenile justice system, and exclusively conducting bivariate analyses. Of the 898 studies, 155 studies were found to meet the eligibility criteria. However, it was then determined that many of these studies relied on the same underlying data, with authors publishing multiple studies using their original data or relying on publicly available datasets (e.g., State Court Processing Statistics). After screening for duplicate data, the final sample analyzed in the current review includes 143 independent effect sizes across 57 studies. The 57 studies were selected following the previously outlined procedure, prioritizing larger samples and more variables included in the analyses.

¹⁰The number of identified works was rather large due to the many different dependent variables examined in the current review. A recent meta-analysis by Zane and Pupo (2021) also examined multiple case processing stages when assessing the impact of a juvenile's race on their case processing. Given the wealth of research on racial disparity, they collected 39,109 references using their search strategies and ultimately included 67 studies (0.17%). This is similar to the current review where 0.41% of the screened references were ultimately included in the meta-analysis.

Table 1. Descriptive statistics of the included studies ($N=57$).

	%	n
<i>Outlet</i>		
Journal article	64.91	37
Book/book chapter	5.26	3
Dissertation/thesis	26.32	15
Agency report	3.51	2
<i>Location</i>		
Midwest	29.82	17
Northeast	14.04	8
South	19.30	11
West	3.51	2
Multiple regions	33.33	19
<i>Jurisdiction</i>		
State	84.21	48
Federal	15.79	9
<i>Time Period¹</i>		
1990s	29.82	17
2000s	31.58	18
2010s	33.33	19
<i>Level of analysis</i>		
Single-level	71.93	41
Multi-level	28.07	16
<i>Type of analysis</i>		
Regression	92.98	53
Propensity score matching	1.75	1
Instrumental variable	5.26	3
<i>Offense type/severity</i>		
Type only	33.33	19
Severity only	5.26	3
Both type and severity	61.40	35
<i>Criminal history</i>		
Binary	21.05	12
Continuous	33.33	19
Both binary and continuous	45.61	26

¹3 studies have an equal number of years falling in two decades.

See Appendix A for the list of included studies, Appendix B for a list of eligible but excluded studies, and [Table 1](#) for descriptive statistics.

Nearly two-thirds of the 57 studies were journal articles (65%), 26% dissertations or theses, 4% agency reports, and 5% books or book chapters. Related meta-analyses have yielded similar estimates, such as Zane and Pupo (2021), who found that 62% of their included studies were published in peer-reviewed journals. In the current review, approximately a third of the studies' data spanned multiple regions of the country (33%), which is unsurprising given the availability of large-scale, nationwide datasets. Approximately 30% of studies exclusively used Midwest data, 14% Northeast data, 19% South data, and only 4% data from the Western region of the U.S. Most of the studies analyzed state-level data (84%) as opposed to federal-level data (16%). A similar number of studies used data from each time period: 30% predominately focused on the 1990s, 32% the 2000s, and 33% the 2010s. Three multi-year studies included an equal number of years from two decades, two spanning the 1990s and 2000s and one spanning the 2000s and 2010s. Most included studies ran single-level analyses (72%), though some ran multi-level analyses (28%). Relatedly, most conducted regression analyses (93%) rather than instrumental variable (5%) or propensity score matching analyses (2%). The included studies typically controlled for both offense type and severity (61%) or only offense type (33%). Few studies controlled for only offense

severity (5%). Finally, approximately 46% of the studies controlled for both binary and continuous measures of criminal history, 33% only continuous, and 21% only binary.

Meta-Analytic Findings

Across each of the 57 studies, one or more weighted effect sizes were generated to measure the impact of pretrial detention on the following case processing outcomes: (1) conviction, (2) guilty plea, (3) dismissal, (4) charge reduction, (5) incarceration sentence, and (6) custodial sentence length. The results of the six separate meta-analyses are reported in Table 2, which includes the weighted odds ratios (OR), 95% confidence intervals, weighted squared deviations (Q), standard deviation of the effect sizes (Tau), the percentage of variation attributed to heterogeneity (I²), and the number of studies examining each outcome. To identify outlier studies, each study was removed from the meta-analyses one by one (Lipsey & Wilson, 2001). Outlier studies affected only three meta-analytic findings, which are discussed further below.

Beginning with convictions (Figure 2) and guilty pleas (Figure 3), findings indicated a medium effect of pretrial detention on both outcomes, where detained defendants were more likely to be convicted and plead guilty. Detained defendants faced 102% higher odds of conviction (OR = 2.02; CI 1.65–2.46; *p* < 0.001) and 104% higher odds of pleading guilty (OR = 2.04; CI 1.66–2.50; *p* < 0.001) than released defendants. No evidence of publication bias was identified in the conviction analysis; however, one

Table 2. Mean effect sizes and heterogeneity statistics.

	OR	95% CI	Q	Tau	I ²	k
Conviction	2.03***	[1.68, 2.46]	3426***	0.29	99.74	10
Plea	2.04***	[1.66, 2.50]	4955***	0.35	99.78	12
Dismissal	0.61*	[0.38, 0.99]	783***	0.53	99.23	7
Charge reduction	0.72	[0.52, 1.02]	2608***	0.69	99.39	17
Incarceration	3.50***	[2.72, 4.50]	18,816***	0.71	99.83	33
Sentence length	1.29**	[1.06, 1.56]	4072***	0.35	99.68	14

Notes. Random effects model. OR=odds ratio. CI=confidence interval. **p* < 0.05, ***p* < 0.01, ****p* < 0.001.

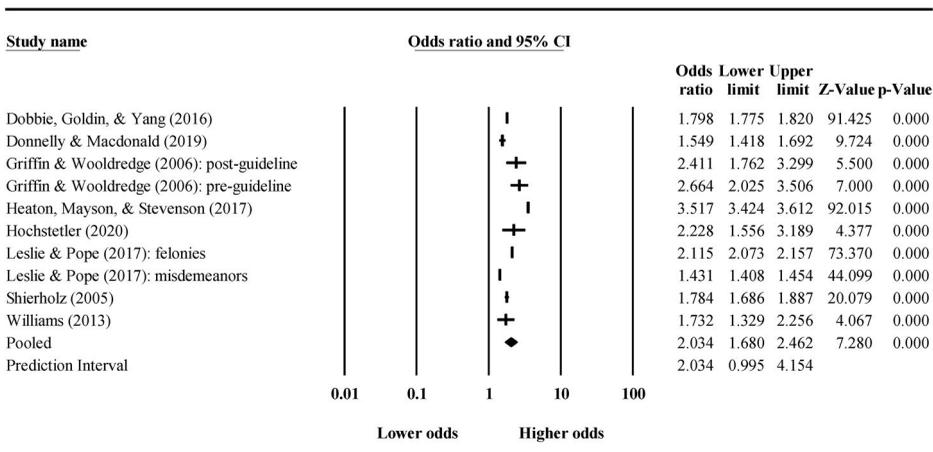


Figure 2. Forest plot of conviction effect sizes.

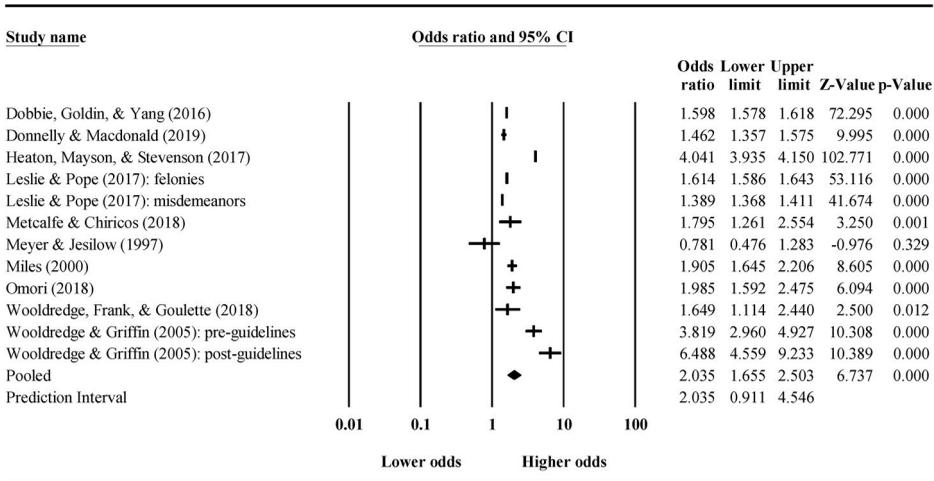


Figure 3. Forest plot of plea effect sizes.

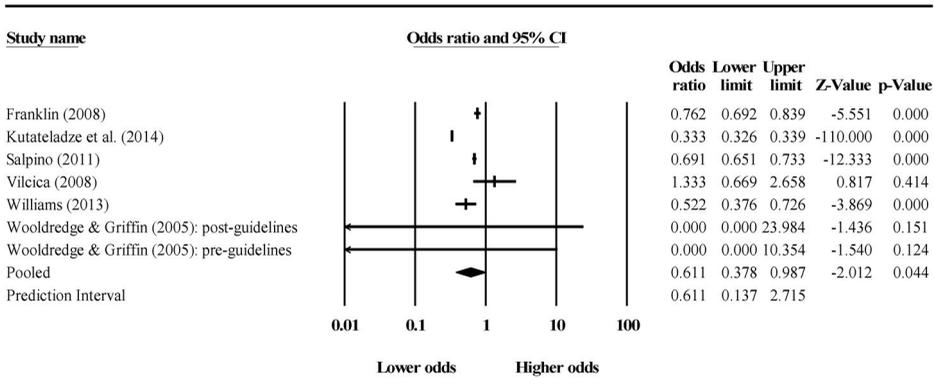


Figure 4. Forest plot of dismissal effect sizes.

effect size was imputed to the right of the mean effect size in the plea analysis, leading to a slightly stronger adjusted effect size (OR = 2.17; CI 1.77–2.65). Notably, every effect size in the conviction analysis, and all but one in the guilty plea analysis, was significant and in the anticipated direction. In both analyses, the magnitude of the pretrial detention penalty varied across studies, with the distribution of effect sizes being heterogeneous. Most of the variation across studies examining convictions ($Q=3426$; $I^2=99.7\%$) and pleas ($Q=4955$; $I^2=99.8\%$) could be attributed to fundamental differences, meaning these differences in magnitude cannot be attributed to chance alone.

Unlike conviction and guilty pleas, results revealed only a small-to-medium effect of pretrial detention on dismissals (Figure 4) and charge reductions (Figure 5). These effects were both border-line significant, where detained defendants were less likely to receive both dismissals (OR = 0.61; CI 0.38–0.99; $p < 0.05$) and charge reductions (OR = 0.72; CI 0.52–1.02; $p < 0.07$). Due to some evidence of publication bias, two effect

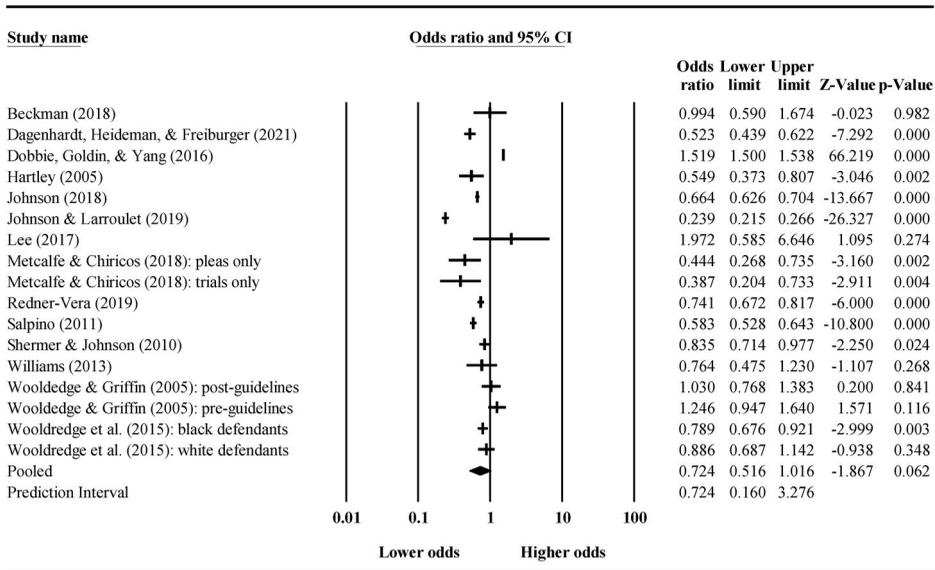


Figure 5. Forest plot of charge reduction effect sizes.

sizes were imputed to the right of the dismissal mean effect size, and one effect size was imputed to the left of the charge reduction effect size. The adjusted effect sizes for both dismissals (OR = 0.55; CI 0.33–0.92) and charge reductions (OR = 0.70; CI 0.50–0.97) were similar to the original estimates. The directionality and significance of the pretrial detention effect were consistent across four of the seven studies assessing dismissals, with the remaining three not identifying a significant relationship. Outlier analyses revealed that three different studies could be removed where the effect of pretrial detention on dismissal would no longer be significant—though it would be similar in magnitude. While the directionality of the pretrial detention effect was also fairly consistent across the charge reduction analysis, its significance varied across the included studies. Omitting three different studies from this analysis would lead to a significant average effect size of pretrial detention on charge reduction, though the magnitude of this effect size would remain small-to-medium. Similar to the prior outcomes, the magnitude of the effect sizes was heterogeneous. The I^2 statistic indicates that over 99% of the variation is attributed to actual differences between studies for both dismissals ($Q=783$; $p<0.001$) and charge reductions ($Q=2608$; $p<0.001$).

Pretrial detention had the strongest effect on the ‘in/out’ decision, where detained defendants had 236% higher odds of being incarcerated in prison/jail (OR = 3.50; CI 2.72–4.50; $p<0.001$). Further analyses indicated some evidence of publication bias, and as such, four studies were imputed to the left of the average incarceration effect size. The adjusted average effect size was slightly weaker than the original effect size, but it remained the strongest effect of pretrial detention on any of the case processing stages analyzed (OR = 3.04; CI 2.38–3.87). The direction of the pretrial detention effect was consistent across all of the included studies, and while most of the effect sizes were significant, some studies did not find a significant relationship between pretrial detention and incarceration (Figure 6). Moreover, there was

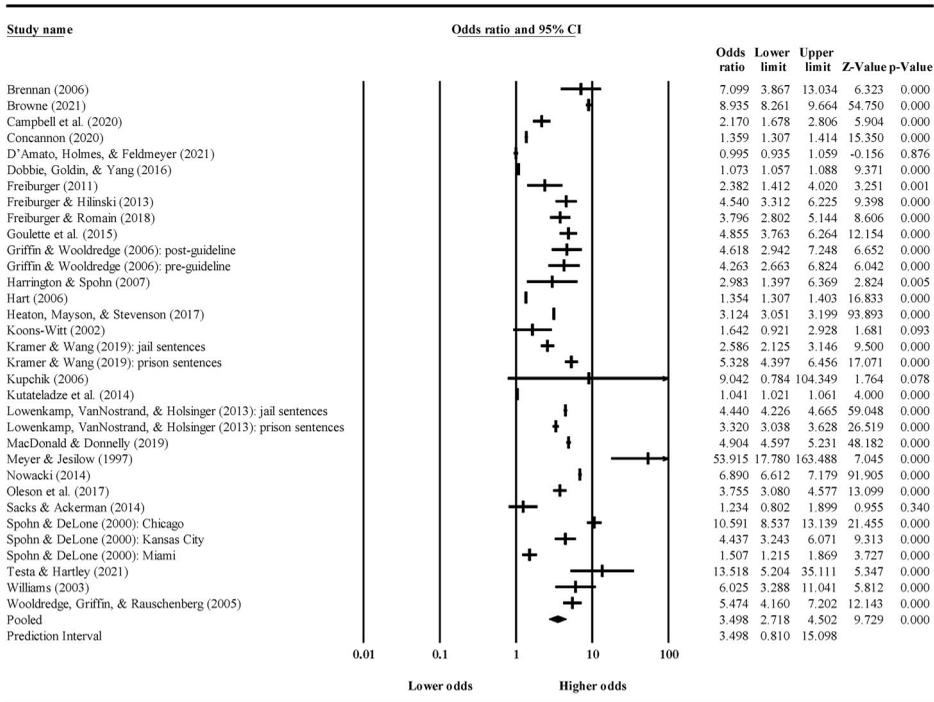


Figure 6. Forest plot of incarceration effect sizes.

substantial heterogeneity in the magnitude of the pretrial detention penalty across the included studies concerning all three outcomes. Over 99% of the variation ($I^2 = 99.8\%$) could be attributed to real differences between the studies ($Q = 18,816; p < 0.001$).

Despite the large effect of pretrial detention on defendants' likelihood of an incarceration sentence, pretrial detention had only a small effect on defendants' custodial sentence lengths (OR = 1.29, CI 1.06–1.56; $p < 0.01$). No evidence of publication bias was identified in the sentence length analysis. Except for three effect sizes stemming from two studies in the same state, the directionality of these findings was consistent where detained defendants face longer custodial sentences than released defendants (Figure 7). The pretrial detention penalty was significant in all but two of the included studies. Notably, outlier analyses revealed that if one study were removed, Kim et al. (2016), the effect of pretrial detention on sentence length would no longer be significant. Similar to all of the other outcomes, there was substantial variation in the magnitude of the pretrial detention effect; the I^2 statistic indicates that approximately 99.7% of the variation ($Q = 4072; p < 0.001$) stemmed from actual differences between the studies.

Moderator Analyses

Given the high heterogeneity of effect sizes at every case processing outcome, I next use a series of moderator analyses to examine factors that may explain the variation

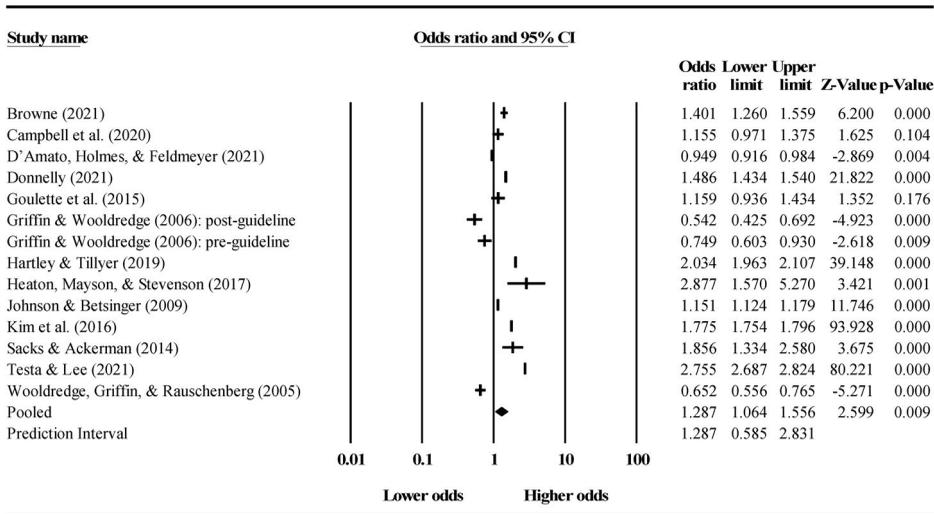


Figure 7. Forest plot of custodial sentence length effect sizes.

across the studies' effect sizes (see Appendix C). Given the small number of studies included in some of the meta-analyses, such as the sample of seven dismissal effect sizes, the moderator analyses should be interpreted cautiously and not as causal (Lipsey, 2003). Nonetheless, they provide information on interesting patterns and trends among the various effect sizes.

Beginning with tests for publication bias, effect sizes for studies published in academic journals were generally not significantly different than those published in other forms, such as dissertations, book chapters, and technical reports. Publication type was not associated with the heterogeneity of effect sizes on nearly all case processing outcomes, except for dismissals, where journal articles reported larger effect sizes. Both measures for type of analysis were also not associated with the heterogeneity of effect sizes across all outcomes. Specifically, there were no significant differences in the average effect size among studies that conducted single- versus multi-level analyses, nor were there substantive differences between studies that used regression versus instrumental variable/propensity score matching.¹¹

Interestingly, the location, jurisdiction, and time period of the studies' data, as well as the coding of criminal history and sentence length, were associated with the heterogeneity of effect sizes across studies for some of the case processing outcomes. Studies that used midwestern data identified stronger effects of pretrial detention on conviction, pleas, dismissals, and incarceration, though not sentence length. Moreover, studies that used federal-level data identified significantly stronger effects of pretrial detention on sentencing (including incarceration and sentence length) than studies that used state-level data. The effect of pretrial detention on case outcomes appears to have shifted over the past few decades. Across the dismissal and sentence length

¹¹Although type of analysis was significantly associated with charge reduction, the finding is not substantively significant given that 16 studies used regression analyses and only one instrumental variable analysis. Future works should further explore this variation given the conflicting directionality of the findings.

analyses, effect sizes were significantly smaller in datasets focusing primarily on the 1990s.

Concerning the studies' coding of criminal history, the effect of pretrial detention on convictions, guilty pleas, charge reductions, and sentence length was the strongest in studies that included a continuous criminal history variable. In many of these works, the continuous measure was a factor score that combined multiple criminal history measures. Except for the incarceration decision, a binary measure of criminal history appears to be associated with the smallest effect sizes.¹² Generally, whether a study controlled for offense type, offense severity, or both measures did not significantly impact the magnitude of the effect sizes. Finally, among studies assessing sentence length, the magnitude of the pretrial detention effect varied based on sentence length's operationalization. Pretrial detention was significantly associated with sentence length in studies that used a transformed variable (e.g., natural log); however, pretrial detention was not a significant predictor of sentence length across studies that measured it in months.

Discussion

Findings supported nearly all hypotheses where detained defendants faced more severe case outcomes than released defendants. However, the magnitude of the pretrial detention penalty differed across the various outcomes and only approached significance in one of the analyses. Across the included studies, detained defendants were much more likely to be incarcerated than released defendants. They also were more likely to be convicted and plead guilty, less likely to have their cases dismissed, and received longer custodial sentences. These findings support the argument that pretrial detainees are at a disadvantage in their case processing compared to their released counterparts. Detained defendants may struggle to prepare their defense and meet with their attorneys, as well as lose their jobs and harm their relationships, making them appear a risk if released on probation. As such, detained defendants may be perceived as more blameworthy and dangerous than released defendants and face these more severe outcomes.

Pretrial detention had the strongest effect on the 'in/out' decision (despite how 'in' was measured). As detained defendants are already held in jail at the time of their sentencing and removed from their communities, jobs, and families, they may be perceived as a "less suitable candidate for probation" compared to released defendants (Wooldredge et al., 2015, p. 190). Moreover, given the sequential nature of case processing, earlier decisions that negatively impact detained defendants (e.g., more likely to plead guilty) may cumulate to disadvantage detained defendants at sentencing. These cumulative disadvantages of pretrial detention may contribute to the strong effect of pretrial detention on the incarceration decision. It is also possible that many detained defendants plead guilty to time-served sentences, which may be coded as

¹²The studies utilize a variety of binary criminal history variables, such as prior arrest, prior conviction, prior prison, and on probation/parole. Moreover, some studies use 0 vs 1+ coding whereas others use 0-1 vs 2+ coding. Other studies specify the felony level, time frames, or certain types of offenses. Future meta-analysis with a larger sample size should investigate these differences further.

incarcerated across the various studies. Had these defendants been released, those convicted of less severe crimes might have instead been sentenced to probation alongside intermediate sanctions. Time-served sentences, particularly those where a defendant can return home upon conviction, can push even innocent defendants to plead guilty (e.g., Euvrard & Leclerc, 2017; Kellough & Wortley, 2002; Sacks & Ackerman, 2014). For this reason and others, such as the difficulties of preparing a defense while detained, it was unsurprising that pretrial detention had a medium effect on convictions and guilty pleas.

Pretrial detention also had a small-to-medium effect on dismissals. Prosecutors may be hesitant to dismiss charges against defendants who have already been held in jail, given their loss of liberty during the pretrial process, and when even innocent detained defendants opt to accept guilty pleas, dismissals would be lessened accordingly. On the contrary, given the loss of freedom associated with pretrial detention, many state statutes specify that the timeline associated with one's right to a speedy trial is shorter for detained defendants than released defendants (Loginsky, 2002). With less time available to meet the speedy trial requirement, prosecutors may be forced to dismiss detained defendants' charges if enough evidence has not been collected in the allotted time. These conflicting perspectives may contribute to the smaller effect size of pretrial detention on dismissals relative to incarceration, conviction, and guilty pleas. The effect of pretrial detention on sentence length was also significant but small. This may be attributed to structured sentencing where, if convicted and sentenced to incarceration, a defendant's custodial sentence length has little room for adjustment based on pretrial status. Finally, pretrial detention had a small, non-significant effect on charge reductions. Released defendants may benefit from charge reductions, especially when they can afford private attorneys with the time and resources to negotiate for reduced charges or prepare a strong defense. However, detained defendants may also benefit from charge reductions during plea bargaining, potentially leading to a lack of significant differences between the two groups' odds of receiving a charge reduction.

For each case processing outcome, the effect sizes across the included studies were highly heterogeneous; over 98% of the variation in pretrial detention effect sizes reflected real, rather than spurious, differences between the studies. The studies' location, jurisdiction, and time period of focus, in addition to their coding of criminal history and sentence length, helped explain some variation in effect sizes across studies. This may also be attributed to the different policies and practices of jurisdictions across the U.S. and their policies concerning pretrial, adjudication, and sentencing decisions. In other words, while the current review identifies a nationwide issue of the pretrial detention penalty, the extent to which this disparity occurs may vary based on the context in which a defendant is processed. Criminal justice reforms are occurring across the U.S., with jurisdictions repealing their tough-on-crime policies, re-thinking their reliance on cash bail, and responding to the COVID pandemic, so this contextual variation in the pretrial detention penalty may be even more prevalent today. As additional research is conducted and the current review is replicated, meta-regression may be a valuable tool for further exploring these relationships.

Limitations

Some studies that met the eligibility criteria did not report enough information to generate effect size estimates. For example, many studies examined both the incarceration decision and sentence length but reported descriptive statistics for only the incarceration decision sample (with the sentence length analysis using a reduced sample of incarcerated people). It is possible that the inclusion of these analyses would impact the average weighted effect sizes, though these studies were generally replaced with other works using the same dataset. Additionally, most included studies did not specify when the pretrial detention data were collected. Whether the data were pulled within a few days of arrest, during adjudication, or at another stage may impact the effect size estimates. Of those that did include this information, they were not enough to make meaningful comparisons. For instance, Rempel et al. (2017) identified a much larger effect size of pretrial detention on conviction for defendants held the entire pretrial period than for defendants detained specifically at arraignment. Thus, whether the defendants were held for a few hours, days, or months may impact the average effect sizes. Studies have found that defendants detained for more days face more severe case outcomes, meaning that the length of time a defendant is held in pretrial detention is an important consideration (Martinez et al., 2020). Further, many studies assessing sentencing did not outline how time-served sentences were coded. Effect sizes may be stronger in studies that include time served sentences in their incarceration and sentence length variables and smaller for those that do not, as emphasized by sensitivity analyses conducted in related works (Campbell et al., 2020; Phillips, 2007, 2008).

The quality of a meta-analysis depends on the included studies, meaning the limitations present in those studies extend to the current review. With few of the included works using quasi-experimental methods, such as instrumental variables, some studies may be affected by omitted variable bias. Although the studies' analyses needed to control for offense type/severity and criminal history to be included in the current review, other potentially important measures, such as quality or quantity of evidence, were not controlled for in most studies. As such, the relationships identified in the current study may be impacted by selection bias, with detained defendants not being similarly situated to released defendants. The average effects presented here should thus be understood as suggestive associations rather than causal effects. While the lack of significant differences in effect sizes between the regression and instrumental variable/propensity score matching analyses is promising, the potential impact of selection bias on the pretrial detention penalty is a critical area in need of additional research.

Finally, the current study focused on the most researched case outcomes, but due to feasibility and limited prior studies, it could not examine every possible court decision (e.g., diversion, departures). Therefore, it is recommended that scholars collecting original data strive to examine additional case processing stages, include under-studied measures (citation omitted), use quasi-experimental methods when possible, and that as this body of research further develops, a follow-up systematic review and meta-analysis is completed.

Implications

This paper has shed light on pretrial detention's effect on defendants' case outcomes that were difficult to discern without an empirical review. Nearly every included study focusing on convictions, guilty pleas, and incarceration identified a significant effect of pretrial detention. However, pretrial detention's effect on dismissals, charge reductions, and sentence length appeared more inconsistent, given that some studies did not yield significant results. Meta-analysis, however, moved beyond this focus on significance and also considered the magnitude and directionality of the pretrial detention effect across studies. By doing so, it was revealed that pretrial detention substantially impacts defendants' likelihood of incarceration in jail and prison and their likelihood of conviction or pleading guilty. As many more studies focused on sentencing outcomes compared to adjudication outcomes, it is recommended that future works continue to explore the relationship between pretrial detention and convictions, pleas, dismissals, charge reductions, and also jail sentence length, and that this review is then replicated.

Having found that detained defendants generally face more severe case outcomes than released defendants—and that when they do, the effect of pretrial detention is strong at certain stages—I recommended that we reduce the number of people held in pretrial detention. Most people detained pretrial are held on cash bail rather than denied bail (Reaves, 2013). Nearly half of adults in the United States cannot afford emergency expenses of \$400 (Federal Reserve Board, 2016), so affording pretrial release can be difficult for defendants. As such, the number of people detained pretrial may be drastically reduced by eliminating cash bail or considering a defendant's ability to pay when assigning bail. Several states have overhauled their bail systems accordingly, with some states setting bail based on a defendant's ability to pay and other states eliminating cash bail for most low-level offenses. For example, courts in Massachusetts (*Brangan v. Commonwealth* 2017) and Texas (*O'Donnell v. Harris County* 2017) ruled that cash bail must be set relative to a defendant's financial status, as not doing so could lead to the long-term detention of disadvantaged defendants.

In addition to re-evaluating the constitutionality of bail and appointing more progressive prosecutors and judges, pretrial populations can be reduced through other policy changes, such as citations in lieu of arrest, a presumption of pretrial release, and providing defendants counsel during pretrial proceedings (Baughman, 2017; Page & Scott-Hayward, 2022; Scott-Hayward & Fradella, 2019). In approximately 32 states, indigent defendants do not have counsel physically present at the initial appearance (Gross, 2017). Lack of counsel, combined with automatic bail setting based on the charged offense, often gives the prosecution the upper hand during pretrial proceedings, contributing to unnecessarily high bail amounts and the over-use of pretrial detention (Baughman, 2017; Page & Scott-Hayward, 2022). In addition to providing counsel at pretrial proceedings, supporting defendants through the pretrial process (e.g., childcare, transportation) can help increase court appearance, decrease re-offending, and decrease pretrial populations. For example, court notification programs, such as redesigned summons forms, text messages, and phone calls, have effectively encouraged court appearance even as more defendants are released pretrial (Ferri, 2020; Fishbane et al., 2020).

By lessening the number of people held in pretrial detention, the approximately \$13.6 billion annual costs of pretrial could potentially be invested into other evidence-based programs (Wagner & Rabuy, 2017). As the fines and fees associated with the pretrial process disproportionately impact disadvantaged populations, in effect “punishing poverty” (Scott-Hayward & Fradella, 2019), reducing pretrial populations can also lessen the financial and emotional burdens that the pretrial process places on defendants and their loved ones. These burdens are particularly felt by female family members who may forgo basic necessities to afford bail, pay-to-stay fees, jail phone calls, and more to support their unconvicted loved ones (Page & Scott-Hayward, 2022).

Defendants that are detained must not be perceived as more blameworthy and dangerous through their case processing by virtue of their pretrial detention. In the current review, only studies that controlled for offense type or severity and criminal history were included, yet there remained a strong effect of pretrial detention on various adjudication and sentencing outcomes. As such, detained defendants should have opportunities to display “post-offense rehabilitative efforts” so the court may, in turn, view these defendants as less of a threat to public safety and able to be successful on probation. This may be accomplished through work-release programs or matching defendants with remote jobs they could continue with if/when released. Moreover, to help defendants sustain their family ties and better prepare their defense, reforms may include more visiting-based technology, better products to promote hygiene, and access to their cell phones while meeting with their attorneys.

Pretrial reform is further needed due to the disparities in these subjective decisions. The pretrial stage has been described as the “toughest” because “you really don’t know a lot about the case so you have to consider as much information as you have in front of you and, um, do your best” (Clair & Winter, 2016, p. 345). As such, stereotypes associated with blameworthiness and dangerousness can lead specific subgroups of defendants, namely young black and Latino males, to be detained at a higher rate (e.g., Demuth, 2003; Wooldredge et al., 2015). In fact, in a recent systematic review and meta-analysis of race/ethnicity and juvenile justice outcomes, Zane and Pupo (2021) found that black and Hispanic juveniles were more likely to be detained pretrial than white juveniles and that out of all the case processing outcomes examined, race/ethnicity had the strongest effect on pretrial detention. As such, pretrial decisions can contribute to cumulative disadvantage in the criminal legal system when black and Latino defendants are more likely to be detained pretrial, and detained defendants face more severe case outcomes. In other words, “as the person moves through the system, [disproportionalities] add up to substantial, and often statistically significant, disparities in the processing and outcomes for different social groups” (Zatz, 1987, p. 76). Recent studies have found that pretrial detention contributes greatly to cumulative disadvantage (e.g., Kramer & Wang, 2019; Kutateladze et al., 2014; Omori, 2018; Schlesinger, 2007; Spohn, 2009; Sutton, 2013; Wooldredge et al., 2015), further emphasizing the importance of the pretrial detention penalty and the urgent need for reform. Notes

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