The Fallout from Criminal Justice System Contact

HEDWIG LEE, ALEXANDRA GIBBONS, GARRETT BAKER, AND CHRISTOPHER WILDEMAN

Twenty-five years ago, Bruce Western and Katherine Beckett (1999) provided the seed of what would come to be a novel area of inquiry: the consequences of the carceral state for inequality. In this article, we review in four stages the last twenty-five years of research on the fallout from criminal justice system contact. In the first stage, we describe the contours of the carceral state to highlight how prevalent each level of criminal justice contact is today relative to earlier historical eras and to each other and how unequally distributed these levels of criminal justice contact are by race, ethnicity, and class. In the second stage, we consider the questions often left unaddressed in prior work, including our own prior work: why might we expect racial differences in the effects of criminal justice contact, and are there racial differences in the effects of criminal justice contact? In the third stage, we provide a discussion of the datasets and methods used to consider these relationships. In the fourth stage, we consider the direct and vicarious effects of contact with the police, experiencing prison or jail incarceration, and being placed on community supervision using evidence spanning several disciplines. By providing a review that is exhaustive in terms of levels of criminal justice contact, limitations of data and methods, and the existence of race-specific effects, we offer a comprehensive description of the state of scientific research on the scope and scale of criminal justice contact and its collateral consequences for inequality in the United States.

Keywords: incarceration, criminal justice system, inequality, race, collateral consequences

Twenty-five years ago, Bruce Western and Katherine Beckett (1999) famously made the argument that the penal system was a core labor market institution in the United States. In crafting their argument, they relied on four empirical facts. First, far more Americans per capita are in prison or jail on any given day than ever before in the US, and the US imprisons more people than do other developed democracies. Second, far more Black Americans than White Americans per capita are in prison or jail on any given day. Third, counting individuals who

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are incarcerated (who are not included in the household-based surveys used to calculate labor market participation rates) as jobless fundamentally changes the employment rate—and racial disparities in the employment rate—in the US. Indeed, doing so leads to rates of joblessness in the US that more closely align with those of other developed democracies. And, perhaps more alarmingly, doing so shows that much of the progress Black men had appeared to make in the labor market through the 1990s was a function of failing to count incarcerated Black men as jobless rather than of real improvements in the labor market. Fourth, incarceration appears to decrease employment for at least several years after release and does so in similar ways for Black and White men, suggesting that the penal system may also exacerbate long-run earnings disparities.

While there was certainly research on the prevalence and consequences of penal system contact before Western and Beckett's (1999) seminal work, the publication of this study provided what can be considered the key moment in launching the literature on what has come to be called the "collateral consequences" of mass incarceration.[1] In reviewing the now-massive literature that Western and Beckett (1999) spawned, we are attentive to a few key ways in which this literature has evolved. First, while much early research in this area focused on incarceration or on felony convictions (Pager 2003), recent research considers many other forms of entanglement with the system, ranging from being stopped by the police to being placed under community supervision. Second, while much early research in this area focused on direct effects of criminal justice contact, research has turned to considering the ramifications of indirect criminal justice contact—both familial (such as parent or sibling) and vicarious (such as peer or neighborhood level). And finally, while much early research in this area focused on labor market outcomes, research has now turned to testing for effects in such a vast array of areas that it is, to be blunt, dizzying for many of us working in this area to keep up. Together, these developments have produced a formidable literature expanding beyond "collateral consequences" on what we will here call the fallout from criminal justice contact.[2]

We proceed in four stages. In the first stage, we describe the contours of mass criminal justice contact—what some have come to call the carceral state (Wakefield and Turney 2025, this issue)—to highlight how prevalent each level of criminal justice contact is today relative to earlier historical eras and to each other and how unequally distributed each level of criminal justice contact is by race, ethnicity, and class. In the second stage, we build on these descriptive findings to consider the questions so often unaddressed or inadequately addressed in prior research—including in our own prior work—in this area: are there racial differences in both the prevalence of criminal justice contact and its effects, and what are the ensuing implications? In the third stage, we provide an extensive discussion of the datasets and suite of methods used to consider these relationships. In the fourth stage, we consider the direct and indirect effects of contact with the police,[3] experiencing prison or jail incarceration, and being placed on community supervision on a range of outcomes spanning sci-

1. It should be noted that this is also the year that Hagan and Dinovitzer (1999) provided the first review of some of the effects of incarceration on families and communities that has become core to the field in the last fifteen years.

2. Collateral consequences have traditionally been used to describe "legal and regulatory restrictions that limit or prohibit people convicted of crimes from accessing employment, business and occupational licensing, housing, voting, education, and other rights, benefits, and opportunities" (National Inventory of Collateral Consequences of Criminal Conviction 2023). As such, they tend to focus on more formal legal restrictions rather than broader effects.

3. One key feature of our review is an intensive discussion of what exactly one should consider reasonable evidence in this space, with an emphasis on how one should think about studies that have stronger and weaker designs for detecting causal effects and studies that generalize to a broader swath of the justice-involved population.

entific disciplines. Because there are so many general reviews (Kirk and Wakefield 2018; Pattillo et al. 2004; Travis et al. 2014; Wakefield and Uggen 2010; Western 2006) and specific reviews (Comfort 2007; Foster and Hagan 2015; Hagan and Dinovitzer 1999; Kruttschnitt 2010; Martin et al. 2018; Mears et al. 2015; Turney and Conner 2019; Wakefield and Wildeman 2013; Wildeman 2020; Wildeman et al. 2019; Wildeman, Goldman et al. 2018; Wildeman and Wang 2017) on the fallout from criminal justice contact, in introducing our article, we first provide a discussion on what is new in our review—aside from a small number of articles that have come out since the last comprehensive reviews.

We start by describing the contours of the carceral state. As seminal works in this area have done before (Bonczar 2003; Pettit and Western 2004; Western and Beckett 1999), we are attentive to daily and yearly rates of contact with different criminal justice institutions and to the cumulative prevalence of contact with different criminal justice institutions over the life course. The figures we present in this section repeat many themes that will be familiar to anyone working in this area: rates of incarceration dramatically increased in the mid-1970s and have remained high; rates are especially high for Black individuals, particularly when considered over the life course; and rates of earlier stages of criminal justice contact—especially engaging with the police—are far higher than rates of later stages of criminal justice contact (especially being imprisoned). Yet at the same time, these basic descriptive statistics highlight some information that is new or often overlooked by the field: Native Americans also have very high rates of criminal justice contact, especially considered cumulatively; rates of incarceration for Black people, while still high, have decreased substantially, especially in roughly the last decade; and class inequality within racial and ethnic groups in incarceration, especially for Whites, is rising to a rate that firmly recenters Whites with low levels of education as central to this literature.

The contours of the carceral state lead us to our next section, which considers race-specific effects of criminal justice system contact,[4] in our attempt to better understand the consequences of the carceral state for inequality. We start this section by briefly reviewing the limited research on race-specific associations and effects. Our core findings from reviewing this literature are as follows: virtually no research includes separate tests for Native American or Asian populations; there seems to be a growing accumulation of evidence for larger negative effects for White children, especially with regard to parental incarceration; there is generally significant heterogeneity when papers do conduct race-specific tests, which implies a need for this to become a central condition of future research in this area; and some papers that do conduct race-specific tests do not actually test whether across-race differences are significant, or lack statistical power to do so rigorously, leaving questions unanswered regarding the conclusiveness and generalizability of potential cross-group differences. In this section, we also briefly note that existing research has rarely sought to provide evidence of gender-specific or class-specific effects, both of which, according to our review of the demography of criminal justice contact, merit more significant attention. As such, this section provides some evidence that the implications of the carceral state for inequality may in fact be far more uncertain than most prior reviews have concluded. After documenting the relative paucity of research estimating race-specific associations and effects in this area, we provide an overview of theoretical accounts for why race-specific effects may—or may not—exist and how they may be greater for White or Black individuals.

As a brief illustration of the importance of testing for race-specific associations and considering theoretical perspectives that might explain racial differences, consider the case of outcomes related to employment. Western (2002) finds that the wage growth penalty of incarceration is seemingly larger for White people compared to Black people; findings along these lines could be explained by what we refer to as floor effects, in that Black people start at

4. Following Lee (2024, 234) we use the term "criminal justice system" to be consistent throughout, though "criminal legal system" is becoming increasingly popular among advocates and researchers.

a lower point because of structural inequalities in other systems, and thus, setbacks following criminal justice system contact may be hard to detect owing to high baseline levels of disadvantage. Similarly, given that Black people are more likely than White people to face indirect exposure to the criminal justice system at, for instance, the familial or neighborhood levels (Muller and Roehrkasse 2022), the effects of individual-level exposure could be muted. We refer to this idea as multiple levels of treatment (see also Haskins and Lee 2016). On the other hand, in the case of unemployment, David Harding and colleagues (2018) find that while White people with presentence work histories experience a longer-term negative effect of imprisonment on employment, Black people with work histories do not. We see this as being related to racial bias in the form of cumulative disadvantage accrued over the life course (see also Nahra et al. 2025, this issue).

Having established the relative paucity (and clear necessity) of evidence regarding race-specific associations and effects of criminal justice contact, we move to the two sections that form the core of our review article. The first of these considers data and methods, focusing on the datasets most often used to consider the direct and indirect effects of criminal justice contact and the methods most often used to test them. Our review focuses only on quantitative analyses estimating the effects of criminal justice contact on individuals, a decision we discuss more throughout this article. On the data side, we provide an extensive discussion of the main longitudinal datasets used to consider these relationships, the measures that exist in these datasets, and some of the issues associated with trying to tease out these effects in these data; we do the same for administrative data in this area. On the methods side, we focus our attention on the various methods that have been used in this area, and their frequency by subfield, for all methods that are more rigorous than basic covariate adjustment. In this section, we also discuss the relative tradeoffs of the methods used in this area including, most notably, the rotational assignment of judges to cases, which, regrettably, now forms what many would consider the core of causal research in this area. As we highlight, the limitations of this methodology are not often well understood or clearly articulated, and we aim to address this directly to provide more context for those of us who consume and produce work using such methods.

The fourth section of this article contains the actual review of the existing literature in this area, with emphasis on more recent works whenever possible. There are several takeaways from this section. First and foremost is that studies contain far more variation in the details regarding the margins of criminal justice system contact and geographic context than is often acknowledged in academic and policy discussions—and that this should become more central to how we interpret and generalize results. Relatedly, the data and methodological strategies used are highly consequential, with the most rigorous studies—which often use administrative data and focus on narrow contexts—providing more mixed evidence regarding the association between criminal justice contact and subsequent outcomes. And there is also burgeoning evidence of substantial heterogeneity in effects by race, as well as a range of studies showing null or even protective effects for some groups in certain domains—such that researchers working in this area must develop theories around why it is that criminal justice contact does occasionally have either positive or null effects, and for whom.

Given the centrality of race and class in this article, we offer definitions from the outset. Following Rashawn Ray and Nicole DeLoatch (2016), we see race as a "classification system that is socially constructed." Race, and how race is defined, is a product of complex contemporary and historical social processes and is shaped by multiple social factors, including by institutions like the criminal justice system. In the context of this review, race can be seen as a construct assigned by criminal legal entities and actors (Muhammad 2011; Haney López 2006). As we discuss later, the operationalization of race as a social category in related research that draws largely from administrative and survey data sources is as an imprecise—and varying, depending on how data are collected—proxy that reduces complex identities to simplistic categories that may not reflect in-

dividuals' self-identification or ethnic heritage. Furthermore, we lack sufficient data to adequately consider ethnicity, immigrant generation, country of origin, and several other core dimensions that are also likely to be important in the criminal justice system. We also note that while Hispanic identity is commonly defined as an ethnicity that encompasses diverse racial categories, it can also be conceptualized as a racial construct, particularly in the context of recent (and hotly debated) changes to the census response categories and given how administrative data are often collected in the modern era by criminal justice entities (Asad and Clair 2018; Eppler-Epstein et al. 2016; Finlay et al. 2024; Nuñez et al. 2024). Indeed, a growing body of research has shown how Hispanic populations are racialized through processes of social exclusion, withholding of political and social rights, and statistical discrimination (Asad and Clair 2018; Brown et al. 2018). Therefore, we include estimates for Hispanics (and Native Americans), when available from empirical studies, to be as complete as possible, though we focus our discussion throughout on Black and White people.

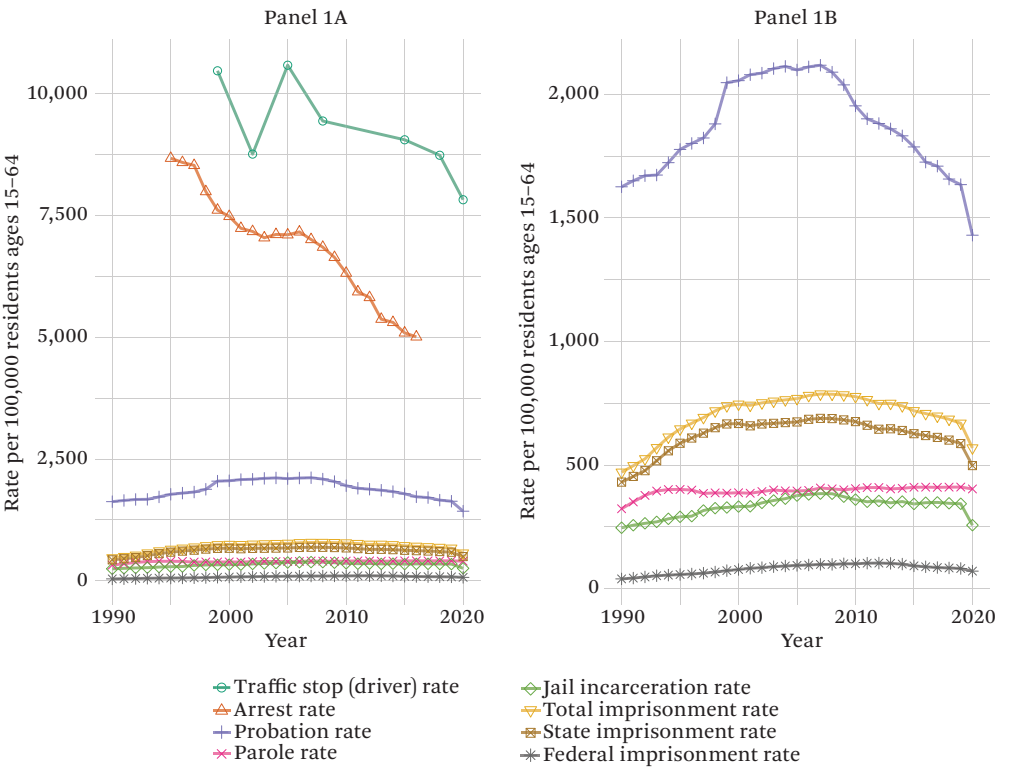
Similarly, we define class primarily using educational attainment as a proxy owing to the dearth of other measures available in criminal justice system administrative data sources that has made this the long-standing conventional measure in research on punishment in the United States (Pettit and Western 2004; Muller and Roehrkasse 2022). As Christopher Muller and Alexander Roehrkasse (2022, 810) note, studying class using educational attainment has become increasingly relevant as the college-no college divide has emerged as a critical factor in life course outcomes in the United States. However, we also acknowledge that other measures of class—including wealth, income, neighborhood poverty rate, and receipt of government assistance—provide a more global assessment of class position yet are unavailable in existing relevant (administrative) data. This means that class, as used in this review, can also be seen as class as assigned by criminal legal entities. This definition, as with race, reflects the practical constraints of available data rather than our theoretical preferences, and this limitation shapes how we, and others, understand and analyze patterns of criminal justice contact.

In closing, we advance four core arguments. First, as we have stated in at least two other reviews of this literature (Lee and Wildeman 2021; Wildeman 2020), research in this area cannot advance without broad data collection efforts focused on criminal justice contact, criminal activity, and the various personal and familial difficulties and other factors that produce both criminal activity and criminal justice contact. Second, analyses using administrative data should seek to provide tests of the fallout from criminal justice contact that are as close as possible to providing causal estimates of effects and as broadly representative as possible. Third, such tests are also highly attentive to the precise nature and context of system contact as well as to potential heterogeneous effects by race. As a result, we argue not for an ever-increasing number of studies testing these associations using existing data but, instead, for a new wave of research using data that are uniquely positioned to consider these relationships because they were designed to do precisely that. This will require significant funding for studies that rely on both administrative data and the collection of new survey data. Finally, we warn that the research community's ever-increasing focus on producing narrow causal estimates can undermine the immense power of descriptive research in this area. Mass incarceration irreversibly transformed entire communities—and, indeed, even the nation as a whole—in fundamental ways that cannot be quantified using traditional causal methods. We hope to use this article to argue that privileging causality over rich description leaves us less informed about the gravity of society's collective fallout in the wake of mass incarceration.

A DESCRIPTIVE PORTRAIT OF THE CARCERAL STATE

Although the focus of our review is on the fallout from criminal justice contact, we see a description of the carceral state as a core precursor to understanding such effects. As a result, we include a brief overview of the daily, yearly, and lifetime (or cumulative) prevalence of multiple stages of criminal justice contact, on the basis of some new analyses and the best avail-

Figure 1. Rates of Criminal Justice System Contact by Type



Source: Authors' calculations.

Note: The coverage of probation agencies expanded in 1998 and 1999. Therefore, the probation rate may not be comparable to prior years.

able data. Owing to limited data, we do not directly engage with the role of prosecutors in shaping the prevalence of contact with later stages of the criminal justice system (see Bushway et al. 2025, this issue, for a discussion of prosecutorial decisions).

Long-Term Trends in Criminal Justice System Contact

Figure 1 provides the first stage of that description by showing what the arrest, probation, parole, jail incarceration, state imprisonment, federal imprisonment, and total imprisonment rates are over roughly the last thirty years.[5] The figure is broken into two panels. Panel 1A includes arrests and traffic stops, while panel 1B excludes them. We have created separate panels because arrests and traffic stops are much more common than all other forms of criminal justice contact. As panel 1A shows, traffic rates and arrests are the most common form of criminal justice system contact, although the arrest rate has declined markedly since the mid-1990s. Probation is the next most common form of criminal justice system contact, though the probation rate has declined since the early twenty-first century. Rates of parole, which typically occurs following prison release, have increased slightly since 1990. Jail incarceration rates also climbed in the late twentieth century

5. For figure 1, we draw on various data sources, including "Arrest Data—Reported Number of Arrests by Crime" from the FBI (2019); "Bridged- Race Population Estimates" from the US Census Bureau and the National Center for Health Statistics (n.d.-b); and "Contacts Between Police and the Public Report Series" and "Estimated Number of Persons Under Correctional Supervision in the United States, 1980–2020," both from the United States Department of Justice, Bureau of Justice Statistics (USDOJ, n.d.-c, 2022c).

and early aughts, followed by a slight decline beginning around 2008 (note the COVID pandemic emergency years, which brought about a steeper decline). People in local jails are typically either being held pretrial or serving sentences that are generally under one year, though some jails also rent out beds to state correctional authorities and federal agencies. People in prisons have generally been convicted and are serving longer sentences. State imprisonment rates have always been higher than the federal imprisonment rate (Cantwell 1980). State and total imprisonment rates peaked in 2007, while the federal imprisonment rate peaked in 2012, and all three rates have since trended down. Note that these rates reflect point-in-time counts of contact with the criminal justice system, which is distinct from measures of admission to the criminal justice system.

Daily and Annual Estimates

In 2018, over sixty-one million people ages sixteen and older had contact with the police (Harrell and Davis 2020).[6] In the same year, there were over ten million arrests for all offenses (FBI 2019). Arrest rates in the United States have long been the highest for Black Americans,[7] followed by Native Americans (we refer to this group as AI/AN in figures to be consistent with data sources). Since the mid-1990s, arrest rates for all groups have trended downward, coinciding with the Great Crime Decline (Sharkey 2018); this decrease has been largest among Black Americans and Native Americans, which are the groups that had the highest starting rates (figure 2, panel 2A).[8] Note that unless otherwise specified in figure 2,[9] racial and ethnic categories are mutually exclusive, in that all racial groups are non-Hispanic, and Hispanic is its own category. We use the term Hispanic throughout this article because that has historically been the term used by most, if not all, criminal justice agencies.

In 2019, the last year before the COVID-19 pandemic, 10.3 million people were admitted to jails, and 734,500 people were held in jails during a point-in-time count (Zeng and Minton 2021). The average length of stay in 2019 was 26.2 days and was longer in larger jail jurisdic-

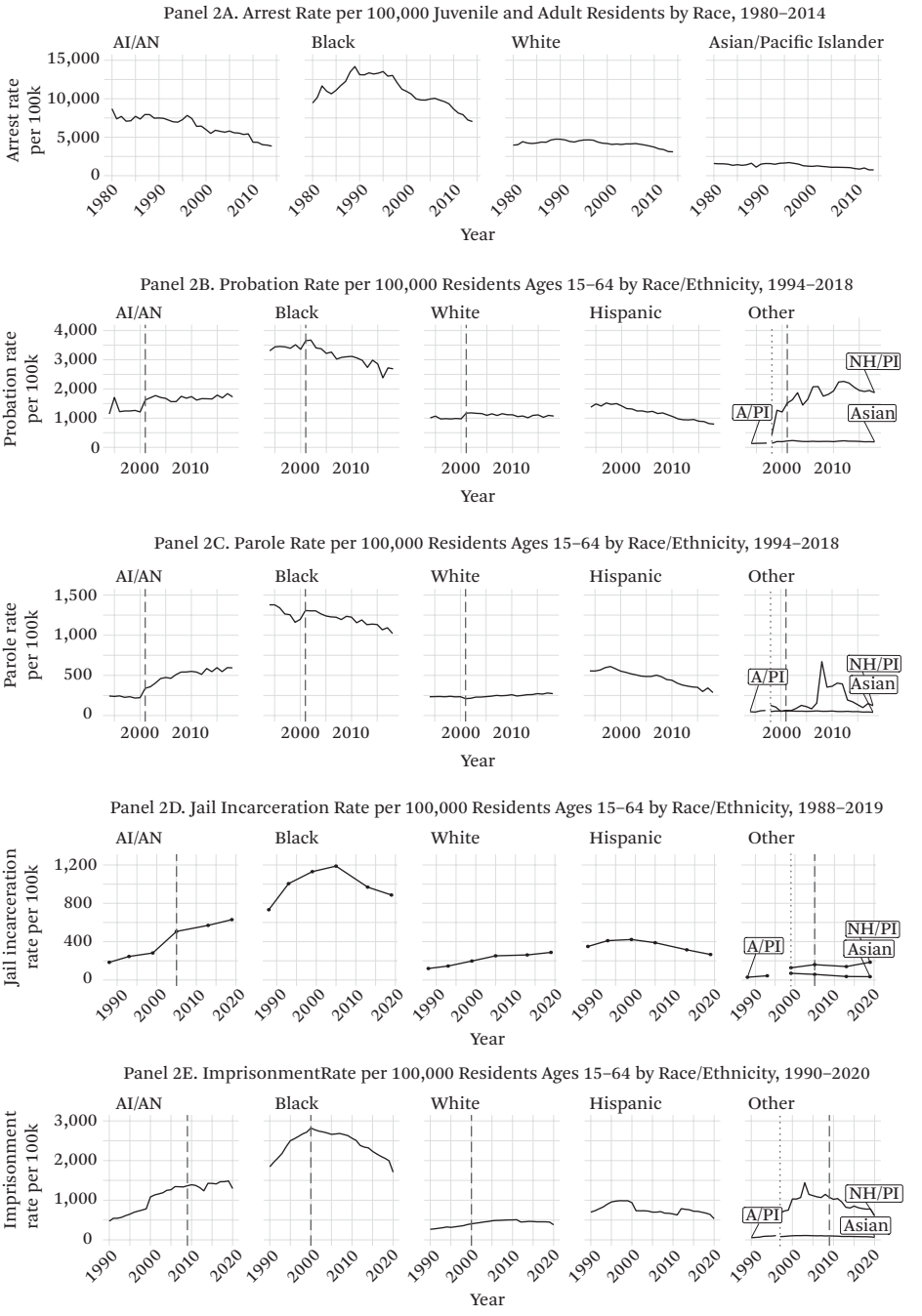
6. This report is based on data from the US Justice Department's Bureau of Justice Statistics' 2018 Police-Public Contact Survey (PPCS), which is a supplement to the National Crime Victimization Survey (NCVS). In the 2018 PPCS, US residents were asked about instances in which they sought help from police (resident-initiated contacts) and when police approached or stopped them (police-initiated contacts). Resident-initiated contacts with police included reporting a crime, disturbance, or suspicious activity; reporting a noncrime emergency; nonemergency inquiries, such as asking for directions; participating in a block watch or other anticrime program; or approaching or seeking help from police for another reason. Police-initiated contacts included being stopped by police while in a public place or a parked vehicle, being stopped by police while driving a motor vehicle, being arrested, or being stopped or approached by police for some other reason. The PPCS also collected data on contacts from traffic accidents. The study found that 61.5 million residents had at least one contact with police. Twenty-four percent of residents experienced contact with police, up from 21 percent in 2015. The findings include percentages by race or ethnicity, sex, and age on the types of police contacts and whether they experienced threats or use of force.

7. Given persistent levels of Black-White racial residential segregation (Logan and Stults 2021) and racially disparate arrest rates, it should not be a surprise that police stop patterns also differ by neighborhood racial and socioeconomic composition—even after one controls for other factors like neighborhood crime (Fagan et al. 2009).

8. See Stevenson and Mayson (2018) for rates of likely misdemeanor arrests over time by race (Black versus White).

9. Data sources include the "Annual Parole Surveys" for 1994–2018 (USDOJ, n.d.-a), "Annual Probation Series" for 1994–2018 (USDOJ, n.d.-b), Synder et al. (n.d.) , "Censuses of Jails/Jail Inmates" from 1988, 1993, 1999, 2005, 2013, 2019 (USDOJ 1996, 2005, 2007, 2009, 2018, 2022a), "National Prisoner Statistics" (USDOJ 2022b), "Bridged-Race Population Estimates" from the US Census Bureau and the National Center for Health Statistics (n.d.-b), "Estimates of the Resident Population by Single Year of Age, Sex, Race, and Hispanic Origin for the United States: April 1, 2000 to July 1, 2010" from the US Census Bureau (n.d.-a), and "Single-Race Population Estimates" from the US Census Bureau and the National Center for Health Statistics (n.d.-c).

Figure 2. Point-in-Time Contact Rates by Contact Type and Race/Ethnicity



Source: Authors' calculations.

Note: Vertical dotted lines represent when two separate categories replaced the combined Asian/Pacific Islander category. Vertical dashed lines indicate that in the years following, counts by racial group do not include Hispanics; in the jails plot, White and Black were already specified as non-Hispanic. Jail population data are adjusted for all years using procedures described in Census of Jails, 2005–2019, Statistical Tables, 48.

tions relative to smaller ones (Zeng and Minton 2021). Point-in-time jail incarceration rates have increased slightly since the 1990s, a trend primarily driven by an increase in jail incarceration rates among Native Americans and White Americans, as seen in figure 2, panel 2D. Jail incarceration rates have increased slightly among Native Hawaiian / Pacific Islanders and are consistently higher among the latter than among Asian Americans. Mirroring other stages of contact, jail incarceration rates remain the highest for Black Americans, though they have declined since around 2005. Jail incarceration rates have also declined for Hispanics over the last two decades.

Point-in-time rates are higher for imprisonment than for jail incarceration, but jails impact a larger number of people annually owing to higher churn; still, well over one million people were held in state or federal prisons as of year-end 2021 (Carson 2022).[10] Figure 2, panel 2E shows that imprisonment rates are the highest for Black Americans, though these have declined over the past two decades; among Native Americans, imprisonment rates have increased since 2000. Imprisonment rates for Asians and Native Hawaiian / Pacific Islanders are often combined, but this conceals stark differences in imprisonment rates among the two groups, with Native Hawaiian / Pacific Islanders consistently facing much higher imprisonment rates than Asians.

Increasingly, class inequality in imprisonment exceeds racial inequality. The ratio of no-college/any-college prison admissions was about ten times higher than the ratio of Black–White prison admissions as of 2015 (Muller and Roehrkasse 2022). Figure 3 shows these trends.[11] While the prison admission rate among Black people without any college education has fallen since 2000, the prison admission rate among White people without any college education has steadily increased—though the rate among Black people with no college education remains higher (Muller and Roehrkasse 2022). The prison admission rate for Black people with a college degree peaked in 1990 and has since declined, while the prison admissions rate for White people with college degrees remained low throughout. However, racial disparities in vicarious exposure to imprisonment, whether measured through familial exposure or neighborhood exposure, continues to eclipse class disparities, likely because of racial disparities in class permeability (Muller and Roehrkasse 2022).

Racial disparity in familial exposure to incarceration mirrors research that finds imprisonment is concentrated in neighborhoods with a larger share of Black, and to a lesser extent Hispanic, residents and with higher levels of concentrated disadvantage, even accounting for crime and other explanatory factors (Sampson and Loeffler 2010; Simes 2021). Although imprisonment is often thought of and studied in urban contexts, Black-White racial disparities in imprisonment are present across the urban-rural spectrum (Eason et al. 2017).

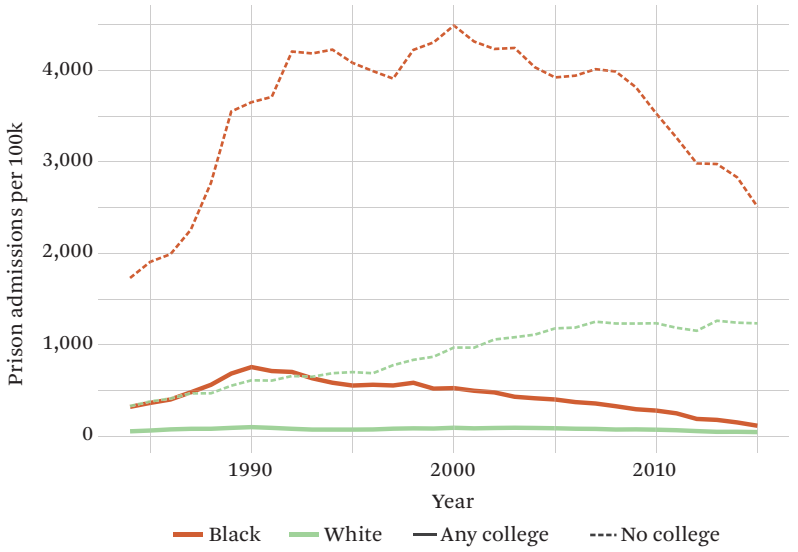
Approximately 3,745,000 Americans were under some form of community supervision as of year-end 2021; 2,963,000 were on probation (the second most common type of contact with the criminal justice system), and 803,200 were on parole. The total count excludes adults on both probation and parole (Kaeble 2023). Over past decades, according to available data,[12] the probation rate has declined among Black and Hispanic Americans, increased among Native Americans and Native Hawaiians / Pacific Islanders, and remained stable among White and Asian populations. Yet, as is clear from figure 2, panel 2B, it remains highest among Black

10. Moreover, in 2016, 684,500 people incarcerated in prisons were parents to almost 1.5 million minor children, and these estimates do not include stepparents or other potential caregivers (Maruschak et al. 2021).

11. Please note that educational attainment is standardized to the distribution of educational attainment in 2015 (Muller and Roehrkasse 2022).

12. Missing data are a problem across all data sources, but especially for probation; data on race and ethnicity are missing for between 23 percent and 39 percent of people on probation depending on the year. See also Sarah Eppler-Epstein and colleagues (2016) on Hispanic ethnicity reporting in administrative criminal justice system data, and Keith Finlay and colleagues (2024) on the misreporting of race and ethnicity in administrative criminal justice system data. Unfortunately, we are unable to correct for such inaccuracies in the data we present in figure 2.

Figure 3. Prison Admissions per 100,000 Residents Ages 20–39 by Race and Educational Attainment



Source: Muller and Roehrkasse 2022. Reprinted with permission.
Note: Rates are adjusted to account for changes in the distribution of educational attainment; see Muller and Roehrkasse 2022.

Americans relative to all other racial groups. Since 1994, the parole rate has also declined the most among Black and Hispanic Americans, while climbing most significantly among Native Americans (figure 2, panel 2C). The parole rate for Native Hawaiian / Pacific Islanders peaked just before 2010, following the peak in imprisonment rates for this group. Black-White racial disparity is smaller in magnitude for probation than for parole, which typically follows imprisonment.

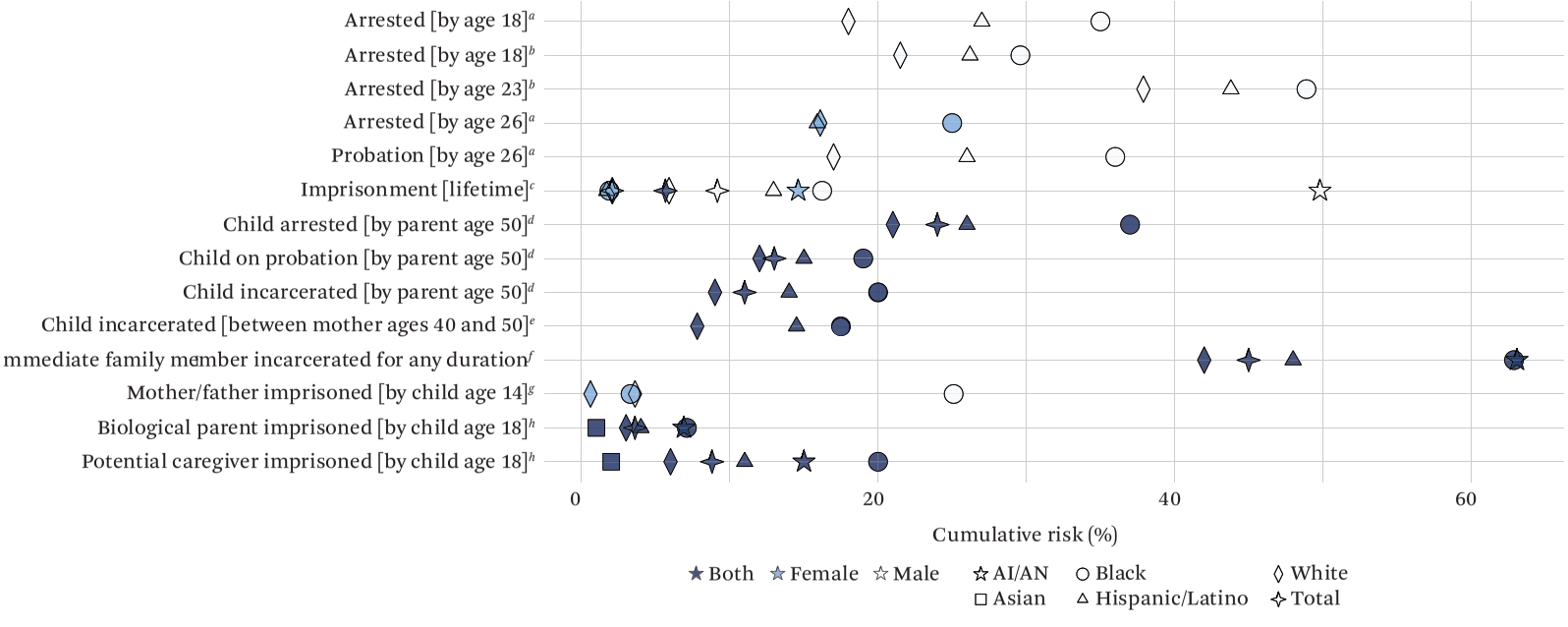
Cumulative Estimates

Figure 4 explores racial and ethnic disparities in contact with each part of the criminal justice system over a lifetime—or, in some cases, a distinct stage of a lifetime such as childhood—rather than on any given day or year. Across all parts of the criminal justice system, Black and Native Americans experience the highest cumulative risk of direct and vicarious exposure. Estimates suggest that over 60 percent of Black and Native Americans have ever had an immediate family member incarcerated (in either a jail or a prison) for any amount of time (Enns et al. 2019). Within racial and ethnic groups, the cumulative risk of direct exposure to the criminal justice system is higher for males than for females (Boen, Graetz, et al. 2022; Roehrkasse and Wildeman 2022).

By age eighteen, 35 percent of Black men will have ever been arrested; by age twenty-six, this risk for Black men increases to almost 60 percent, which is almost double the risk for White men (Boen, Graetz, et al. 2022). The cumulative risk of arrest for women is lower, but still substantial, especially for Black women. Notably, while Hispanic men face a higher cumulative risk of arrest by age twenty-six than do White men, Hispanic and White women face the same risk of arrest by age twenty-six. The risk of experiencing arrest also varies by parental educational background, a proxy for class. Among men whose highest-educated parent has a high school education or less, 60 percent of Black men and 39 percent of White men will be arrested by age twenty-six, compared to 39 percent of Black men and 24 percent of White men who have a college-educated parent. Even if one considers parental background, racial disparity remains stark. Black men with a college-educated parent are just as likely as White men with parents that have at most a high school education to be arrested by age twenty-six (Boen, Graetz, et al. 2022).

Vicarious exposure also varies by race, eth-

Figure 4. Direct and Indirect Cumulative Exposure Risks to Arrest, Probation, and Incarceration



Sources: [a] Boen, Graetz, et al. 2022; [b] Brame et al. 2014; [c] Roehrkasse and Wildeman 2022; [d] Boen, Olson, and Lee 2022; [e] Goldman 2019; [f] Enns et al. 2019; [g] Wildeman 2009; [h] Finlay et al. 2023.
Note: Overlapping estimates are jittered to ensure visibility.

nicity, and education. By age fifty, almost half of Black parents with sons will experience the arrest of a son, compared to 23 percent of White and 31 percent of Hispanic parents (Boen, Olson, and Lee 2022). The cumulative risk of familial arrest is also larger among individuals with less education (Boen, Olson, and Lee 2022). However, among parents with sons, by age fifty White parents with at most a high school education experience comparable risks of child arrest as do Black parents with at least a college education (Boen, Olson, and Lee 2022).

Because of data limitations, there is limited research on the cumulative risk of jail incarceration. Western and colleagues (2021) use data from New York City, where the jail incarceration rate is relatively low, and find that a sixth of Hispanic men and upward of a quarter of Black men have experienced jail incarceration by age thirty-eight. The Black-White racial disparity is extreme. Black men's relative risk is between eight and twenty times higher than that for White men, with the disparity increasing with the number of jail incarcerations. In fact, despite gender differences in the relative risk of criminal justice system contact, Black women experience a higher risk of jail incarceration than do White men (Western et al. 2021). These inequities in jail incarceration risk are spatially concentrated in neighborhoods with high levels of poverty. For Black and Hispanic New Yorkers, the cumulative risks of being jailed were higher by about 50 percent if they resided in a high-poverty neighborhood (Western et al. 2021). However, the geography of jail incarceration is moving away from large urban centers like New York City. According to an analysis of a large sample of US counties, the average jail incarceration rate among nonlarge urban counties surpassed that of large urban counties in 2003 (Simes 2021).

Vicarious exposure to jail incarceration has not yet been estimated, but some studies estimate vicarious exposure to incarceration without distinguishing between jail incarceration and imprisonment. Boen, Olson, and Lee (2022) used the *Transition into Adulthood Supplement* from the Panel Study of Income Dynamics (PSID) and estimated that Black parents are more than twice as likely to experience the incarceration of a child by the time they are fifty than are White parents. The cumulative risk of vicarious exposure to incarceration also differs by education level, but the magnitude of these differences is larger for White parents than for Black parents (Boen, Olson, and Lee 2022). Peter Enns and colleagues (2019) estimate that 45 percent of Americans have ever experienced the incarceration of an immediate family member for any duration, capturing both jail and prison stints. The cumulative prevalence of immediate family member incarceration is highest among Black and Native American people at about 63 percent (Enns et al. 2019). Black Americans are also more likely to have had more family members incarcerated and family members from more generations incarcerated (Yi 2023).

Like jail incarceration, cumulative exposure to imprisonment varies by race, ethnicity, and location. Recent research shows that Native American men face an extremely high lifetime risk of imprisonment at almost 50 percent, compared to a total risk of under 6 percent among the general population (Roehrkasse and Wildeman 2022). Black men continue to have a high lifetime risk of imprisonment relative to most other racial groups, though their lifetime risk of imprisonment has declined markedly among more recent birth cohorts (Robey et al. 2023). Racial differences in the cumulative risk of imprisonment also differ geographically; Black men are 60 percent more likely to experience imprisonment in the Midwest than in the West, while Hispanic men face the highest lifetime risk of imprisonment in the West (Muller and Wildeman 2016).

Black and Native American children are more likely to be exposed to parental imprisonment (Finlay et al. 2023; Wildeman 2009). Exposure risks are even higher when all potential caregivers are considered, with about 20 percent of Black children experiencing the imprisonment of a potential caregiver during their childhood, or well over twice the exposure risk faced by White (6 percent) and Asian (2 percent) children (Finlay et al. 2023). The cumulative risk of parental imprisonment is the highest in the Midwest, Northeast, Florida, and Texas for Black children, and in the Northeast and West for Hispanic children (Muller and Wildeman 2016).

Educational levels shape vicarious exposure

to imprisonment, though this relationship varies by racial group. For Whites, the cumulative risk of having a family member incarcerated for more than one year among college-educated people is one-fifth the risk for Whites with less than a high school degree, while this risk ratio is only 1:2 for college-educated Black Americans relative to Black people with less than a high school degree (Enns et al. 2019). Black Americans with a college degree face the same risk of having a family member incarcerated for more than one year (23 percent) as do Whites with less than a high school degree (Enns et al. 2019).

There is limited research on the cumulative risk of community supervision and of vicarious exposure to community supervision. An estimated 36 percent of Black men have experienced probation by age twenty-six, compared to 17 percent of White men and 26 percent of Hispanic men (Boen, Graetz, et al. 2022). Courtney Boen and colleagues (Boen, Olson, and Lee 2022) estimate that by age fifty, about 13 percent of all parents will have experienced having a child on probation. This risk is 26 percent for Black parents with sons, double the risk for White parents with sons. While still significant, the racial disparity in the cumulative risk of familial exposure to probation is smaller than it is for more serious types of contact. Community supervision is, unsurprisingly, spatially concentrated; of individuals paroled from Michigan prisons in 2003, half moved to census tracts representing 12 percent of total tracts in Michigan, all of which were located in just three counties (Morenoff and Harding 2011).

EXISTING PERSPECTIVES ON RACIAL AND ETHNIC HETEROGENEITY

In this section, we provide an overview of existing research on racial and ethnic differences in the associations between criminal justice contact and individual and familial outcomes. We then provide an extended discussion on the different ways in which there might be racially disparate associations between criminal justice contact and individual and familial outcomes.

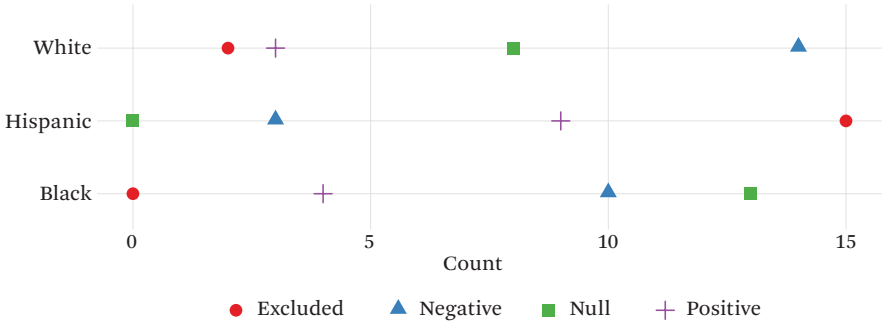
We note that, as the research highlighted in the previous section shows, it is also important to consider class- and gender-specific associations between criminal justice contact and individual and familial outcomes. Because very little research that we are aware of considers such heterogeneous effects, however, there is simply no literature for us to review in this space. And because there is little existing theoretical discussion motivating such class-specific analyses, there is also little in the way of theoretical perspectives for us to review here. We do, however, point to several recent quantitative studies that emphasize the importance of heterogeneity in effects by gender (Legewie and Fagan 2019; Novak and Gilbreath 2023) and by both cohort and historical period (Neil and Sampson 2021; Neil et al. 2021) in understanding the fallout from criminal justice contact.

Existing Research on Race-Specific Associations

Relatively little research has examined racial heterogeneity in the impacts of contact with the criminal justice system; figure 5 provides counts of the directionality of findings, which are categorized and described in more detail in table 1. What little we know about racial heterogeneity in these impacts thus presents, at best, a murky and incomplete picture. The available empirical evidence, which almost exclusively examines differences between Black and White populations, is limited in a number of ways, which we will discuss in some detail. These issues significantly impede our ability to understand the implications of racial disproportionality in contact for other forms of racial and ethnic inequality that result from this contact.

Research considering the direct impacts of criminal justice contact seems to reliably uncover heterogeneity by race, though some parental incarceration research suggests more consistent average effects (Hagan and Foster 2012; Norris et al. 2021), which would still lead to racial inequalities in youth outcomes given vastly disproportionate contact. But, taken together, the literature is fairly mixed in terms of race-specific consequences. Some work suggests stronger negative impacts for Black populations compared to White populations, which would lead to an amplification of inequalities related to disproportionate contact and impact (Ang 2021; Apel and Powell 2019; Legewie and Fagan 2019; White 2019b). However, other work suggests weaker effects for Black populations

Figure 5. Results from Race-Stratified Models in Existing Literature



Source: Authors' calculations.

compared to White populations, which would lead to a reduction (but not necessarily an elimination) of inequalities (Harding et al. 2018; Haskins 2016; Massoglia et al. 2013; Western 2002; Wildeman et al. 2024). In one case—mortality rates during incarceration—Black populations (but not White populations) may experience a protective or beneficial effect, potentially leading to a reversal in inequalities (Patterson 2010; Spaulding et al. 2011).

Drivers of Race-Specific Associations

There are many reasons to expect variation by race in the impacts of contact with the criminal justice system. However, when considering such differences, we must first decouple race-specific associations from race-specific effects. Most research on racial variation in the impacts of criminal justice contact provides evidence of race-specific associations, not effects. In many cases, this is by design or due to the inability to measure key confounders or account for selection. However, even when we use causal modeling approaches, significant issues complicate how we can interpret racial variation in outcomes. Race, in and of itself, is not a causal factor. For example, there is nothing inherent about self-identifying as Black that would make one more likely to have contact with the criminal justice system or more or less vulnerable to that contact. Instead, the experiences and conditions of being a Black person in a highly racialized society that has historically devalued Blackness create risk and vulnerability. Indeed, structural factors that are a product of historical and contemporary processes of devaluation (such as racial residential segregation, which determines access to public goods like high quality schools, employment opportunities, and access to health care) can increase the risk of criminal justice contact and the capacities to mitigate the consequences of this contact (Ray 2019; Lee 2024). These underlying factors are harder to disentangle in even the most sophisticated causal modeling approaches. Despite these challenges, we can use theory as a guide to better understand and evaluate prior research and inform future research in this area. Below we summarize key theoretical considerations and conceptual frameworks from the social sciences.

Unobserved Heterogeneity

Racial disproportionately in contact with the criminal justice system means that while a diverse portion of the Black, Hispanic, and Native American population is in contact with the criminal justice system, only a highly select portion of the White population is in contact with this system. The unobserved characteristics of these White populations introduce bias into models that attempt to isolate the impact of criminal justice contact from selection into that contact. While selection is a methodological concern for any racial group, interpretations of racial variation must consider that unobserved heterogeneity may introduce more bias into models for White populations than for Black, Hispanic, or Native American populations. For example, larger negative social, economic, and health consequences of criminal justice contact for White populations com-

Table 1. Results from Race-Stratified Models in Existing Literature

				Coefficient significantly different from 0? Positive (+) or Negative (–) or Null (x) *Rounded sample size in thousands*				
Citation	Dataset	Type of contact	Outcome	AI	AS	B	H	W
Individual (that is, one's own) contact with the criminal legal system								
Ang 2021	Multiple sources	Police	Schooling		x 43	– 86	– 528	x 57
Apel and Powell 2019	NLSY97	Incarceration	Employment			– 2.3	x 1.9	x 4.7
Bovell-Ammon et al. 2021	NLSY79	Incarceration	Mortality			– 3		x[a] 5
Harding et al. 2018	Admin records (MI)	Incarceration	Employment 4Q post release (no work history)			+ 17		+ 14
			Employment 4Q post release (w/ work history)			x 29		– 46
			Employment 12Q post sentence (no work history)			+ 16		+ 14
			Employment 12Q post sentence (w/ work history)			x 28		– 45
Legewie and Fagan 2019	Admin records (NYC)	Police in neighbor-hood	Schooling (boys)			–	x	
			Schooling (girls)			x	x	
Massoglia et al. 2013	NLSY79	Incarceration	Neighborhood residence			x 1.3	x 0.5	– 0.8
Pager 2003	Audit study	Criminal records	Employment			– 0.2		– 0.15
Patterson 2010	Admin data (US)	Incarceration	During-incarceration mortality			+		–
Spaulding et al. 2011	Admin records (GA)	Incarceration	During-incarceration mortality (men)			+ 16		–[b] 8
			During-incarceration mortality (women)			x		x
			Post-release mortality (men)			–		–
			Post-release mortality (women)			–		–

Western 2002	NLSY79	Incarceration (prior only)	Wages	x 0.6	– 0.3	– 1.2
White 2019b	Admin records (TX)	Incarceration	Voter turnout	– 32		x 78
		Familial contact with the criminal legal system				
Bruns and Lee 2020	FFCWS	Incarceration	Drug use	– *1.8*	x *1*	x *0.8*
			Heavy Drinking	x	x	–[c]
			Heavy Smoking	x	x	x
Haskins 2014	FFCWS	Incarceration	Non-cognitive test scores (male)	– 0.6		– 0.3
			Non-cognitive test scores (female)	x 0.5		x 0.3
			Cognitive test scores (male)	x 0.4		x 0.2
			Cognitive test scores (female)	x 0.4		x 0.2
Haskins 2016	FFCWS	Incarceration	Cognitive development	x 1.1	x 0.6	– 0.5
Norris et al. 2021	Admin records (OH)	Incarceration	Criminal justice contact	+[d]		x
Wildeman and Turney 2014	FFCWS	Incarceration	Behavioral problems (caregiver-reported)	x 1.6	–/x[e] 0.85	+/x[e] 0.7
Wildeman et al. 2024	PHDCN+	Criminal justice contact	Age of arrest	x 0.2	x 0.25	– 0.1
			Number of arrests	x	–	–

Source: Authors' compilation.

[a] Non-Hispanic/Non-Black.

[b] In the abstract, the authors refer to "white" men/women, but in the paper/tables they refer to "nonblack."

[c] This coefficient is only shown visually in a coefficient plot and the confidence interval bound visually appears to be right up against 0, so it's unclear if it's technically statistically significant at the $p < .05$ level.

[d] Tables 4 (panel E) and A15 show total *N* but no sample size broken down by race.

[e] Indicates that the large number of outcomes vary in terms of being positive/negative/null (as opposed to listing all outcomes as separate rows).

pared to Black, Hispanic, or Native American populations may simply reflect the characteristics of White populations in contact with the system rather than any differences in the magnitude of harms White populations experience relative to other groups.

Floor Effects

Racial inequalities do not exist only in the criminal justice system. Racial inequalities exist across all systems, including education, the labor market, health care, and housing (Lee 2024). Because of histories and contemporary realities of systematic racial bias, minoritized populations face significant structural and social disadvantages prior to and in conjunction with direct and indirect contact with the criminal justice system. Consequently, we might expect to see the negative impacts of criminal justice contact for Black and Native American populations suppressed in empirical models.

For example, extreme levels of racial residential segregation that keep Black populations in the most disadvantaged neighborhoods both increase the risk of contact with the criminal justice system and limit further downward mobility for those Black populations in contact with the criminal justice system (Massoglia et al. 2013). In other words, there is nowhere farther to fall if you are already at "The Bottom"[13] (Haskins and Lee 2016). The idea that increased adversity due to criminal justice contact in a population with an already-high baseline of adversity can limit the negative consequences of criminal justice contact has been described as floor effects in prior work (Massoglia et al. 2013).[14] This also means that, in some cases, criminal justice contact may be protective for Black populations but not for White populations. For example, research has found incarceration to reduce mortality risk for Black males but not White males (in the short term), suggesting that while prisons, in general, are unhealthy and unsafe environments with flawed health care delivery (Puglisi and Wang 2021; Wildeman, Fitzpatrick, et al. 2018), they provide safer places and better access to medical care for Black males compared to the environments in which they resided prior to imprisonment (Patterson 2010). Indeed, the fact that a recent study shows that young Black and Hispanic males in parts of major US cities faced greater firearm-related risks of death and injury than did soldiers at war in Afghanistan and Iraq calls for an urgent response (Del Pozo et al. 2022).

Multiple Levels of Treatment

Related to floor effects, racial disproportionality in criminal justice contact also suggests that Black populations are exposed to the criminal justice system at multiple levels of social contact (that is, individual, family, neighborhood, school). For example, owing to racial residential segregation and the concentration of policing and incarceration in predominantly Black neighborhoods and schools, criminal justice contact is both a community-level and an individual-level treatment or exposure. For example, the effects of incarceration at the community level could suppress individual-level estimates of the effect of incarceration for Black populations—but potentially not White populations—when one employs quasi-experimental methods that compare same-race "control" counterparts, since the control group would be indirectly exposed to the treatment in ways that are often difficult or impossible to measure in existing data (Haskins and Lee 2016).

Racial Bias

Racial disproportionality in contact with the criminal justice system does not occur by osmosis and is at least partly due to a long history of racial bias across all points of contact with the criminal justice system (Hinton and Cook 2021; Thompson 2020). For example, a growing body of research shows that police stops and use of force during stops are heavily racialized (Kramer and Remster 2018). In other words, police are more likely to stop and arrest, and use

13. "The Bottom" is a term used to describe segregated Black communities, especially in urban areas (Davis 2018).

14. Although Massoglia and colleagues (Massoglia et al. 2013) do not use the specific phrase floor effects in their article, this article nonetheless provides an excellent example of floor effects in this specific research area.

force against, Black people than White people because police deem Black people more suspicious owing to their being Black (see Dovidio and Solomon 2025, this issue).

Police also use force more often against Black people than White people because police deem Black people to be more violent. Research on unconscious bias provides empirical evidence of systematic biases in perceptions of criminality and violence due to race (Eberhardt et al. 2004). These biases impact risk of contact and risk of violence during contact. A recent study demonstrates that the racial disparity in police stops between Black and White drivers is reduced at night when it is more difficult to discern the race of the driver (Pierson et al. 2020). Similarly, Felipe Goncalves and Steven Mello (2021), using data from the Florida Highway Patrol, find that compared to White drivers, minorities are less likely to get a "discount" on their speeding tickets. Mark Hoekstra and CarlyWill Sloan (2020) found that while Black and White officers exhibit similar patterns of gun force when assigned to majority White neighborhoods, they differ significantly when dispatched to neighborhoods with more than 80 percent Black residents. In those neighborhoods, White officers are roughly five times more likely to use gun force compared to Black officers. This form of selection into contact due to police discrimination combined with racial biases in perceptions of aggression and violence may increase the risk of fatal and nonfatal injury due to police contact for the Black population.[15]

These findings are consequential because contact with police is most often the first stage of criminal justice system processing and, thus, shapes who experiences later stages of contact. Yet, as a large body of research indicates, racial bias operates well beyond the criminal justice system in ways that can serve to amplify the negative consequences of criminal justice contact for Black and Native Americans. For example, racial discrimination in hiring practices can be compounded by employment discrimination if the applicant has a criminal record, further limiting employment opportunities for Black people (Pager 2003; Holzer et al. 2006). In other words, Black Americans and other devalued racial groups are more likely to face cumulative disadvantage both over their life courses and during processing through the criminal justice system (Kurlychek and Johnson 2019; Lee 2024).

Stigma

While racial disproportionately in contact with the criminal justice system can be considered a consequence of racial bias, it is also a source of racial bias and stigma because it racializes criminal or legal categories or statuses such as ex-felon (Asad and Clair 2018). In other words, there are "spillover effects of discredited legal classification among members of racial/ethnic groups who do not hold the legal status" (Asad and Clair 2018, 20) that stigmatize these racial groups in ways that enable statistical discrimination. For example, policies aimed at eliminating hiring discrimination based on a person's criminal record—such as Ban-the-Box, which restricts employer's ability to include questions about prior criminal history on job applications—have been shown to increase racial disparities in employment because, in lieu of this information on job applications, employers use Black and Hispanic race as an analog of criminal status (Doleac and Hansen 2016). In this way, race, ethnicity, and legal status become challenging to isolate in a model and can potentially lead to null effects. These issues are made all the more complicated when one considers that there is evidence suggesting both that the stigma attached to incarceration may be greatest for Black people (Braman 2004) and that the stigma attached to incarceration may be greatest for Whites (Brew et al. 2022).

Conclusion

There is limited evidence regarding race-specific associations and effects of criminal jus-

15. At least in part because of racial bias, Black and Native American men and women are far more likely than White men and women to be killed by police; Hispanic men are also more likely than White men to be killed by police (Edwards et al. 2019). Black men face the highest lifetime risk of being killed by the police (Edwards et al. 2019).

tice contact. Likewise, there are myriad and complex theoretical perspectives suggesting, in many instances, competing perspectives in terms of where race-specific effects and associations are likely to be most profound. As we showed in the introduction, even in a single domain like employment, different theoretical perspectives may be relevant for explaining different outcomes. We engage with these issues again in the article's conclusion.

DATA AND METHODS USED

Researchers face many challenges in identifying the consequences of contact with the criminal justice system. In this section, we provide a wide-ranging and detailed overview of the various data sources and empirical methodologies that quantitative scholars use in this area.

We begin by differentiating between survey data and administrative record data and discuss the pros and cons of each. We discuss the data sources in this research area prominently used over the last few decades and display them in table 2. We also discuss several published journal articles that use administrative records and list them in table 3. We then describe the most promising empirical methods used to analyze the fallout from criminal justice contact. While our goal in this section is not necessarily to create a how-to guide or gold standard for the use of these data or methods, we hope to provide enough breadth and depth to offer a starting point for researchers, students, and policymakers who seek to learn more about the empirics of the field.

Data Sources Used

Quantitative social science research in this area has relied on two main sources of data: surveys and administrative records. In the following section, we describe each in turn, along with newer data-related efforts that hold promise for pushing the field forward.

Surveys

Surveys have long been used in the social sciences to collect data directly from individuals about their life experiences, and many fundamental contributions to the crime and punishment literature have come from longitudinal surveys collected over the last few decades. Surveys have many advantages, not least of which is their accessibility. Survey data are often made publicly available or are accessible through some type of application process that ensures human subjects' protection. This process allows academic and nonacademic researchers, students, practitioners, and others to be able to use these data with relatively few barriers. Surveys are also advantageous in their broad geographic range. Some surveys (such as Add Health, the PSID, and the NLSY) are nationally representative, meaning their coverage spans the entire United States, while others (such as the PHDCN and RYDS) are specific to one locale.[16] Others (such as PROSPER or Pathways to Desistance) encompass multiple places. This range is especially advantageous in thinking about contact with the criminal justice system, given its decentralized nature. On the one hand, results can be compared across surveys located in different city, county, and state demographic and policy contexts, while on the other hand, national-level estimates provide evidence that an empirical phenomenon is not restricted to one locale.

Finally, surveys typically provide users with a broad swath of sociodemographic, familial, and socioenvironmental information, often across multiple age cohorts over a long duration. While the exact range, nature, and detail of these different constructs will vary significantly by survey—especially when a given survey project's aim is focused on a particular focus (such as health)—most surveys will capture many different demographic characteristics and dimensions of an individual's life. Surveys also allow researchers to be attentive to the relevance of age or cohort differences across time (see Neil and Sampson 2021). In sum, surveys often offer researchers rich sources of (often population-representative) data to provide statistically generalizable descriptions of individuals and families that experience contact with the criminal justice system and the conse-

16. Throughout the text, we refer to all surveys using common acronyms or shorthand for the purpose of brevity; a list of full names is in table 2.

Table 2. Survey Data Overview

Source	Year(s)	Locale	Sampling/ Eligibility	CLSC question example(s)
Add Health	1994–present	National	School-based stratified sampling; high school and middle school students were eligible.	1. Have you [child] ever been arrested or taken into custody by the police? (W3/ W4) 2. Have you [child] ever been convicted of or pled guilty to a crime in adult court? (W3/ W4) 3. (Has/did) your [parent] ever (spent/spend) time in jail or prison? (W4)
FFCWS	1998–present	National	Three-stage stratified sample starting with cities of pop >200k, to hospitals, to births within hospitals. Random sample separately taken of marital and non-marital births.	1. Has father ever spent any time in jail/prison? 2. Did you [parent/partner] spend any time in adult correctional facilities? 3. [Child] Ever arrested or taken into custody by police?
NLSY79	1979–present	National	Sample made up of three independent probability samples; representative of entire population of youth 14–21 as of December 31, 1978	1. *Respondent resides in correctional facility at time of interview.*
NLSY97	1997–present	National	Sample made up of two independent probability samples; representative of youth born January 1, 1980, through December 31, 1984	1. Have you [child] ever been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)? 2. In total, how many times have you [child] been arrested? 3. Were you [child] sentenced to spend time in a corrections institution, like a jail, prison or a youth institution like juvenile hall or reform school or training school or to perform community service?
NSAL	2001–2003	National	National probability sample at household level of Blacks, non-Hispanic whites, and Afro-Caribbeans, ages 18 and older.	1. Have you ever been arrested? 2. *Family member not in household because in jail or prison*
PSID	1968–present	National	Nationally representative sample of 5,000 US households (with oversample of low-income households); follows descendants of original household since 1968	1. In the last 6 months, about how many times have you [child] been arrested by the police? (CDS only) 2. *This individual has been in an institution while connected to a responding family for at least one year during the study.*

(*continued*)

Table 2. (*continued*)

Source	Year(s)	Locale	Sampling/ Eligibility	CLSC question example(s)
Pathways to Desistance	2000–2010ish	Phoenix and Philadelphia	Sample of 14–18 year olds who had been found guilty of a serious offense in Maricopa County, AZ or Philadelphia, PA.	1. Number of times subject has been arrested in lifetime 2. Number of times locked up in a detention center/jail before their most recent arrest/summons
PHDCN	1994-present	Chicago	Three-stage stratified sampling starting with neighborhoods, to block groups, to households. Children within 6 months of birth, ages 3, 6, 9, 12, 15, and 18 were eligible.	1. Has [any family member] had trouble with the police or been arrested? (W1) 2. Has [any family member] been arrested, convicted of a crime, or put in jail? (W2/W3) 3. How old were you [child] the first time in your whole life that you were in trouble with the police? 4. Were you [child] held in jail for some time? 5. Of the contacts you [child] have had with the police over the past 12 months, on how many occasions were you arrested or charged with an offense? 6. W4/W5 question
PROSPER	2001-present	Iowa and Pennsylvania	Sample of 28 rural school districts in IA and PA; middle and high school students eligible	1. During the past 12 months, about how many times did you [child] get picked up or arrested by the police? 2. During the past 12 months, have you ever been arrested and charged with a misdemeanor offense/felony? 3. During the past 12 months, about how many times did you spend time in jail?
RYDS		Rochester	Sample of 7th and 8th grade students from Rochester Public Schools selected to overrepresent youth at high risk for serious delinquency and drug use.	1. Were you [child] picked up for questioning for [delinquency act]? 2. Were you [child] actually arrested for [delinquency act]? 3. "Police knew" as an option for consequences of various forms of delinquency

Source: Authors' compilation.

Note: Text in italics represents instances where incarceration is inferred based on household, interview, or similar status. PHDCN = Project on Human Development in Chicago Neighborhoods; Add Health = National Longitudinal Study of Adolescent to Adult Health; FFCWS = Future of Families and Child Wellbeing Study; NLSY = National Longitudinal Survey of Youth; PSID = Panel Study of Income Dynamics; PROSPER = PROSPER Peers Project; RYDS = Rochester Youth Development Study; Pathways = Pathways to Desistance Study; NSAL = National Survey of American Life.

quences of this context over time and across the life course.

However, surveys also have drawbacks. First and most prominently for our discussion, most surveys are not designed with the criminal justice system as a focus.[17] Thus, many surveys fail to offer detail about the nature of one's criminal justice system contact. For example, some surveys do not differentiate between jail and prison incarceration or do not differentiate between police contact and arrest. Some surveys, such as the PSID and NLSY79, do not have any recurring system contact questions and instead rely on a flag or designation of an individual respondent (who has been part of the sample) being housed in a facility at the time of data collection.[18] This leads to a second related issue: surveys, by definition, rely on the respondent to report accurately and knowledgably about their experiences. While this limitation can be a hurdle for any topic, it is particularly salient for criminal justice system contact. Recall bias, or the concern that individuals may make systematic errors in their remembrance and reporting on details from the past, is often mentioned as a problem in surveys. Recall bias can be compounded for individuals who have multiple contacts with the criminal justice system (jail churning, for instance) and because of social desirability bias. Some respondents may underreport behaviors or experiences considered to be undesirable. Beyond recall bias is the fact that individuals may genuinely not know what form of contact they (or their family member) had with the criminal justice system. An individual may not be sure whether they were arrested (or whether their family member was arrested) during a police encounter, or they may misunderstand the difference between jails and prisons. Indeed, jail and prison are often used interchangeably in colloquial language (Burns 2002). In the PSID, respondents are asked if, and at what age, they spent time in jail, but researchers interpret this measure as representing all types of incarceration (Boen, Graetz et al. 2022; Boen, Olson, and Lee 2022;).

Surveys also suffer from attrition and small sample sizes, and small sample sizes are especially inimical to assessing heterogeneity by race, gender, or other sociodemographic factors. Survey sampling frames also typically leave out incarcerated individuals, thus limiting and obscuring our understandings of various social phenomena (Pettit 2012; Western and Beckett 1999). Given that most surveys are not designed with criminal justice contact in mind, usually only a small number of people in a given survey's sample will have previously experienced system contact, and further breaking that down by group results in an even greater loss of statistical power. This is one reason we still know relatively little about the consequences of system contact for certain sociodemographic groups, such as women (and, more specifically, mothers), Native Americans, and individuals who live in rural counties. Finally, the smaller sample size, combined with less regular data collection (most surveys are not capturing data on a weekly or monthly basis) means that survey data are typically not conducive to most conventional econometric techniques for causal inference. These techniques, which we will describe in a later section, typically rely on precise discontinuities or timing-based shocks that are not identifiable in most surveys.

Administrative Records

The other type of commonly used quantitative data, which has become more frequently used in recent years, is administrative criminal records. Administrative records are prospectively collected and track individual-level information by an official entity—in this area of research, typically a county, city, or state government agency—for the purpose of record-keeping. For example, state prisons maintain records of all incarcerated people in their facilities. In some cases, these data may be linked

17. Also, many surveys designed with the criminal justice system as the focus, like the Family History of Incarceration Survey (FamHIS; Enns et al. 2019), are not longitudinal in nature, making it difficult to ascertain the effects that contact with the criminal justice system could have on individuals.

18. PSID asks about system contact in the *Transition into Adulthood Supplement*; the NLSY79 asks only in the 1980 wave.

to other administrative sources, such as schooling data or voter records. In table 3, we offer a representative overview of administrative data used in studies examining the fallout from criminal justice contact. This table shows that administrative record usage spans several dimensions—locales, years, outcomes, and disciplines. The table also shows that these criminal justice data have been linked to a wide array of other official record sources, which broadens the scope of possible empirical inquiries.

Administrative records have several highly appealing features. In contrast to surveys, administrative records provide an enormous amount of detailed information on the specific nature of a given individual's criminal justice contact that is not subject to the biases of self-reported survey data. Researchers could know, for example, the exact criminal charge, date of entry and exit in and out of a facility, and whether an individual recidivates. This is beneficial from a descriptive standpoint as well as from a causal inference standpoint, since it allows for sufficient sample size and detailed timing and sentencing data that can be leveraged in ways that we will describe in the methodology section.

Administrative records, however, have significant drawbacks. First and foremost, administrative records are justifiably difficult to access. Given their highly sensitive nature, there are many challenges to accessing and using these data. Researchers often need to spend a significant amount of time (sometimes years) building relationships with the government agencies and employees who oversee and provide access to these data, and researchers are subject to myriad administrative and bureaucratic hurdles along the way beyond standard Institutional Review Board (IRB) processes and requirements for human subjects research. The hurdles might include establishing and maintaining relationships with officials who maintain the data, unwillingness of officials to share data, significant bureaucratic restrictions on data sharing, and stipulations on what can be produced with the data. While necessary for protecting the rights and identities of individuals who are in the system, the hurdles make gaining access to these data difficult. Also, while some administrative data are publicly available, the means for accessing these data is often opaque or insufficiently organized to enable the creation and processing of usable datasets for quantitative analysis (see the Vera Institute's *Police Data Transparency Index* for more information on police data quality).

Another shortcoming is the multiple sources of inaccuracy and inconsistency in the ways race and ethnicity are reported in criminal justice data (McCormack et al. 2023; Finlay et al. 2024). For instance, jurisdictions vary in how their data management systems are set up to classify race and ethnicity, and these systems can change over time. In addition, classification is subject to both conscious and unconscious misreporting and bias on behalf of the bureaucrat responsible for determining and inputting an arrested or incarcerated individual's race (Saperstein and Penner 2012). For example, data on the racial and ethnic identity of people encountering police may be inaccurate. Ayobami Laniyonu and Samuel Donahue (2023) show discordance in police officer racial classification and those same individuals' self-classification. Elizabeth Luh (2020) shows that the average Texas trooper is more likely to falsely report that failed searches of Hispanic people are searches of White people, compared to successful searches of Hispanic people. Thus, aggregate-level exposures of contact by race and ethnicity may misrepresent the true magnitude of disproportionality in contact, thus biasing causal estimates of exposure for all racial and ethnic groups and by race and ethnicity. Laniyonu and Donahue (2023, 295), for instance, point out that in their sample, they "find that officer classification of Hispanics as White may lead analysts to incorrectly conclude that Hispanics are no more likely than Whites to be cited by police." Recent research by Keith Finlay and colleagues (2024) documents the extent of this misreporting and finds that 17 percent of felony or misdemeanor defendants (such court data often informs jail records) and 10 percent of people in prison are listed with a race or ethnicity in administrative data that does not match their Census Bureau composites (which is primarily self- or family-reported). Agency-recorded race and ethnicity are the least accurate for Hispanic, Native

Table 3. Administrative Data Overview

Location	Year(s)	Information	Articles Using
		City or County Administrative Records	
Anonymous mid-size city	2015–2017	Arrest data linked with school records	Mark et al. 2022
Cook County (Illinois)	1990–2008	IL imprisonment records linked with Cook County juvenile court records and Chicago Public Schools data	Aizer and Doyle 2015
Cuyahoga, Franklin, and Hamilton Counties (Ohio)	1992–2017	2021 paper linked adult court cases to birth records, school data, voter registry in all 3 counties; juvenile court records only for Cuyahoga 2022 paper linked adult court cases to death records in all 3 counties	Norris et al. 2021, Norris et al. 2022
Harris County (Texas)	2008–2012 1980–2017	Harris County data on misdemeanor case defendants linked with voter records Harris County court and jail records linked with state-wide prison, criminal history, and earnings/employment data	White 2019b; Mueller-Smith and Schnepel 2021
Hillsborough County (Florida)	2012–2020	Hillsborough PD public traffic stop data linked to voter records	Ben-Menachem and Morris 2023
Milwaukee (Wisconsin)	2008–2009	Milwaukee PD data on nuisance property citations linked with 911 calls, and neighborhood-level crime statistics, property code violations, and population estimates	Desmond and Valdez 2013
New York City	2003–2012	NYPD data on geocoded police activity linked with NYC schools student-level data	Legewie and Fagan 2019
Orleans, Jefferson, Plaquemines, St. Bernard, and St. Tammany Parishes (Louisiana)	2001–2006	LA parolee records linked with LA criminal justice system data and parish-level characteristics	Kirk 2009
Suffolk County (Massachusetts)	2000–2020	Suffolk DAO criminal complaint records (court records and non-prosecuted complaints)	Agan et al. 2023
		State-Level Administrative Records	
Michigan	2003–2013	Michigan DOC felony records linked with employment information from the Michigan UI Agency	Harding et al. 2018
Florida and California	1987–2002	Linked state prison (FL) and federal judicial system (CA) records to employment records	Kling 2006

Source: Authors' compilation.

American (AI/AN), Asian, and Pacific Islander individuals (Finlay et al. 2024). However, Finlay and colleagues (2024) show that the consequences of this misreporting extend beyond these groups: imprisonment rates based on National Prisoner Statistics (NPS) data are underestimates for Black and White people, though underestimates are most extreme for Native American (AI/AN) people.

Administrative records are also constrained from a geographic standpoint; there is very limited national individual-level data on criminal justice contact. Only one survey collects original data on a national scale: the National Crime Victimization Survey from the Department of Justice, which shifts the reference frame away from contact with the criminal justice system and toward victims of crime, potentially producing more reliable macro-level crime and victimization statistics (Lauritsen et al. 2015). Therefore, researchers are often restricted to conducting their analysis of system contact at the state, county, or city level. This often makes it difficult to compare results across jurisdictions or to generalize to the US at large. The one "national" data source of system contact, the FBI's Uniform Crime Reporting (UCR) Program, is simply a repository where local police departments and law enforcement agencies provide their own data of crimes reported to the police. Note, however, that UCR reporting is highly uneven, and its data quality has been questioned for decades (see Blackman and Gardiner 1984; Kaplan 2023, Ch. 2.2.). As table 3 shows, the result is that there is a wide range of research across states, cities, and counties across the US, but cross-state and national estimates are sparse.

Finally, administrative data often lack demographic information. Demographic information is also not uniformly collected across agencies when it is collected (for instance, in differentiating between race and ethnicity). It is not always a guarantee that even basic constructs like gender, race, and ethnicity will be reliably recorded,[19] and socioeconomic information (such as employment and income) is typically not included in these records at all. This limits researchers' ability to provide a comprehensive descriptive picture of their sample, assess heterogeneity by socioeconomic status (SES) and related measures, or consider how familial and socioenvironmental influences may factor into their analytic framework.

Ongoing Data Efforts

In the introduction, we argue that new, innovative, and robust data efforts were needed in the criminal justice space. Here, we highlight two recent (and ongoing) efforts on which future initiatives can build. The first is the Criminal Justice Administrative Records System (CJARS), which is hosted at the University of Michigan and involves intensive collaboration with the United States Census Bureau. CJARS is conducting an ongoing effort to integrate and harmonize administrative records related to punishment across the US, which is useful given that (as we highlight in our overview of the literature later) most studies using administrative data focus narrowly on just one or two counties. As of January 2024, CJARS contained at least some criminal justice records from most US states and counties. CJARS has produced several recent publications in high-impact journals with their collaborators from the Census Bureau, which provide further description of the initiative and available data (Deshpande and Mueller-Smith 2022; Finlay and Mueller-Smith 2021; Finlay et al. 2023).

The second is the PHDCN+, which is an extension of the PHDCN survey. The original PHDCN began with cohorts of youth and their families in the mid-1990s, and a subsample was followed into adulthood (through 2021). In recent years, these data have been linked to administrative criminal records via the Illinois Criminal History Record Information system (CHRI). Such linkage is particularly impactful and innovative, as it is the only (to our knowledge) large-scale longitudinal survey to be linked to administrative criminal records in the contemporary era—at least in the US context. The PHDCN+ has published several recent pieces in high-impact journals that provide more details about survey structure and data linkage process for the administrative data

19. See Luh (2020) for an example. Also see Durlauf and Heckman (2020).

(Neil and Sampson 2021; Montana et al. 2023; Wildeman et al. 2024).

Methodologies Used

The empirical and methodological landscape is continuously changing and developing. In this section, we provide a breakdown of the most common analytic strategies used to document the fallout from criminal justice contact. Building on the data section, we also highlight how different empirical approaches are better (or worse) suited to distinct types of data, and what implications this has for researchers and our understanding of criminal justice contact. Finally, this section is complemented by table 4, which provides more detail on the data sources and examples of papers across various outcomes, disciplines, and geographic areas that use these different methods. We also present a comprehensive and detailed analysis of Google Scholar data to show how the use of different methodologies varies by outcome.

Matching and weighting strategies—such as propensity score matching or inverse probability of treatment weighting—are frequently used with survey data. These condition-on-observables strategies are beneficial because they are more accessible and easier to implement in the absence of panel data and measures or some exogenous shock but are limited in their ability to account for unobserved variation across treatment and control groups. As one example, to tease out the effects of juvenile arrest on high school completion, David Kirk and Robert Sampson (2013) use propensity score matching with survey data to make youth who experienced juvenile arrest "look" more like those who did not. A large body of literature on parent incarceration (see Wildeman 2020) relies heavily on these matching and weighting approaches.

Longitudinal fixed effects models rely on panel data, where repeated measures are taken from the same individuals over time. These models control away time-invariant effects within individuals but are subject to the oft-inconsistent ways that surveys ask about criminal justice contact across survey waves, as well as variation in the time span between survey waves. Less frequently employed is a "strategic comparison group" strategy, which is also typically used with survey data and relies on the creativity of the researcher to leverage the timing or some other feature of a survey to produce a control group that is more like the treated group. In the context of parental incarceration, where this approach has mostly been used, Christopher Wildeman (2020) lauds this "ingenious way to isolate effects" with survey data. For example, Garrett Baker (2023), Erin McCauley (2020), and Lauren Porter and Ryan King (2015) use the retrospective age-of-parent incarceration questions in Add Health to compare those who had already experienced parental incarceration when the outcomes were measured to those who would later experience parental incarceration after the outcomes were measured. These two groups, who both experience parental incarceration—just at times that happen to come before or after the outcomes are measured—are more similar than those who do and do not experience incarceration.

Regression discontinuity and difference-in-differences (DiD) models are two central econometric techniques for recovering causal effects. Regression discontinuity modeling relies on some type of cutoff or discontinuity for eligibility or participation in a program, while DiD relies on panel data and some type of exogenous shock that occurs to one group (that is, the treatment group). To take one example, Michael Mueller-Smith and Kevin Schnepel (2021) use a regression discontinuity framework with administrative records from Harris County (Texas) where they leverage two natural experiments—of a new penal code's enactment and of a failed ballot measure—that significantly and sharply decreased and increased diversion likelihood, respectively, around the two cutoff dates when those changes took place.

Another promising and increasingly used econometric technique uses the random assignment of judges or prosecutors to an individual's legal case as an instrumental variable (often referred to as a "judge IV" strategy). In this analytic approach, researchers use administrative records to take advantage of the fact that, for instance, judges are widely heterogeneous in their stringency or harshness of sentencing. Given that judges are (often) randomly assigned to cases, researchers can then compare similar cases that vary only in whether

Table 4. Overview of Empirical Methodologies

Method	Description/Objective	Example of Articles Using Method	Datasets Used
Matching/weighting	Match or weight on observable characteristics to make control and treatment groups more similar	1) Wildeman 2010; Haskins 2014 2) Brayne 2014 3) Kirk and Sampson 2013	1) FFCWS 2) Add Health 3) PHDCN
Fixed effects	Group means held fixed; in panel data controls for effects of time-invariant variables	1) Massoglia et al. 2013 2) Testa et al. 2022	1) NLSY79 2) Pathways to Desistance
Strategic comparison group	Leverage different timing of treatment or other sample characteristics to generate a more appropriate comparison	1) Western 2002 2) Baker 2023; Porter and King 2015	1) NLSY 2) Add Health
Regression discontinuity	Leverages a cutoff or other threshold and compares those on either side	1) Mueller-Smith and Schnepel 2021 2) Mitchell et al. 2016	1) TX admin 2) FL admin
Difference-in-differences or event study	Compares changes in outcome over time across groups who are and are not exposed to some exogenous shock or treatment	1) Legewie and Fagan 2019 2) Mark, Geller, and Engberg 2022	1) NYC admin 2) Anonymous city admin
Random assignment of judges/prosecutors/etc.	Uses a 3rd variable that affects Y only through its effect on X, which allows estimation of part of the variation in the predictor that is uncorrelated with unobservable factors affecting both predictor and outcome	1) Norris et al. 2021; Norris et al. 2022 2) Agan et al. 2023 3) White 2019b 4) Aizer and Doyle 2015 5) Harding et al. 2018	1) Ohio admin 2) Suffolk County (MA) admin 3) Harris County (TX) admin 4) Cook County (ILL) admin 5) Michigan
Experiment/RCT/audit study	Randomize treatment to minimize differences between groups	1) Pager 2003; Pager et al. 2009; Agan and Starr 2018	N/A

Source: Authors' compilation.

they were assigned to a more or less stringent judge. As one example, David Harding and colleagues (2018) use the random assignment of judges to felony cases in Michigan to estimate the impact of being sentenced to prison (compared to probation) on labor market outcomes.

However, we stress that in addition to the generic generalizability issues articulated elsewhere in this paper, judge IV strategies come with two additional issues. The first is the monotonicity assumption, which considers whether judges are uniformly harsh (or lenient) in their sentencing propensity (for more see Frandsen et al. 2023; Mueller-Smith 2015). Second is the fact that these studies are (typically) analyzing individuals who are on the margin of being incarcerated. In extrapolating the meaningfulness of a given result, one must consider, colloquially, the "localness" of the local average treatment effect (LATE) that is being estimated. While this is less frequently acknowledged in articles using this strategy, Harding and colleagues (2018, 83) clearly explain the tradeoff between internal and external validity and ensuing ramifications for both researchers and policymakers:

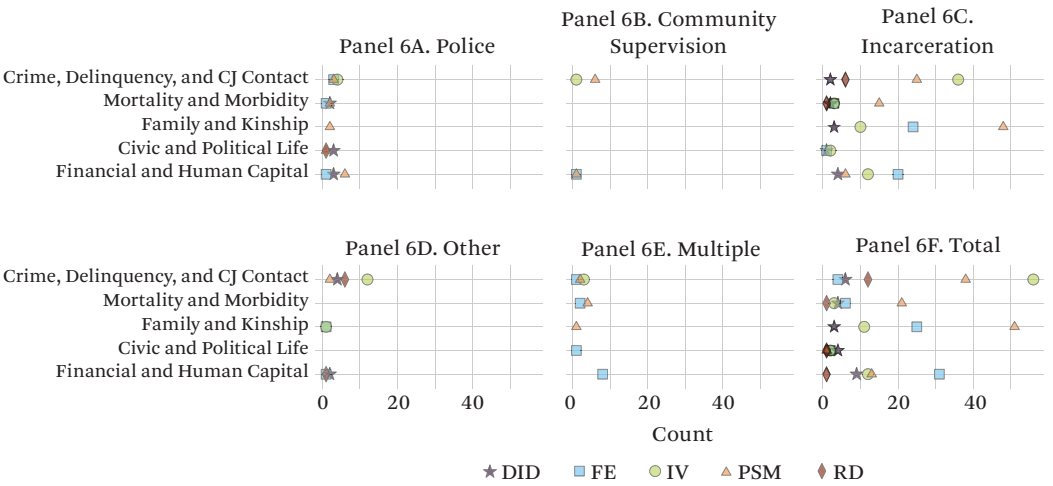
> Causal effect estimates from an instrumental variables analysis are LATE. This means we are estimating the effect of incarceration in prison as compared to probation among individuals for whom the judge assigned made the difference between prison and probation. Those are individuals who are on the margin between prison and probation. Our estimates do not provide average treatment effects for all individuals sentenced to prison in Michigan. As a result, they should not be interpreted as informative regarding radical policy changes such as decarceration on a massive scale, which would surely involve individuals who are far from the margin on which the effects in this article are estimated.

While the authors here stress the limitations regarding informing significant policy changes, we note that such limitations are also relevant in comparing results using this analytic approach to others using different methods and data that may use a less narrow sample and test. Such studies that analyze postincarceration outcomes also inherently rely on exit samples in the sense that they are samples of individuals who are being released from jail or prison (and therefore are oversamples of repeat offenders), thus further reducing their representativeness. Ultimately, we encourage researchers to be transparent about these issues and related tradeoffs by providing context along the lines of what Harding and colleagues (2018) state, instead of relying on readers to be well versed on the intricacies of econometric analyses.

Our discussion about judge-based instrumental variable designs also relates to our prior section on theoretical perspectives on race-specific associations. Recent research on structural racism and more specifically racial disparities in the criminal justice system suggests that Black and White people who are on the margin of incarceration (the margin that judge IV designs leverage) may be quite different in terms of their respective socioeconomic background and criminal history (see also Kurlychek and Johnson 2019; Lee 2024). For instance, Rehavi and Starr (2014) show that in the federal system, Black individuals are subject to harsher sentences and prosecutorial charging decisions than otherwise comparable (that is, similar along many common characteristics such as criminal history and education level) Whites who were arrested for the same crime. Thus, the interpretation of judge IV analyses should also consider that results may be muddied by racial disparities within the distributions from which marginal arrestees (or parolees, defendants, and so forth) are drawn. Furthermore, these studies rely almost exclusively on administrative data, which, as discussed previously, is more likely than self-report data to be biased in terms of race and ethnicity.

Finally, while researchers cannot ethically or practically conduct experiments or randomized controlled trials on whether someone has criminal justice contact, several experiments and audit studies have been conducted in creative ways that help improve our understanding of consequences of criminal justice system contact. In one seminal example, Devah Pager (2003) conducted an audit study where fake job

Figure 6. Statistical Methods Used by Outcome and Treatment Analyzed (1992–August 2023)



Source: Authors' calculations.

applicants approached potential employers, and the only difference between applicants was whether they had a criminal conviction on record. While this user-initiated randomization is a desirable feature, the downside is that such studies may not fully capture the influence of social contexts and interactions that would occur in day-to-day life. For instance, in Pager's audit study, applicants had to convey their (fake) criminal conviction regardless of whether it was solicited by employers. As she notes, in "26% of cases where the application form did not include a question about criminal history, it was necessary to provide an alternate means of conveying this information" (Pager 2003, 951).

Figure 6 shows the count of peer-reviewed publications from 1992 through August 2023 that use common statistical methods to study different outcomes following criminal justice system contact. We also include National Bureau of Economic Research (NBER) working papers given their influence on the field. We break down these counts by the type of criminal justice system contact being examined; for papers comparing multiple stages of contact, we group the papers according to the more serious form of contact for example, a paper comparing prison and probation would be considered a paper on incarceration). Note that we include papers that exploit quasi-experimental variation in the timing of experiencing parental incarceration to net out stable but unobserved characteristics under the fixed effects category. We also include Inverse Probability of Treatment Weighting (IPTW) and other similar weighting and matching designs under propensity score matching. Finally, we count only sources that compare a distinct stage of contact to either another stage or no contact; we do not include sources exploring the texture of contact (for instance, differences in outcomes based on prison stay length).

The two takeaways from this analysis are depicted in figure 6. First, there is more research on outcomes related to recidivism and future criminal justice system contact than on other outcomes, and incarceration is the most common treatment to be studied. Second, the statistical methods most often used vary by the outcome being studied. For papers examining recidivism and future justice system contact, IV was used in almost 50 percent of papers; among all other outcomes, IV was used in at most 20 percent of papers. For papers analyzing family and kinship or health outcomes, propensity score matching was by far the most used method, while for outcomes related to financial and human capital, fixed effects was the most common method. DiD was most often used for civic and political outcomes, though this should be interpreted with caution because there were relatively fewer papers match-

ing our criteria that fell into this outcome category.

OVERVIEW OF RESEARCH ON FALLOUT FROM CRIMINAL JUSTICE CONTACT

In the remainder of this article, we present an overview of the literature on the fallout from criminal justice contact. We focus on peer-reviewed articles published since 2000 (emphasizing the most recent articles when possible) using data from the US. While other similar reviews have included discussion of non-US-based samples (often in Nordic or Scandinavian countries, as seen in Loeffler and Nagin 2022; Wildeman 2020), we choose here to focus only on US studies because of the substantive focus of this issue and the fact that the criminal justice system and sociodemographic structure of the US is unique among the developed world. We also pursue breadth and depth across types of contact and outcomes, and restricting to US-based studies provides more leeway to accomplish this goal in a (relatively) concise manner.

The ensuing sections proceed roughly in the same order as the sequencing of the actual criminal justice system in the US: we begin with the fallout from general criminal justice contact (arrest and police contact), then discuss incarceration (with particular attention paid to jail versus prison), and finally end with community supervision (parole and probation). Within each of these sections, we discuss the most prevalent sources of fallout focused on by existing literature. These include financial and human capital formation (employment, income, and education), physical and mental health and mortality, crime and delinquency and system contact, and civic and political engagement.[20] Finally, we discuss both direct (an individual's own) and indirect (familial or community-level) contact.[21] While each section follows these guidelines, the incarceration section is especially rigidly structured given its outsized presence in the literature.

In synthesizing the literature across these different outcomes and types of contact, we note that the findings are often quite textured, nuanced, and sometimes at odds with each other. We postulate that this does not automatically mean some papers are "wrong," or that differences are due solely to the many complex and countervailing dynamics at play in the real world, but that these diverging findings may also be a product of the specifics in many studies differing in oft-underappreciated ways. (The studies may differ, for example, in terms of geographic areas, historical periods, nature of system contact, and, most importantly for judge IV studies, the margin being considered). In thinking about these points, we argue that the field ought to be less dogmatic about particular data sources and methodologies, spend less time debating which studies are "right" or "wrong," and waste less ink claiming they have uncovered the effect of X on Y—and, instead, should pay more attention to how the various pieces fit together to help us more thoroughly understand the fallout from mass incarceration (which, as we outline later, is highly complex).

As Margaret Marini and Burton Singer (1988, 350) note in their commentary on causality in the social sciences: "Although statistical tools also play a critical role in the gathering of evidence there is no context-free statistical method or set of methods that defines causality. The process of causal inference usually involves multiple studies, which successively increase the degree of belief attached to a causal hypothesis." This is not merely a point about external validity and generalizability—which has been made previously, both in this review and by others—but is also one that acknowledges the tension between mass incar-

20. Despite the limited number of articles in this area, we include it in the hopes of spurring future research as well as the fact that, to our knowledge, no review article in this area has thoroughly included these studies.

21. The area of indirect contact has suffered perhaps more than any other from split lines of research along the operationalization of parent criminality versus criminal justice contact broadly versus incarceration specifically. As Christopher Wildeman (2020, 221) points out, "Studies in this area tend to be measuring the same thing but talking about it differently" (also see table 1 in Wildeman 2020).

ceration's vast, ever-changing shadow and the practicalities of observational quantitative research.[22] The effects of criminal justice contact may differ by whether someone was arrested, convicted, jailed, imprisoned, or paroled; by city, county, or state; by period and age; and by race, ethnicity, sex, socioeconomic status, and other sociodemographic characteristics. No one study's design, statistical technique, sample, or data source can—or should be expected to—illuminate the entirety of mass incarceration's shadow.

Police Contact and Arrest

While much attention has been paid to incarceration—hence the ubiquitous "mass incarceration" terminology—the nature of the criminal justice system is such that contact with police will (almost) always precede any custodial sanction. In this first literature review section, we present an overview of the literature on this proximate form of contact. As we are careful to discuss throughout the section, this contact can take many forms in the real world and is measured and operationalized rather heterogeneously across data sources and articles. We therefore endeavor to specify whether the type of contact in a given data source is arrest (while specifying formal or officially recorded versus self-reported arrest, see Kirk 2006); general contact with, trouble with, or exposure to the police; or other system contact left undefined.

As in each ensuing section, we discuss four frequently studied consequences of system contact: financial and human capital formation (employment and education); health and mortality; crime, delinquency, and system contact; and civic and political engagement. We are also attentive to direct (one's own) versus indirect (familial, peer, or macro-level) contact. Finally, unlike incarceration and community supervision, which are more pervasive (both in the real world and in data sources available to researchers) for adults relative to adolescents, police contact is highly common and salient for youth. Robert Brame and colleagues (2014) find that 30 percent of Black males and 22 percent of White males experience an arrest by age eighteen (also see Geller 2021; Puzzanchera and Hockenberry 2021). We therefore delineate consequences for juveniles wherever possible.

Financial and Human Capital Formation

A wealth of research in this area uses survey data to assess the relationship between juvenile or adolescent arrest and human capital formation—in particular, educational outcomes. Across different contexts and data sources, this research generally finds that arrest and other forms of criminal justice contact are deleterious to educational performance and attainment.

Using linked administrative and survey data, Kirk and Sampson (2013) echo Paul Hirschfield (2009) in finding a negative effect on high school dropout. Gary Sweeten (2006) further shows that self-reported criminal justice contact is associated with dropout, independent of delinquent behavior, and also that court appearances may be more influential than arrest. Related work similarly finds a negative association between arrest and college enrollment (Widdowson et al. 2016). Nicholas Mark and colleagues (2022) use linked administrative records to further examine this relationship and find that students miss more school after an arrest, especially because of court appearances and school suspensions. Attendance and school attachment may thus be important mechanisms for future research to consider.

Research on indirect or vicarious contact finds that macro-level police activity is also detrimental to youth education. Using administrative records from New York City, Joscha Legewie and Jeffrey Fagan (2019) show that exposure to higher levels of police activity in a child's neighborhood is detrimental to test scores, particularly for Black boys. Using data from Los Angeles, Desmond Ang (2021) finds a similarly deleterious effect of police violence.

22. While a discussion about the theoretical nature of "causality" is far beyond the scope of this review, see Megan Stevenson (2023) for some thoughtful commentary and empirical evidence in the context of the criminal justice system.

Finally, related work considers other aspects of human capital. This research finds that juvenile police contact and intervention are associated with unemployment and financial hardship in one's twenties (Lopes et al. 2012). Christopher Uggen and colleagues (2014) probe this relationship further, using an audit study to reveal that those who report an arrest that did not lead to conviction had lower rates of job interview callbacks, suggesting the robustness of this core finding.

Health and Mortality

Survey data generally indicate that young people who experience more frequent stops by police are more likely to self-report higher levels of emotional distress and symptoms associated with anxiety and trauma (Geller et al. 2014; Jackson et al. 2019). This relationship is moderated by perceived procedural injustice, with self-reported health outcomes most strongly predicted by stops perceived as intrusive or procedurally unjust (Geller et al. 2014; McFarland et al. 2019). Exposure to vicarious police stops are also associated with worse self-reported health (McFarland et al. 2019). Michael McFarland and colleagues (2019) also find that reports of direct and vicarious police contact were not related to caregiver-reported adolescent health. Arrests may be especially damaging. Naomi Sugie and Kristin Turney (2017) estimate that arrests explain almost half of the association between poor mental health and incarceration.

Spatial patterns of intensive policing are also tied to health. Using data from New York City, Alyasah Sewell and colleagues (2016) find that men in neighborhoods with more frequent police frisks and uses of force report higher levels of psychological distress; Sewell and Kevin Jefferson (2016) also link neighborhood frisk likelihood with a range of physical health outcomes.

Police encounters can also lead to physical harm and even death. In the economics literature, there have been mixed findings regarding how civilian race is related to police use of force. One study, which combined four different data sources, concluded that while there were racial disparities in nonlethal use of force, there were no racial differences in officer-involved shootings, conditional on police interactions (Fryer 2019). However, this study's conclusions regarding racial disparities in police shootings have been critiqued because of methodological weaknesses in modeling the probability of someone being shot by the police and overreliance on police narratives, which may very well be inaccurate (Durlauf and Heckman 2020; also see Knox et al. 2020). Indeed, other research demonstrates that police systematically misreport demographic characteristics (Luh 2020). A county-level analysis finds that Black Americans are more likely to be shot by the police even after accounting for differences in arrest rates (Ross 2015). Regardless of the exact causal mechanisms driving racial disparities in police use of force (for example, racial residential segregation and other manifestations of systemic racism versus explicit racial profiling), Black and Indigenous Americans are the most likely to experience the deadly outcomes stemming from police contact (Edwards et al. 2019).

Crime, Delinquency, and Recidivism

Using various survey data sources, Bianca Bersani and colleagues (2022), Juan Del Toro and colleagues (2019), and Stephanie Ann Wiley and colleagues (2013) find that juvenile arrest or police contact leads to increased offending and arrest. Elaine Doherty and colleagues (2015) and Akiva Liberman and colleagues (2014) both use different sources of administrative and survey data and similarly find evidence for a criminogenic effect of arrest, though the study by Liberman and colleagues uncovers distinct processes for rearrest versus reoffending.

Evidence of indirect effects primarily focuses on familial and peer contact. As Erin Tinney (2023) shows, also using self-reported survey data, consequences may be "sticky" across friendships. Among a sample of rural youth, she finds that having a friend who is arrested increases one's likelihood of arrest the following year. Wildeman and colleagues (2024) provide evidence of intergenerational transmission of criminal justice contact from parent to child, using self-reported parent po-

lice contact and administrative arrest records for children.

Civic and Political Life

The literature on police contact and political participation has been especially fertile in political science in recent years, though with numerous results that point to contrasting or countervailing empirical relationships. Vesla Weaver and Amy Lerman (2010) find a generally demobilizing effect across different forms of criminal justice contact in national survey data. And using potentially more stringent causal methods and administrative records in one county in Florida, Jonathan Ben-Menachem and Kevin Morris (2023) also uncover a negative relationship between traffic stops and voting.

However, with both national observational data and a survey experiment, other research suggests mobilizing effects of criminal justice contact. For instance, Leah Christiani and Kelsey Shoub (2022) find that experiencing low-level police contact may actually increase political participation, and they further note that these effects are largest for those who hold more favorable opinions of the police (which, they add, is often White individuals). Several studies on indirect police exposure also suggest a countervailing relationship. Using national data, Hannah Walker (2020) finds that vicarious contact with the criminal justice system may spur political participation and may especially do so in the face of perceived injustice, while Kevin Morris and Kelsey Shoub (2023) find that police killings increase electoral participation at a neighborhood level.

Incarceration

The ubiquity of the phrase "mass incarceration" makes clear that the primary form of criminal justice contact discussed in most policy and media conversations is the experience of being held in a jail or prison facility. Unsurprisingly, quantifying how incarceration impacts various outcomes has dominated the "collateral consequences" literature over the last few decades. However, this common representation of incarceration as a monolith belies significant variation. As qualitative work (Walker 2022) makes clear, being held in a jail versus a prison can be entirely different experiences. In addition, many surveys cannot speak to the length of the incarceration regardless of facility type, whereas in many administrative datasets, length of incarceration is usable (White 2019b; Patterson 2013). While this difference is sometimes pointed out in passing in some previous quantitative articles and reviews (see the section "Collateral Consequences of What?" in Kirk and Wakefield 2018), there are still far too many analyses—and ensuing academic and policy discussions—that involve "incarceration" without being specific about the context. As we show throughout this section, different data sources offer different measures of incarceration and, importantly, may not always provide sufficient detail to distinguish between whether a person was in jail or prison or both. In general, though not as a rule, survey data often do not lend themselves to distinguishing between jail and prison incarceration, while research with administrative records tends to focus on prison sentences.

Other issues, such as how accurately survey respondents distinguish between jail and prison, notwithstanding, we stress that researchers should at least provide more robust discussions around their specific measure of incarceration and implications for interpretation. In reviewing the literature for this section, we were struck by how many articles do not specify the type of incarceration they are measuring, imply one form of incarceration without stating it explicitly (such as by restricting the sample to felonies, which implies prison as opposed to jail incarceration), or even refer to "imprisonment" as shorthand when they cannot parse out the nature of the incarceration. Similarly, and as discussed in previous reviews (Kirk and Wakefield 2018; Wildeman 2020), researchers must be cognizant about the counterfactual to whatever type of incarceration they are analyzing. Are they comparing imprisonment to jail, imprisonment to no incarceration, jail to probation, or even a long prison sentence versus a short one? (See, for instance, Jung 2011.) Thus, in this section we take care to discuss research on the fallout from jail and prison incarceration separately, paying close attention as well to how particular studies are (or

are not) able to distinguish between the two in their data.[23]

Financial and Human Capital Formation

In this section, we examine the impact of direct and indirect incarceration on financial and human capital formation.

Direct Incarceration Survey research on incarceration and human capital formation relies heavily on the NLSY79 and NLSY97, which mostly leaves researchers unable to distinguish between jail and prison. Seminal research using the NLSY79 by Western (2002) shows that undefined incarceration reduces wage growth and that incarceration accounts for roughly 8 to 12 percent of the wage gap between White and Black and White and Hispanic adults, respectively. Robert Apel and Gary Sweeten (2010) used the NLSY97 to further uncover that incarcerated individuals dropped out of the labor force (that is, were not searching for employment) at higher numbers, and for longer periods of time, a finding that helps explain employment and wage gaps. These trends are corroborated in other datasets (Geller et al. 2006). However, Apel and Kathleen Powell (2019) find in the NLSY97 that the incarceration wage gap is most pronounced for Black individuals.

Administrative records in this area primarily focus on imprisonment, though one study (Dobbie, Goldin et al., 2018) finds a negative relationship between pretrial detention and employment. Generally, however, extant research specific to imprisonment suggests that employment dips short term but then rapidly returns to pre-prison levels (Kling 2004; 2006; Lyons and Pettit 2011). Similarly, research from Illinois further finds that there is no significant relationship between imprisonment and five-year employment rates (Loeffler 2013).

However, David Harding and colleagues (2018) provide important texture to these findings. Using administrative records from Michigan, they assess heterogeneity in the effects of imprisonment (compared to noncustodial sentencing) by race and pre-imprisonment work history. They find negative post-release effects on employment only among Whites with a prior work history. Black people with a work history experience no downturn, while for both Black people and White people with no formal work history, imprisonment has a short-run positive effect on employment. The authors do find that the positive effect, however, fades over time.

Research on incarceration and educational attainment is relatively scarce, primarily because incarceration's impacts would have to take place early enough in the life course to affect one's education, and it is generally rare—in both the real world and in data sources accessible to researchers—to see adolescents formally incarcerated. However, existing evidence from Anna Aizer and Joseph Doyle (2015) using administrative records in Chicago suggests that juvenile incarceration in a juvenile facility has large deleterious impacts on high school completion. Finally, incarceration is also associated with lower rates of homeownership and delayed homeownership (Bryan 2020), though housing instability among the formerly incarcerated may be driven by felony conviction status (Bryan 2023).

Indirect Incarceration Literature on indirect or familial incarceration and employment is generally scant. The only existing study to our knowledge is by Holly Miller and J. C. Barnes (2015).[24] Using survey data, they find a negative association between paternal incarceration and income but not employment. However, multiple studies using survey data suggest that parental incarceration harms families through financial strain or material hardship (Geller et al. 2011; Schwartz-Soicher et al. 2011). Findings for education-related outcomes for children

23. This means when we use the term "incarceration" ("incarceration had an effect on health"), the study being referred to does not distinguish between prison and jail. When a study does specify the type of incarceration, we note it directly. As we explain in our discussion, few studies differentiate prison and jail incarceration.

24. We note that all the studies mentioned in the next section combine or do not delineate between parental jail and prison incarceration, and thus we focus on pointing out the type of data while naming "incarceration" broadly.

who experience parental incarceration are highly mixed. Joseph Murray and colleagues (2012) use survey data to find no association between parental incarceration and school achievement or performance, which Samuel Norris and (2021) and Rosa Minhyo Cho (2009a) corroborate using administrative records from Ohio and Chicago, respectively. Cho (2010), however, does find, using administrative data, a deleterious effect of maternal incarceration on high school graduation, mirroring the work of Holly Foster and John Hagan (2009), who use survey data. In terms of grade retention, Kristin Turney and Anna Haskins (2014), who use survey data, find a negative association for paternal incarceration, while Cho (2009b), using administrative data, finds a positive effect of maternal imprisonment specifically.

Health and Mortality

Next, we turn to reviewing the impacts of direct and indirect incarceration on outcomes related to health and mortality.

Direct Incarceration Multiple studies using various data and types of incarcerations demonstrate among those in jail and prison high rates of chronic conditions and infectious diseases (Hammett et al. 2002; Wilper et al. 2009) as well as vitamin deficiencies (Nwosu et al. 2014). Other surveys show that incarceration is associated with post-release health conditions related to hypertrophy, hepatitis, and tuberculosis (Wang et al. 2009; Massoglia 2008).

However, these studies tend to rely on data sources where individuals are not closely (if at all) followed before incarceration, and thus it is unclear whether incarceration is causing health deficiencies or merely reflecting preexisting health issues. Incarceration may also lead to the diagnosis of health issues. As Wildeman and Emily Wang (2017) point out, correctional facilities may sometimes provide incarcerated adults with their first regular access to medical care. The causality conundrum is furthered by research suggesting that incarceration's effects on health arise only post-release (Schnittker and John 2007), that chronic conditions may be better managed in prison (Meyer et al. 2014), and that infectious diseases are rarely transmitted in prison (Spaulding et al. 2017).

Research on mortality further complicates the empirical picture of how incarceration affects health. While some average estimates show an increase in overall mortality (Sebastian Daza and colleagues (2020) find a loss of life expectancy due to incarceration of about four to five years at age forty-five), particular attention has been paid to heterogeneity by sex and race. Results seem to diverge especially by data source. For instance, using NLSY79 data, which do not distinguish between jail and prison, Michael Massoglia and colleagues (2014) find higher odds of mortality among formerly incarcerated women but not men, while Benjamin Bovell-Ammon and colleagues (2021) find increased mortality post-release for Black individuals but not for non-Black individuals.

However, using various administrative data sources, both Anne Spaulding and colleagues (2011) and Patterson (2010) find that Black males had lower death rates in prison, and White males had higher death rates. Patterson (2010) also finds higher mortality for females during imprisonment compared to males. Other studies using administrative records find that, on average, mortality risk is cut roughly in half during incarceration (Norris et al. 2022). Importantly, however, Norris and colleagues (2022) find no post-release mortality risk (and thus, the net effect in their study is a reduction in long-term mortality) while Spaulding and colleagues (2011) find higher post-release mortality (to the extent that the net effect is an increased risk in long-term mortality).[25] Finally, Patterson (2013) finds that length of imprisonment is significant: each additional year spent in prison is associated with a two-year decline

25. To reemphasize our previous discussion on generalizability and the importance of appreciating contextual differences across studies, we note that Norris and colleagues (2022) use data from three counties in Ohio from 1990 to 2017 and do not distinguish between jail and prison, while Spaulding and colleagues (2011) use data from one state prison in Georgia from 1991 to 2006. Thus, it is not necessarily the case that these findings are "conflicting"; it may be the case that the effects and ensuing results genuinely differ across such contexts.

in life expectancy, though this heightened mortality risk fades over time.

Although some previously mentioned studies have found evidence that the length of time spent incarcerated has important implications for key health outcomes such as mortality, even short spells in jail or experiencing jail churn can be damaging for individuals with chronic mental and physical health problems owing to disrupted healthcare. In a survey of people incarcerated in the San Francisco County Jail, for example, Wang and colleagues (2008) show that jailed individuals with chronic diseases are less likely than the general population to have a regular source of care—except for jailed people with HIV, who receive discharge planning.

Indirect Incarceration The literature on indirect and familial consequences of incarceration for health focuses especially on the mental health of incarcerated individuals' partners and children, as well as the physical health and behavioral issues of children. This existing scholarship relies almost exclusively on surveys and thus generally lacks both the ability to distinguish the type of incarceration and stringent causal identification strategies.

Across three different survey datasets, Haskins (2015), Wakefield and Wildeman (2011), and Turney (2014) find that parental incarceration is associated with various mental health, behavioral, and developmental issues. Wildeman and Turney (2014) and Turney and Wildeman (2015), however, find that in the case of maternal incarceration, average associations are mostly null and vary by race and socioeconomic status: behavioral problems are reduced among White children and well-being is harmed most for higher-SES children who are less likely to experience maternal incarceration. Using Add Health data, Michael Roettger and Jason Boardman (2012) find that parental incarceration is associated with increased body mass in children, while Rosalyn Lee and colleagues (2013) find negative associations for various mental health outcomes (such as depression and anxiety) and physical health outcomes (such as cholesterol, asthma, and HIV or AIDS).

Extant research also suggests that married and other romantic partners often experience disruptions in the wake of incarceration (Apel 2016; Lopoo and Western 2005). Wildeman and colleagues (2012) find that paternal incarceration increases mother's mental health issues. Hedwig Lee and colleagues (2014) reveal that women suffer from a variety of health problems in the wake of familial incarceration.

Finally, a handful of studies consider community or macro-level associations between both prison and jail incarceration rates and mortality. Specifically, Wildeman (2012a) finds that each additional imprisoned individual (per one thousand state residents) is associated with a decline of .09 years of life expectancy, and Sandhya Kajepeeta and colleagues (2021) find that a one per one thousand within-county increase in the jail incarceration rate is associated with an increase in various specific mortality causes. Additionally, Wildeman (2012b) uncovers an association between imprisonment and infant mortality at the state level.

Crime, Delinquency, and Recidivism

Here, we discuss research on how direct and indirect incarceration contribute (or don't) to future crime, delinquency, and recidivism.

Direct Incarceration A long tradition of theorizing about the "criminogenic" nature of incarceration underlies the field of criminology in particular (Clemmer 1940, Sykes and Matza 1957), yet just fifteen years ago, Daniel Nagin and colleagues (2009, 115) lamented that "remarkably little is known about the effects of imprisonment on reoffending." While the literature has developed since their review, there is substantial uncertainty about the empirical nature of incarceration's criminogenic potential, especially among estimates using administrative records.

Using similar judge instrumental variable methodologies with administrative data from three geographic areas (Washington, DC; Cook County, Illinois; and six counties in Pennsylvania), numerous studies have found null effects of imprisonment on both short- and long-term rearrest and recidivism (Green and Winik 2010; Loeffler 2013; Nagin and Snodgrass 2013). Evan Rose and Yotam Shem-Tov (2021), however, find that imprisonment in North Carolina reduces recidivism in the short-term (three years post-

sentencing) and long-term (eight years post-sentencing).

Andrew Jordan and colleagues (2024) and Harding, Siegel, and Morenoff (2017) use administrative records from Chicago and Michigan, respectively, to provide some potential additional clarity regarding the effects of imprisonment. Jordan and colleagues (2024) show that incarceration causes a lasting reduction in recidivism only for first-time offenders, but not among repeat offenders. Harding, Siegel, and Morenoff (2017), on the other hand, find no effects on recidivating for new offenses but do find an overall increase in reimprisonment—likely owing to technical violations of parole. As Charles Loeffler and Daniel Nagin (2022, 138) mention in their review of this literature, "This finding illustrates how the commonly used measures of recidivism (rearrest, reconviction, and reimprisonment) are not always interchangeable for the purposes of estimating the effects of correctional interventions" and that more theoretical and empirical attention ought to be given to differences between criminal offending and system contact.

Research on juvenile incarceration and on pretrial (that is, jail) detention is similarly nuanced. While some research suggests that juvenile incarceration is associated with higher risk of longer-term (that is, adult) incarceration (Aizer and Doyle 2015; Walker and Herting 2020), other research suggests a net benefit (that is, a decrease in recidivism) for short-term re-incarceration as a juvenile (Hjalmarsson 2009). For pretrial detention, Dobbie, Goldin, and Yang (2018) estimate null effects on recidivism while Paul Heaton and colleagues (2017) find a positive association. Both areas are ripe for future research.

Indirect Incarceration Despite a long tradition of studying how criminality is transmitted from parent to child (Dugdale 1877; Glueck and Glueck 1950; Goddard 1912), and the fact that a number of studies have pointed out the commonality and disproportionality of experiencing parent incarceration (Wildeman 2009; Enns et al. 2019; Finlay et al. 2023), recent quantitative work on parent incarceration (as opposed to arrest or general criminal justice contact, covered in the earlier section) and children's crime and delinquency is scant.[26]

Existing research almost exclusively uses survey data to examine youth outcomes related to delinquency and criminal activity as opposed to formal system contact (that is, arrest or incarceration). Such delinquency outcomes tend to consist of index measures combining reported violence, theft or robbery, and selling drugs. Michael Roettger and Raymond Swisher (2011) find an increase in delinquency (and arrest), and Joseph Murray and colleagues (2012) find an increase in theft. Lauren Aaron and Danielle Dallaire (2010) find no association. Lauren Porter and Ryan King (2015) provide further texture by breaking out types of delinquency and using a potentially more rigorous empirical strategy. In their less rigorous models, they find negative associations for both expressive delinquency (getting into fights, causing property damage) and instrumental delinquency (theft, burglary, selling drugs), but when switching to a more rigorous comparison group, the association for instrumental delinquency is reduced to null.

Finally, the one study that considers parental incarceration using administrative data reveals that parent incarceration is associated with a decrease in children's likelihood of being incarcerated themselves (Norris et al. 2021). Together, these findings beckon for more research—across various samples and contexts—that attempts to parse these divergent findings across delinquency and actual system involvement for youth.

Some have also suggested that at high enough levels, incarceration may have a criminogenic effect in neighborhoods as the churn of residents to and from prisons and jails disrupts and strains social networks and undermines informal social controls (Rose and Clear 1998; Clear 2007). A few empirical studies have

26. The research is especially thin in the US context; a relatively large number of peer-reviewed (Hjalmarsson and Lindquist 2012; Kinner et al. 2007; Murray and Farrington 2005; Murray et al. 2007) and non-peer-reviewed (Dobbie, Grönqvist et al. 2018; Bhuller et al. 2018) studies have analyzed parent incarceration and children's crime, system contact, and delinquency using non-US samples.

attempted to test this hypothesis, which is called coercive mobility, but more research is needed (see Travis et al. 2014 for a review).

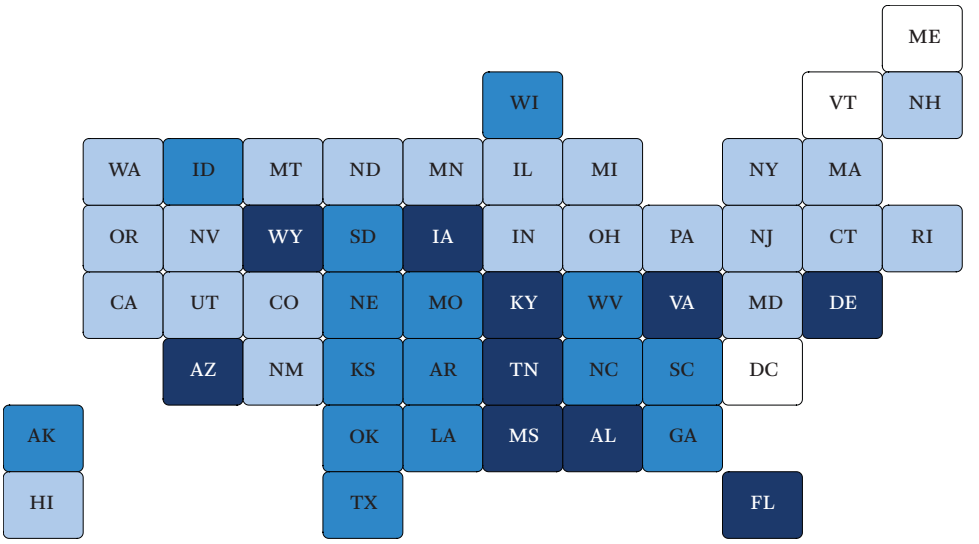
Civic and Political Life

Because of the relative paucity of research on consequences related to civic and political life (itself a shortcoming in the literature), we combine the direct and indirect sections under one umbrella here. Ariel White (2019b) uses administrative records and finds that jail sentences—typically very short in nature—lead to a substantial decrease in voter turnout for Black individuals but not White individuals. This finding builds on seminal work by Weaver and Lerman (2010), who use two national surveys to show that incarceration (jail or prison undefined) is associated with reduced likelihood of voting and being registered to vote.

White (2019a) further extends this line of inquiry by looking at indirect political consequences of incarceration in a jail. Using administrative records, she finds that experiencing a household member going to jail has a small and short-lived negative effect on voting likelihood, but this effect does not last beyond a couple months. At the neighborhood level, Traci Burch (2014) finds that neighborhoods with larger concentrations of people entangled with the criminal justice system vote at lower rates, while Kevin Morris (2020) shows that neighborhoods with a larger share of disenfranchised, incarcerated voters have lower neighborhood turnout in New York City, and these findings are concentrated in Black neighborhoods.

Finally, there are state laws that prevent incarcerated people, who are disproportionately Black, from voting in all states except for Maine, Vermont, and the District of Columbia. As can be seen in figure 7, many states extend the loss of voting rights past the period of incarceration (as of October 2024). In some cases, voting rights can be lost for life. For more details on the statuses of these laws, see the Restoration of Voting Rights for Felons Brief

Figure 7. Restoration of Voting Rights Following Felony Conviction, as of October 2024



Source: National Conference of State Legislatures 2024.

compiled by the National Conference of State Legislatures (2024).

Community Supervision

Despite media, politicians, and academics focusing primarily on arrest and incarceration, some scholars have begun to argue that we now live in an age of mass supervision (Miller and Stuart 2017; Phelps 2017, 2020; Schiraldi 2023). Community supervision typically refers to probation and parole, which we focus on here. Probation is often used as an alternative to incarceration but also sometimes occurs following incarceration (referred to as a split sentence) and can itself result in incarceration because of technical violations (Olson 2019). Parole, on the other hand, comes after an incarceration stint is over and can carry stringent conditions (Reitz and Rhine 2020; Travis and Stacey 2010). Because the literature on the fallout from these forms of supervision is nascent, we organize this section by delineating probation and parole separately.

Some analyses of probation relative to incarceration have found that probation results in a lower likelihood of future criminal justice system contact post-release (Aizer and Doyle 2015; Harding, Morenoff, et al. 2017). It is difficult to disentangle what drives these results, especially since parole typically follows imprisonment and has stricter conditions than probation and thus related technical violations could be driving future imprisonment (see Harding, Morenoff, et al. 2017). However, others find no difference between recidivism outcomes following probation compared to prison (Loeffler 2013; Eren and Mocan 2021), or even that probation results in worse recidivism outcomes (Hjalmarsson 2009; Rose and Shem-Tov 2021). Diverging findings could reflect the heterogenous landscape of probation programs or differing measurement and analytic strategies (see Doleac and LaForest 2022 for a review of studies that try to isolate the causal impact of community supervision on reoffending and future criminal justice system contact). Some studies try to isolate the causal effect of varying levels of supervision intensity. For instance, Jordan Hyatt and Geoffrey Barnes (2017) compare the impacts of varying intensities of probation supervision on recidivism and find that those experiencing more intense supervision do not exhibit differences in offending compared to those receiving standard probation, but they are incarcerated at significantly higher rates as a result of technical violations.

There is limited evidence on the impact of parole on recidivism and future criminal justice system contact (again, see Doleac and LaForest 2022). However, extant work implies that parole may serve as an incentive for people in prisons to complete programming and avoid disciplinary infractions (Kuziemko 2013). Data from Michigan also shed light on parole as a labor market institution. Parole does seem to increase employment; however, parolee employment does not reduce poverty or recidivism odds (Seim and Harding 2020). There are also significant racial differences in the quality of jobs that parolees obtain: White parolees are more likely than Black parolees to secure a stable job in higher-wage industries such as construction, partially owing to the location of these jobs (Rucks-Ahidiana et al. 2021). In addition, short-term incarcerations due to parole violations result in lower earnings, especially for those working in the formal labor market (Harding, Siegel et al. 2017).

Probation and parole typically are accompanied by fines and fees; most states allow supervision fees, and in some states, failure to pay can itself result in incarceration (Brett et al. 2020; Fines and Fees Justice Center and Reform Alliance 2022). Seminal work by Alexes Harris and colleagues (2010) uses both national- and state-level data to show that various financial sanctions are widespread and result in large amounts of legal debt for individuals convicted, which may be especially burdensome as well as relevant for understanding inequality, given that many of these individuals are already poor (also see Harris 2016). In the first US-based randomized controlled trial centered on fines and fees, Pager and colleagues (2022) analyzed a legal debt relief program in one county in Oklahoma. They found no impact of the debt relief program for misdemeanor defendants on ensuing criminal charges or jail-specific recidivism, though they did find that individuals who did not receive the debt relief experienced more new warrants and referrals to a private debt collector—which may keep individuals tied to the

legal system even in the absence of subsequent criminal offending.

CONCLUSION

In this review, we provide the most up-to-date and comprehensive description of the state of research on the scope and scale of criminal justice contact and its collateral consequences for racial and ethnic inequality in the United States. Our goal was to provide a side-by-side overview—at a depth and scale that has not been conducted in previous reviews—of the data sources and methods used in this area alongside empirical results stemming from these data and methodological approaches. We show that the data sources, methodologies, types of contact, and outcomes studied are wide-ranging. We argue that while this literature has grown tremendously over the past twenty-five years and great strides have been made in our understanding of the reach and impact of criminal justice contact across outcomes and populations, the details, oft-overlooked, matter. In other words, results across studies are often more mixed and nuanced by the nature of contact (imprisonment versus jail), data source (survey versus official records), geographic context, and sample composition, among other things, than is typically accounted for by high-level summaries of main findings in this area. In showing this tremendous breadth of toolkits and extant results, therefore, we hope to push future research not toward privileging any particular type of data source, methodological approach, or form of contact or consequence but toward employing more discernment and transparency in who and what are being studied in a given analysis,[27] with an eye toward how the implications of these details (within a given study) are grounded in theory and mesh with the broader literature.[28]

We would be remiss to conclude this article without briefly mentioning the impact of COVID-19 on the criminal justice system. Research consistently finds that the prevalence of COVID-19 in carceral facilities exceeded rates in the general population and that mortality rates from COVID-19 were also higher (Marquez et al. 2021, see Puglisi et al. 2023 for an extensive review on related work). While average daily jail populations and annual admissions fell during the early pandemic—representing one of the most significance instances of decarceration in recent history—by 2022 these numbers began rebounding toward pre-pandemic levels (Zeng 2024). This era of pandemic-driven changes coincided with widespread protests following George Floyd's murder by police, which increased public recognition of anti-Black racism (Reny and Newman 2021). However, subsequent White backlash to mainstream recognition of structural racism has since shaped policies and practices across a range of institutions (see, for instance, Ray and Gibbons 2021). These simultaneous crises and social movements highlight the dynamic nature of the criminal justice system, reinforcing the argument that fallout from system contact must be understood within broader frameworks that consider social change (see also Sampson 2025, this issue).

These recent developments also highlight that while our literature review is extensive, it relies on evidence drawn from quantitative data that can provide quantifiable and statistically generalizable evidence of the fallout from criminal justice contact and therefore must be understood alongside evidence drawn from other approaches. As others have argued, the causal effects estimated from this research when examined apart from evidence drawn from other research approaches "often do not provide much causal understanding" (Marini and Singer 1988, 348). Indeed, we can think about fallout in ways outside a traditional cause-effect framework as much of the research that launched this research area did (Western and Beckett 1999; Pettit and Western 2004). For instance, descriptive evidence (such as what was chronicled in earlier in this article) provides striking depictions of the ways that mass incarceration has come to permeate entire communities. In addition, a substantial body of qualitative research examines the ways in

27. As well as when (see Neil and Sampson 2021) and where a population are being studied.

28. Also see Ian Lundberg and colleagues (2021, 532). Their contention is that "much attention is already placed on how to do estimation; a similar degree of care should be given to defining the thing we are estimating."

which criminal justice contact fundamentally alters the lived experience of individuals, their loved ones, and the communities in which they are embedded (Comfort 2008; Comfort 2016; Clear 2007; Miller 2021; Phelps and Ruhland 2022; Turanovic et al. 2012; Western 2018). This descriptive and qualitative research creates deeper understanding and pushes us to consider the reality that mass incarceration has restructured the social fabric of day-to-day life for many marginalized groups and communities—which itself also presents difficulties for the traditional counterfactual thinking that undergirds conventional causal modeling.

One way to show the utility of this perspective is to consider how incarceration reshapes the lived experiences of individuals even in the face of some null or even positive effects. Quantitative research suggests, for instance, that maternal incarceration's effects on children are, on average, quite muted (Wildeman and Turney 2014). It also finds that incarceration decreases the mortality risks of young Black men through a host of mechanisms that highlight the significant risks that Black men face in their home communities (Patterson 2010). Focusing only on effects in these instances could lead to us fundamentally misunderstand the extent to which mass incarceration has indeed produced extensive fallout. Put differently, the "protective" effects of imprisonment on Black men's mortality risk should in no way, shape, or form undercut the ways in which one in four Black men going to prison by their early thirties has fundamentally transformed American society (Pettit and Western 2004). Likewise, the fact that maternal incarceration appears to have no average effect on children's mental health should not lead us to forget that Black American children are as likely to have a mother imprisoned as Danish children are to have a father imprisoned (Wildeman and Andersen 2015; Wildeman 2009). Fallout from the carceral state has occurred regardless of whether research identifies any causal effects.

Qualitative research has also been instrumental in generating theories and hypotheses about the material and symbolic consequences of criminal justice contact as well as the social processes and contexts linked to these consequences (Braman 2004; Comfort 2008). Moreover, the theoretical generalizations derived from this work have directly informed quantitative analyses of criminal justice fallout—from the formulation of research questions and research design to interpretation of results. Qualitative research has played a particularly critical role in guiding interpretation of quantitative research results such as ambiguous patterns, anomalous cases, and heterogeneity in impacts across outcomes and population groups (Grigoropoulou and Small 2022).[29] Indeed, qualitative studies, by design, enable researchers to "gather more accurate data and speak to individuals" from hard-to-reach populations (Lamont and White 2008, 12). It also allows for deep interpretation of meaning and lived experience within and across contexts. These insights help researchers to understand and situate complex results from statistical surveys impacted by sample selection, attrition, and predetermined variables (which we allude to in the methodology section). It also provides articulation and insight about the complexities in the experiences, nature, and consequences of criminal justice contact that may be obscured in quantitative work owing to the limitations of survey data and other forms of quantitative data collection. For example, qualitative research can unearth the ways in which criminal justice contact may be both good and bad for the same person or population (Comfort 2008), good for some populations but bad for other populations (Siegel 2011), or good for some persons in a family and bad for others (Turanovic et al. 2012). In these ways, qualitative research is foundational to the evidence base of this research area. Moreover, it is imperative that quantitative research continue to be and, in the case of some disciplines, begin to be, informed by the theoretical insights and innovations derived from qualitative research.

There are also important and underutilized theoretical tools from the humanities that inform empirical research on criminal justice system fallout. For example, if we use a critical race lens to understand racial and ethnic variation in the impacts of criminal justice contact,

29. Also see the section on racial heterogeneity in this article.

it becomes clear that the criminal justice system operates in conjunction with myriad other systems (healthcare, education, labor market, child welfare, and political systems, for instance) that share histories and logics of racial bias, violence, exclusion, and control (Lee 2024; Muller 2012; Roberts 2012). We see evidence of racial and ethnic inequalities in contact and outcomes in each of these systems; they are overlapping, interactive, and mutually reinforcing and operate in ways that complicate our ability to model and interpret variation on the impacts of the criminal justice contact by race or ethnicity (Muller and Roehrkasse 2022). We can use insights from critical race theory to help us to better understand and contextualize research findings. While the examples in table 1 largely focus on differences between Black and White men, these perspectives can be expanded to other racial, ethnic, and gender groups. Additional insights from critical race theory (for example, intersectionality) can also help us to understand potentially significant but largely unexplored differences at the intersection race and ethnicity, social class, and gender (as well as other dimensions of identity such as sexual orientation and immigration status) in the future.

Relatedly, our review indicates that little research in this area considers heterogeneity in effects and consequence outside race (and, sometimes, ethnicity).[30] However, existing research and theory indicate that there is likely to be considerable variation by gender, class, geography, and cohort, as well as other key demographic characteristics and their intersections. And this is to say nothing of the ways in which the nature of the criminal justice contact can shape outcomes. From an empirical lens, we also stress that our review focusing on race-specific results implies that significant heterogeneity does indeed exist and that these tests ought to be central to future research in this area. Thus, research in this area would do well to highlight the who, what, when, where, and why of how the fallout from criminal justice contact occurs.

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