WILL DOBBIE
Harvard University

CRYSTAL YANG
Harvard University

The Economic Costs of Pretrial Detention

ABSTRACT We measure the economic costs of the US pretrial system using several complementary approaches and data sources. The pretrial system operates as one of the earliest points of entry in the criminal justice system. It typically represents an individual's first opportunity to be incarcerated, potentially leading to subsequent long-term damage in the form of family separation, work interruption, loss of housing, and so on. We find that individuals lose almost $30,000 in forgone earnings and social benefits when detained in jail while awaiting the resolution of their criminal cases. These adverse consequences are also present in aggregate measures of economic well-being, with increases in county pretrial detention rates associated with increases in poverty rates and decreases in employment rates. Counties with high levels of pretrial detention also exhibit significantly lower levels of intergenerational mobility among children, consistent with pretrial detention having an adverse impact on young children who may be the dependents of individuals affected by the pretrial system.

The US criminal justice system has experienced a more than threefold expansion in the past several decades, with the number of inmates in local jails, state prisons, federal prisons, and privately operated facilities rising from 220 per 100,000 US residents in 1980 to 756 per 100,000 US residents in 2008. In addition, the high and growing incarceration rate in the United States has had a disproportionate impact on economically disadvantaged and minority communities, with significantly higher arrest, conviction, and incarceration probabilities for Black and Hispanic individuals compared to observably similar white individuals (Abrams, Bertrand, and

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Mullainathan 2012; Anwar, Bayer, and Hjalmarsson 2012; Rehavi and Starr 2014; McConnell and Rasul 2018; Raphael and Rozo 2019). By some estimates, more than 8 percent of the adult population and 33 percent of the Black adult male population in the United States has a prior felony conviction (Shannon and others 2017).

The economic consequences of mass incarceration may be substantial. Criminal records can result in substantial barriers to employment, particularly for minority individuals (Pager 2003; Holzer, Raphael, and Stoll 2006, 2007; Agan and Starr 2018), and recent work has shown that the increase in nonwork among US men, and Black men in particular, is partially attributable to the rising incarceration rate (Western 2002; Moffitt 2012; Neal and Rick 2014; Bayer and Charles 2018). These findings are particularly concerning given the persistently lower employment rates for Black and Hispanic individuals compared to non-Hispanic whites, with some of these gaps increasing in the wake of economic recessions and the recent COVID-19 pandemic.

In this paper, we measure the economic costs of the US pretrial system—an important but often neglected feature of the US criminal justice system that affects more than 10 million individuals each year who are arrested for an offense in the United States. To put this annual number into perspective, consider that approximately 8.1 million jobs were lost during the Great Recession between December 2007 and November 2009 (Shierholz 2009). The pretrial system operates as one of the earliest points of entry in the criminal justice system and typically represents an individual's first opportunity to be incarcerated. In theory, the pretrial system is meant to ensure the equitable release of most individuals before trial while minimizing the risk of flight or danger to the public. Despite this, defendants detained before trial represent over 75 percent of all jail inmates in some parts of the country. The pretrial system has also faced increased public scrutiny in recent years due to the all too common stories of arrested individuals who, despite being first-time offenders accused of low-level crimes, spend months in pretrial detention and face subsequent long-term damage in the form of family separation, work interruption, loss of housing, or even death. Pretrial detention can also generate substantial spillover effects, as the costs of paying money bail and other related court fees and fines often fall on other family and community members of detained individuals. Many of these harms can result even when the period of incarceration is brief and individuals are not ultimately convicted of any crime.

We measure the economic costs of the US pretrial system using several complementary approaches and data sources. We begin by describing

causal evidence about the effects of pretrial detention on individual economic outcomes such as formal labor market attachment and the receipt of unemployment insurance (UI) and the earned income tax credit (EITC), drawing on estimates from Dobbie, Goldin, and Yang (2018). Using quasi-experimental estimates based on the random assignment of cases to bail judges, these estimates capture the direct effects of detention on "marginal" defendants, or those for whom judges disagree on the appropriateness of pretrial detention. These quasi-experimental estimates show that individuals are nearly 10 percentage points less likely to be employed in the formal labor market if detained before trial. Detained individuals are also significantly less likely to receive EITC payments and receive substantially smaller UI and EITC amounts. Taken together, the estimates from Dobbie, Goldin, and Yang (2018) imply that individuals lose an average of $29,000 over the course of the working-age life cycle when detained in jail for just three days while awaiting the resolution of their criminal cases. These individual-level estimates further suggest that reforms such as the elimination of money bail could potentially increase aggregate earnings by as much as $80.91 billion per year, although we caution that the underlying quasi-experimental estimates are measured with considerable noise.

Our second contribution is to show that the adverse consequences of pretrial detention remain present in aggregate measures of economic well-being that also include potential spillover effects on other individuals in the household or community at large. One potential limitation of the quasi-experimental estimates from Dobbie, Goldin, and Yang (2018) is that they estimate direct effects on marginally detained defendants, whether through a short-run incapacitation or long-run job destabilizing effect. But pretrial detention is likely to generate spillover effects on other individuals in both the short and long run given its potential impact on families and communities. These spillover effects are all the more likely given that more than half of individuals detained pretrial are parents of children under the age of 18.[1] In the absence of convincing quasi-experimental variation in aggregate pretrial detention rates, we estimate the aggregate effects of pretrial detention inclusive of these spillover effects by comparing changes in county-level poverty and employment-to-population rates to changes in county-level pretrial detention rates. While the analysis is exploratory in nature, we find that a 10 percentage point increase in county pretrial

1. Based on data from the Survey of Inmates in Local Jails, at Prison Policy Initiative, "How Does Unaffordable Money Bail Affect Families?," https://www.prisonpolicy.org/blog/2018/08/15/pretrial/.

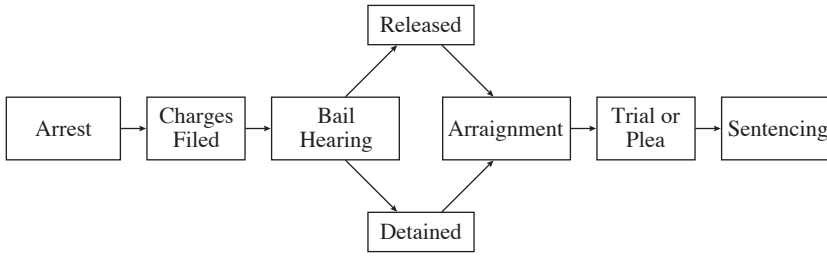
detention rates between 2000 and 2009 is associated with a 1.41 percentage point increase in county poverty rates and a 2.06 percentage point decrease in county employment rates between 2000 and 2010. The general relationship between changes in pretrial detention rates and poverty and employment rates is similar when we add additional controls and is typically stronger for economic outcomes of demographic groups most likely to be affected by pretrial detention, such as working-age Black individuals.

The final contribution of our paper is to explore more tentatively the intergenerational spillover effects of pretrial detention on young children. Leveraging measures of intergenerational mobility obtained from Chetty and others (2018), we find that counties with high levels of pretrial detention when children are young (age 7–12) exhibit significantly lower levels of intergenerational mobility for children when they are in adulthood. We find, for example, that a 10 percentage point higher pretrial detention rate in 1990 is associated with a 0.66 lower predicted income percentile for children born to parents at the 25th percentile for income over twenty years later. These findings are consistent with pretrial detention having an adverse impact on young children who may be the dependents of individuals affected by the pretrial system, although we caution that we are unable to control for many potential differences between low- and high-detention areas.

Our complementary pieces of evidence suggest that reducing the scope of the pretrial system, such as through the elimination of money bail, is likely to generate significant economic returns for both directly affected individuals and the communities they live in. Related reforms such as cite and release policies in lieu of arrests are also promising ways to limit the number of individuals at risk of pretrial detention. In contrast, reforms that limit the ability of employers to ask about criminal records, such as ban-the-box policies, may come too late to mitigate the economic harms associated with detention given the likely cumulative and scarring effects of pretrial detention. These later-stage reforms may also yield unintentional consequences for minority individuals (Doleac and Hansen 2020; Agan and Starr 2018).

The remainder of the paper proceeds as follows. In section I, we provide a brief background of the pretrial system, who it affects, and why it may have an impact on both individual and aggregate economic outcomes. Section II provides some descriptive statistics on pretrial detention and economic outcomes. Section III describes our evidence on the effects of pretrial detention on detained individuals at both the individual and aggregate levels. Section IV concludes and discusses areas of future work.

Figure 1. Criminal Justice Process from Arrest to Sentencing



Source: Authors.

I. A Brief Overview of the Pretrial System

The pretrial system serves as one of the first points of entry into the US criminal justice system. Figure 1 presents a simplified diagram of the various key interactions that can occur within the criminal justice system. In most jurisdictions, individuals appear for their first court appearance approximately 24–48 hours after arrest and booking by law enforcement. Figure 1 highlights that individuals can be detained in the pretrial system without eventually being either convicted of a crime or incarcerated post-conviction. In Philadelphia and Miami, for example, over 40 percent of detained individuals are not convicted of a crime and nearly 70 percent of detained individuals serve no additional incarceration (Dobbie, Goldin, and Yang 2018).

I.A. The Purpose of the Pretrial System

The US pretrial system is meant to allow all but the most dangerous criminal suspects to be released from custody while they await trial. Under the Eighth Amendment of the US Constitution, "excessive bail shall not be required, nor excessive fines imposed, nor cruel and unusual punishments inflicted." Applied to the pretrial system, the importance of release is grounded in the presumption of innocence, an axiomatic and elementary right designed to protect defendants before any finding of guilt. The pretrial system reflects the notion that pretrial detention should be used only in limited circumstances and is not deemed appropriate simply because a defendant may be guilty of the alleged crime.

The main objective of the pretrial system is to guarantee appearance at court. The federal system, along with at least forty states, also considers public and community safety explicitly as part of the release or detention

decision (Baughman and McIntyre 2012). Today, these competing objectives are embodied in the standards of the American Bar Association, which state that the judicial decision of whether to release or detain a defendant requires judges to "strike an appropriate balance" between the competing societal interests of individual liberty, court appearance, and public safety (American Bar Association 2007, 29–30).

In most jurisdictions, bail judges are granted substantial discretion when making decisions about pretrial release. These bail judges generally may consider factors such as the nature of the alleged offense, the weight of the evidence against the defendant, any record of prior flight or bail violations, and the financial ability of the defendant to pay bail. Today, with the rise in pretrial risk assessment instruments like the Arnold Ventures Public Safety Assessment (PSA), judges may also rely on a pretrial recommendation based on a risk score.

Bail judges have several options in setting conditions for pretrial release after assessing an individual's flight or public safety risk. For defendants who pose a minimal risk of flight or danger, the judge may simply release the defendant—known as release on recognizance (ROR) or personal recognizance—in which the defendant promises to return for all court proceedings and abide by all other release conditions. Defendants may also be released subject to some form of nonmonetary conditions, sometimes known as conditional release, when a judge determines that certain conditions are necessary to prevent flight or harm to the public. These conditions can include regular reporting to a pretrial services officer, drug treatment or testing, no-victim-contact orders, and even more intensive measures such as electronic monitoring or home confinement.

A judge may also impose monetary bail (cash bail or bond), which generally requires defendants to post either the full bail amount or some fraction of the bail amount to secure release. Since the twentieth century, the primary means of ensuring pretrial compliance in the United States has been the use of monetary bail as defendants are able to largely recoup their payments if they comply with all release conditions. In many jurisdictions, those who do not have the required deposit in cash can borrow this amount from commercial bail bondsmen or sureties. These agents will often accept cars, houses, jewelry, or other forms of collateral and generally charge a nonrefundable fee of 10 to 20 percent of the bail amount for their services. Another common type of monetary bail is an unsecured bond, which requires the defendant to promise to pay a certain amount of money if they do not return to court but does not require an upfront payment to secure release. If the defendant fails to appear or commits a new

crime (broadly known as pretrial misconduct), either the defendant or the bail bondsman is theoretically liable for the full value of the bail amount and forfeits any amount already paid. The amount of monetary bail may be unilaterally determined by the judge or prespecified in a bail schedule, which determines bail amounts for each type of offense or grade of offense. For example, a bail schedule might specify that a level 1 felony is associated with a $50,000 bail amount. Bail schedules are regularly used in some parts of the country, such as California and Texas, although a judge often has the discretion to reduce (or increase) the amount.

Finally, for the most serious crimes, the bail judge may require that the defendant be detained pending trial by denying bail altogether. In many jurisdictions, denial of bail is often mandatory in first- or second-degree murder cases. However, it can also be imposed for other crimes, such as domestic violence, when the bail judge finds that no set of conditions for release will guarantee appearance or protect the community from the threat of harm posed by the defendant.

In many parts of the country, determinations of bail and conditions of release are decided in short hearings that last anywhere from ten seconds to a few minutes. Defendants are often videoconferenced in from the jail, and a judge reviews the case and criminal history of the defendant, sometimes asking the defendant a few questions, before imposing conditions for pretrial release. If present, a prosecutor or defense attorney may also present their recommendations for bail.

I.B. Who Is Affected by Pretrial Detention

Decisions made at the pretrial stage affect more than 10 million individuals each year who are arrested for an offense in the United States. In some parts of the country, defendants detained before trial represent over 75 percent of all jail inmates.[2] A large contributor to the high rate of pretrial detention in the United States is the increasing use of monetary (or cash) bail, and the corresponding decreasing use of ROR over the past several decades. For example, between 1990 and 2009, in seventy-five of the most populous US counties, the share of defendants assigned monetary bail exceeded 40 percent in 2009, an 11 percentage point increase from 1990 (Reaves 2013). Over this same time period and sample, the fraction of defendants released on their own recognizance decreased by about

2. Pretrial detainees are housed in local jails. Other jail inmates include individuals serving relatively short post-conviction sentences. Prison inmates are individuals serving longer post-conviction sentences.

13 percentage points, with only 14 percent of defendants being released on their own recognizance in 2009 (Reaves 2013).

Today, the widespread use of monetary bail in many jurisdictions has resulted in high pretrial detention rates. In 2009, among felony defendants assigned monetary bail in the seventy-five largest counties, 46.9 percent were detained for the entirety of their case. Detention rates are high even when defendants are assigned relatively small monetary bail amounts. In New York City, for example, an estimated 46 percent of all misdemeanor defendants and 30 percent of all felony defendants were detained before trial in 2013 because they were unable or unwilling to post bail set at $500 or less (New York City Criminal Justice Agency 2014). Most available evidence suggests that defendants often have low earnings and rates of employment, suggesting that detention for even relatively small amounts may be due to inability to pay bail, either directly or through a bail bondsman. For example, among individuals detained in Philadelphia (for 2007–2014) and Miami (for 2006–2014), only 32 percent were employed in the year prior to arrest, only 77.2 percent had any income, and the average annual income was $4,524 (Dobbie, Goldin, and Yang 2018).

The high rate of pretrial detention and its disparate prevalence across demographic groups has contributed to concerns regarding the effectiveness of the current bail system. Critics of the current system argue that pretrial detention generates substantial costs to detainees that far outweigh the benefits to society (Pinto 2015). Critics argue, for example, that pretrial detention increases the risk of wrongful conviction by pressuring defendants to accept plea bargains to get out of jail. Pretrial detention and excessive bail conditions may also generate collateral consequences outside of the criminal justice system by disrupting defendants' lives, putting jobs, housing, and child custody at risk. These critics also argue that many jurisdictions set bail without adequate consideration for the defendant's ability to pay. As a result, they argue that pretrial detention is determined by a defendant's wealth, not their risk to the community, which reduces the current system's effectiveness and simultaneously exacerbates socioeconomic disparities. These concerns led the Department of Justice to conclude that the pretrial systems in many jurisdictions "are not only unconstitutional, but . . . also constitute bad public policy" (US Department of Justice 2016, 13).

A second concern of the current system deals with the presence of large and persistent disparities in the treatment of seemingly similar defendants, in particular by race and ethnicity. There are significant disparities in bail conditions and pretrial detention among defendants who are similar across

legally relevant dimensions in most large US counties, contributing to the overrepresentation of certain demographic groups in the pretrial system. Controlling for observable and legally relevant charge and defendant characteristics, nationally representative data on felony defendants in state courts show that, on average, Black and Hispanic defendants are substantially more likely to be detained before trial compared to observably similar white defendants (Demuth 2003; McIntyre and Baughman 2013). Disparate rates of pretrial detention are likely due to the fact that Black and Hispanic defendants are generally more likely to be assigned monetary bail and higher monetary bail amounts, compared to observably similar white defendants (Demuth 2003; Demuth and Steffensmeier 2004; Schlesinger 2005; Arnold, Dobbie, and Yang 2018). There is also substantial heterogeneity in the size of the racial gap in detention rates even after accounting for the relevant case and defendant characteristics. Harris County in Texas, for example, is 34 percent more likely to detain Black defendants compared to white defendants with the same observable characteristics, while Baltimore County in Maryland is 1 percent less likely to detain Black defendants compared to white defendants (Dobbie and Yang 2019). In recent quasi-experimental work that exploits the release tendencies of quasi-randomly assigned bail judges, researchers have found that these racial disparities can be attributed to substantial statistical discrimination and forms of racial bias (Arnold, Dobbie, and Yang 2018; Arnold, Dobbie, and Hull 2020).

Based in part on the above concerns, the pretrial systems in many jurisdictions are in flux as there is significant public support for reforming the pretrial system in the United States. In recent years, cities across the country have implemented widely supported changes to their pretrial systems. Sometimes these changes arise due to lawsuits challenging the constitutionality of money bail and bail schedules. For example, in April 2017, a federal judge in Houston issued a preliminary injunction on the current misdemeanor bail system in Harris County, Texas. Similar lawsuits are under way in many other large cities across the country. Several jurisdictions have voluntarily explored alternatives to pretrial detention, such as electronic or in-person monitoring for low-risk defendants. New York, for example, has earmarked substantial funds to supervise low-risk defendants instead of requiring them to post bail or face pretrial detention. Risk assessment instruments, such as the Arnold Ventures PSA, have been adopted by more than thirty-nine jurisdictions across the country, based on the promise of being able to more accurately predict offender risk of danger or flight. Other jurisdictions, such as New Mexico and New Jersey, have enacted large-scale reforms to their systems, effectively eliminating cash bail. In

addition, a wave of community-based efforts to change the current pretrial system has also swept the country to counteract the effects of cash bail, with charitable bail organizations like the Bail Project posting bail on behalf of eligible individuals.

I.C. Why the Pretrial System May Affect Economic Outcomes

There are two main channels through which the pretrial system can affect economic outcomes at the individual level. The first is through the direct effect of pretrial detention on the individuals who are actually detained. Even a short period of pretrial detention can be destabilizing for detained individuals, resulting in immediate job loss and affecting the extensive margin of employment, which can subsequently affect take-up of government benefits tied to formal sector employment (Dobbie, Goldin, and Yang 2018). Pretrial detention can also have a longer-run destabilizing effect on detained individuals even after the period of incapacitation ends through, for example, the stigma of a criminal conviction and lower future employment prospects, which can affect both labor supply and labor demand (Pager 2003; Agan and Starr 2018).

The second way that pretrial detention can have an impact on the economic outcomes of individuals is through spillovers on other individuals in the household or community at large. Anecdotal evidence suggests that the burden of additional caretaking responsibilities, as well as financial responsibilities that accompany monetary bail, often falls on the family members and friends of detained individuals. The spillover effects of pretrial detention may also extend across generations, as the majority of detained individuals are parents to children under the age of 18, some of whom may be placed into child custody as a result of pretrial detention. Estimates of just the direct effect of pretrial detention on detained defendants may therefore understate the effect of pretrial detention on individuals more generally.

The negative effects of the pretrial system on individual-level outcomes (both direct and spillover) can also translate into worse aggregate macroeconomic outcomes, such as employment rates. Pretrial detention removes detained individuals from the labor market during the period of detention through a short-run incapacitation effect. In a standard neoclassical model, this short-run reduction in labor supply will lower aggregate employment unless labor demand is perfectly inelastic. In practice, however, our measure of employment—the employment-to-population ratio—may be unaffected by this short-run effect given that detained individuals are removed from both the numerator (employed individuals) and denominator (noninstitutionalized civilian population). The evidence from section III.A

also suggests that the negative effects of pretrial detention on individual economic outcomes are not driven by these short-run incapacitation effects, so we do not primarily focus on this channel in our work.

Pretrial detention can also have an adverse impact on aggregate macroeconomic outcomes through the sustained and cumulative scarring effect of pretrial detention on individuals, affecting decisions to invest in human capital which in turn affect job productivity. Smith (2021) documents stories of individuals who made decisions that could have an impact on human capital accumulation following short stints in pretrial detention, such as dropping classes that were needed to receive certification for certain jobs or taking on odd jobs in between school and formal employment to pay off court-related debt in the form of fines and fees. Smith also documents stories of frequent assault and trauma during the period of detention, resulting in difficulties assimilating back to school and work after release. There is also substantial evidence from both qualitative and quantitative studies that pretrial detention causally increases future criminal legal involvement (Smith 2021; Heaton, Mayson, and Stevenson 2017; Dobbie, Goldin, and Yang 2018), with Smith (2021) and Smith and Broege (2020) arguing that pretrial detention puts people on a fundamentally different criminal justice trajectory, further leading to skill depreciation, reduced human capital investment, and reduced incentives to search for work. These sustained and cumulative scarring effects can thus reduce the labor supply of detained individuals and other individuals in the household or community at large, generating reductions in aggregate employment based on the share of affected individuals relative to the overall population. The scarring effects that lead to skill depreciation or reduced investment can also lower the labor demand for these individuals, again generating reductions in aggregate employment, particularly if there are frictions like search costs.

The goal of this paper is to produce micro-level estimates of the direct effect of pretrial detention on individuals who are actually detained, as well as macro-level aggregate estimates that combine the direct and spillover effects of pretrial detention through the channels discussed in this section.

II. National Trends in Pretrial Detention and Economic Outcomes in the United States

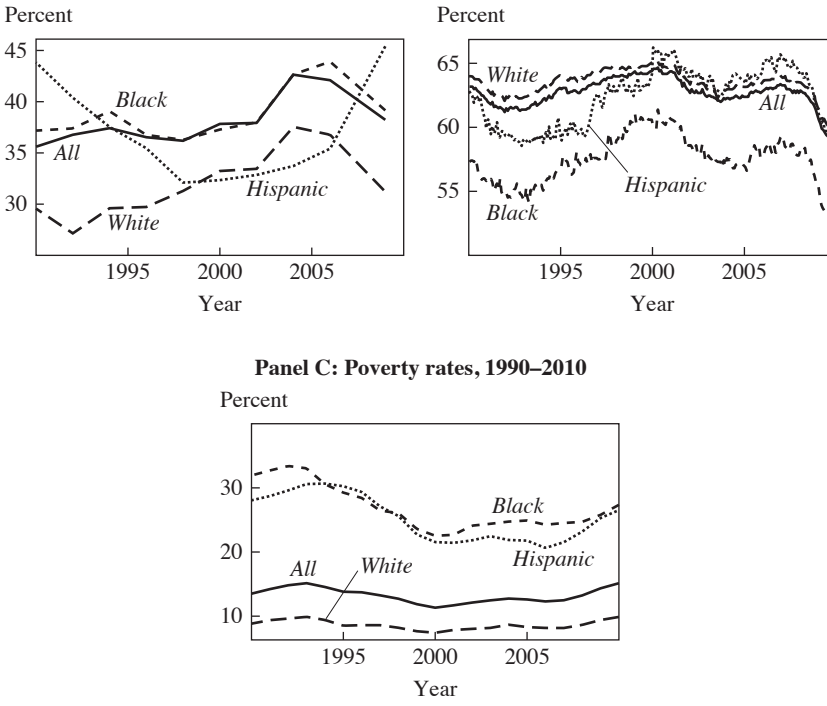
We begin by documenting the characteristics and trends of felony defendants entering the US pretrial system from 1990 to 2009 in the seventy-five largest US counties. Our data come from the Bureau of Justice Statistics' State Court Processing Statistics (SCPS), which are designed

as a nationally representative sample of seventy-five large urban counties and include information on over 140,000 individuals arrested for felony offenses. These seventy-five counties account for more than a third of the US population and approximately half of all reported crimes. The data track what happens to each defendant from the time of arrest to case disposition, providing detailed information on the arrest offense, defendant demographics and criminal history, and, importantly for our analysis, whether or not the individual was released or detained pretrial. The data include approximately forty of the nation's seventy-five most populous counties in even numbered years from 1990 to 2006 and 2009, as well as weights that allow researchers to calculate statistics that are representative of the full set of seventy-five populous counties.

Online appendix table A1 presents descriptive statistics on these felony defendants in the seventy-five most populous counties. Column 1 presents descriptive statistics for the full sample of defendants. The data reveal that 23 percent of felony defendants are non-Hispanic white, 8.1 percent are Hispanic white, and 46.9 percent are Black. Most felony defendants in the data are male (83.2 percent) and relatively young, with 35.8 percent age 24 and under and almost 90 percent age 44 and under. The most common lead charges are drug offenses (34.9 percent), followed by property offenses (30.8 percent), violent offenses (24.8 percent), and public order offenses (9.4 percent). There is an average of 4.6 prior arrests, 2.9 prior felony arrests, 2.6 prior convictions, and 1.2 prior felony convictions in our sample. In addition, there is an average of 0.5 past failures to appear for court appearances.

Columns 2 and 3 present corresponding descriptive statistics for felony defendants who are released and detained, respectively. In the SCPS, defendants are defined as detained if they remained in jail for the entirety of the time from arrest until case disposition. These individuals can be detained either because of denial of any bail (as can be the case for capital offenses or domestic violence) or because of inability to pay the assigned monetary (or cash) bail. Release and detention decisions are far from random. Non-Hispanic white defendants comprise a larger share of released defendants (25.2 percent) compared to their share among detained defendants (19.6 percent), while Black defendants comprise a lower share of released defendants (46.4 percent) compared to detained defendants (47.5 percent). We also see an overrepresentation of female defendants and defendants under age 24 among released defendants, as well as defendants with less substantial prior criminal histories (either in terms of arrests, convictions, or past failures to appear). For example, released defendants

Figure 2. Trends in Detention and Employment Rates



Sources: State Court Processing Statistics (SCPS); Bureau of Labor Statistics; and the Census Bureau.

Note: Panel A uses county-year level SCPS weights to report the share of arrested felony defendants detained pretrial in a representative sample of the nation's seventy-five most populous counties from 1990 to 2009. In the SCPS data, we define "white" as non-Hispanic white and "Hispanic" as Hispanic white. Race and ethnicity trends in the SCPS are presented only for defendants who are not missing race and ethnicity information. Panel B reports the employment-to-population ratio from 1990 to 2010 for the entire country, using data from the Bureau of Labor Statistics' Current Population Survey (Household Survey). The BLS data define "white" as all individuals identifying as white and "Hispanic" as all individuals identifying as Hispanic. Panel C reports the poverty rate from 1990 to 2010 for the entire country, using data from the Census Bureau's Current Population Survey (Annual Social and Economic Supplement). The Census Bureau data define "white" as non-Hispanic white and "Hispanic" as all individuals identifying as Hispanic.

have 3.8 prior arrests on average, while detained defendants have 5.9 prior arrests on average.

Panel A of figure 2 examines how detention rates have evolved from 1990 to 2009 among felony defendants in the seventy-five most populous counties, where we again define defendants as detained if they remained in jail for the entirety of the time from arrest until case disposition. The detention rate for felony defendants increased from 35.6 percent in 1990

to 42.1 percent in 2006, before falling slightly to 38.2 percent in 2009. As described in past work, the general upward trend in detention rates is due to the increasing use of monetary bail and corresponding decreasing use of ROR during the time period (Reaves 2013). The data also show that the detention rate for Black defendants was higher than the detention rate for non-Hispanic white defendants for the entire sample period. In 1990, for example, 37.2 percent of Black defendants were detained compared to 29.6 percent of non-Hispanic white defendants. In 2009, 39.2 percent of Black defendants were detained compared to 31.2 percent of non-Hispanic white defendants.

To motivate our later analysis, Panel B of figure 2 presents trends in the employment-to-population ratio for individuals by race or ethnicity during about the same time period of 1990 to 2010. These monthly data on employment rates are obtained from the Bureau of Labor Statistics. We view the employment-to-population ratio as the best comprehensive measure of labor market prospects, particularly for Black individuals given the rise in nonwork (Bayer and Charles 2018; Rodgers 2019).[3] The employment-to-population ratio increased from 62.8 percent in 1990 to 64.4 percent in 2000, before declining to 59.3 percent in 2009 during the Great Recession. Notably, there is a substantial racial gap in the employment-to-population ratio throughout the entire sample period, with a lower employment-to-population ratio among Black individuals compared to both white and Hispanic individuals. In 1990, for example, the average employment-to-population ratio was 56.7 percent for Black individuals and 63.7 percent for white individuals. In 2009, the average employment-to-population ratio was 53.2 percent for Black individuals compared to 60.2 percent for white individuals. These racial gaps are also present among other metrics of employment, such as the labor force participation rate and unemployment rate (Neal and Rick 2014; Bayer and Charles 2018).

Panel C of figure 2 similarly presents trends in poverty rates for individuals by race or ethnicity during the same time period. These data are obtained from the Census Bureau's Current Population Survey, Annual Social and Economic Supplement. Panel C shows that aggregate poverty

3. The employment-to-population ratio is calculated as the number of employed people divided by the civilian noninstitutionalized population. The civilian noninstitutionalized population does not include people confined to prisons or jails, and as such does not include those who are in pretrial detention in jails at the time of the survey. However, given the relatively short-term nature of pretrial detention for most defendants, we would expect many individuals who have experienced pretrial detention to be counted in the numerator and denominator of the employment-to-population ratio.

rates remained relatively consistent over the two decades, decreasing slightly from the mid-1990s to 2000 and increasing slightly thereafter. However, this aggregate trend masks substantial variation by race or ethnicity. Poverty rates for Black and Hispanic individuals declined sharply from 1990 to 2000. For example, the average poverty rate for Black individuals was 31.9 percent in 1990 and 25.8 percent in 2009. After 2000, the poverty rates for these two groups increased slightly, matching the aggregate trend. There is a clear racial gap throughout the entire period, with white poverty rates at least 12.5 percent lower than Black and Hispanic poverty rates at every point in time.

Taken together, figure 2 reveals substantial racial gaps in pretrial detention, employment-to-population ratios, and poverty rates. We also see similar broad trends in key time periods such as 2000 to 2006 (before the onset of the Great Recession), where pretrial detention rates rose sharply while employment-to-population ratios declined and poverty rates increased. In addition, Black-white gaps in pretrial detention, the employment-to-population ratio, and poverty rates all widened between 2000 and 2006. The common trends in pretrial detention and economic well-being, both overall and by race or ethnicity, raise the question of whether there is a causal relationship between pretrial detention and economic outcomes. We now turn to this question using a variety of complementary data sources and approaches.

III. The Economic Consequences of Pretrial Detention

In this section, we describe our results showing that there are real and substantial economic costs to pretrial detention, both at the individual and aggregate level. We begin by describing quasi-experimental estimates from recent work that measure the direct effects of pretrial detention on detained individuals' outcomes. We then examine the relationship between pretrial detention and aggregate macroeconomic measures of economic well-being, which also include potential spillover effects on other individuals in the household or community at large. Finally, we more tentatively explore the relationship between pretrial detention and intergenerational mobility among children.

III.A. Pretrial Detention and Individual-Level Economic Outcomes

Causal evidence on pretrial detention and the individual-level labor market comes primarily from Dobbie, Goldin, and Yang (2018), who leverage large-scale administrative data on criminal defendants to estimate

the impact of pretrial detention on a range of individual-level outcomes that capture many important costs of pretrial detention. The authors exploit plausibly exogenous variation in pretrial decisions from the quasi-random assignment of cases to bail judges who vary in their detention or release propensities. The so-called judge-IV empirical design utilized in this paper recovers the direct effect of pretrial detention for individuals at the margin of detention, meaning cases in which bail judges disagree on the appropriate bail conditions.[4]

To measure economic outcomes, Dobbie, Goldin, and Yang (2018) link data on criminal defendants to administrative tax records from the Internal Revenue Service (IRS) to examine the effects of pretrial detention on post-trial economic outcomes such as formal sector employment, individual and household income, and the take-up of government benefits. The IRS data include every individual who has ever acquired a social security number, including those who are institutionalized. Information on formal sector earnings and employment comes either from annual W-2s issued by employers or from tax returns filed by individual taxpayers. Individuals with no W-2s or self-reported income in any particular year are assumed to have had no earnings in that year. The IRS data also include information on UI from information returns filed with the IRS by state UI agencies and information on the EITC claimed by taxpayers on their return. For additional details on the IRS data and how the authors measure formal sector employment, individual earnings, and total household earnings, see Dobbie, Goldin, and Yang (2018). The authors were able to link over 300,000 criminal defendants (both misdemeanor and felony) arrested in Miami and Philadelphia from 2007 to 2014 to administrative IRS data.

The authors find that baseline earnings and formal labor market attachment are very low among arrested individuals in Miami and Philadelphia. Among defendants who are detained for at least three days pretrial, only 32 percent are employed in the year prior to arrest, 77.2 percent have any income, and the average annual income is $4,524. Among defendants released within three days, 42.3 percent are employed in the year prior to

4. The judge-IV design requires an assumption of first-stage monotonicity (Imbens and Angrist 1994; Heckman and Vytlacil 2005), which imposes a strong restriction on how judges choose which defendants to release before trial. This first-stage monotonicity assumption has received recent scrutiny both in general (Mogstad, Torgovitsky, and Walters 2019) and in the specific context of judge-IV designs (Mueller-Smith 2015; Frandsen, Lefgren, and Leslie 2019; Norris 2019; Arnold, Dobbie, and Hull 2020). Recent work argues that the monotonicity assumption is unlikely to hold exactly in judge-IV designs but that these estimates can still identify a convex combination of treatment effects under a weaker assumption of average monotonicity (Frandsen, Lefgren, and Leslie 2019).

Table 1. Pretrial Detention and Individual Outcomes

	Detained mean (1)	*2SLS estimates (2)*	*NPV estimates (3)*
Panel A: Binary outcomes			
Any formal sector earnings	0.378	−0.094	—
	(0.485)	(0.057)	
Any unemployment insurance	0.064	−0.013	—
	(0.246)	(0.033)	
Any earned income tax credit	0.233	−0.105	—
	(0.423)	(0.049)	
Panel B: Outcomes in dollars			
Formal sector earnings	5,887	−948	−18,961
	(15,897)	(1,128)	
Unemployment insurance	245	−293	−5,860
	(1,335)	(193)	
Earned income tax credit	357	−209	−4,180
	(998)	(127)	
Observations	144,290	334,943	—

Source: Dobbie, Goldin, and Yang (2018).

Note: Column 1 reports the mean outcome for detained defendants in the sample. Column 2 reports two-stage least squares (2SLS) estimates that instrument for pretrial detention using a judge leniency measure that is estimated using data from other cases assigned to a bail judge in the same year, controlling for court-by-time fixed effects and baseline controls. Column 3 reports the net present value of each change at a 3 percent discount rate, under the assumption that the percentage point gain for each outcome remains constant over the working life cycle. The standard deviations of each outcome for detained defendants are reported in parentheses in column 1 and robust standard errors two-way clustered at the individual and judge level are reported in parentheses in column 2. All outcomes are measured three to four years after an individual's arrest using administrative tax data.

arrest, 81.4 percent have any income, and the average annual income is $7,223. These descriptive statistics indicate that arrested individuals are likely to be different from the general working-age population.[5]

Moving to two-stage least squares estimates, Dobbie, Goldin, and Yang (2018) find that pretrial detention causally decreases both attachment to the formal labor market and the receipt of employment- and tax-related government benefits in their sample of Miami and Philadelphia cases. Table 1 summarizes the main results based on these linked data from Dobbie, Goldin, and Yang (2018). As reported in panel A of table 1, pretrial detention decreases the probability of employment in the formal labor market three to four years after the bail hearing by 9.4 percentage points in this sample, a 24.9 percent decrease from the mean. The authors find evidence that pretrial detention primarily affects earnings at the extreme low end of the income distribution, with little discernible effects at other points

5. Grogger (1995) finds similar patterns of substantial joblessness in a sample of adult individuals in California arrested between 1973 and early 1987 matched to UI records.

of the distribution. In terms of UI receipt and EITC receipt—measures of formal sector engagement that are particularly welfare-relevant in our low-income population—pretrial detention also decreases the probability that the defendant takes up any UI benefits within three to four years after case disposition by 1.3 percentage points, a 20.3 percent decrease, and decreases the take-up of EITC benefits by 10.5 percentage points over the same time period, a 45.1 percent decrease.

Panel B of table 1 presents results on outcomes in dollars. Pretrial detention reduces formal sector earnings by $948 per year over the same time period, a 16.1 percent decrease from the mean. In terms of UI receipt and EITC receipt, the authors find that three to four years after arrest, pretrial detention decreases UI benefits by $293 and EITC benefits by $209 per year, 119.6 and 58.5 percent decreases from the mean, respectively. All of the estimated effects are larger among individuals with no prior offenses in the past year. These results are consistent with the stigma of a criminal conviction lowering defendants' prospects in the formal labor market (Pager 2003; Agan and Starr 2018) and reduced labor supply and human capital accumulation (Smith 2021; Smith and Broege 2020), which in turn limits defendants' eligibility for employment-related benefits like UI and EITC.

The findings from the recent quasi-experimental empirical literature thus suggest that pretrial detention imposes substantial economic costs to individual defendants at the margin, reducing formal sector employment at the extensive margin. The findings from Dobbie, Goldin, and Yang (2018) also suggest that the long-run effects on employment are unlikely to be primarily driven by a short-run incapacitation effect (e.g., losing one's job while being detained pretrial), given that employment is lower long after the period of pretrial detention. The longer-run effect of pretrial detention on formal sector employment is likely driven by lower labor supply by detained individuals or lower labor market demand for individuals with criminal records, as discussed above.

Applying the two-stage least squares estimates from Dobbie, Goldin, and Yang (2018), we can use back-of-the-envelope calculations to explore the amount that each marginally detained defendant loses in income over the course of the working life cycle (see column 3 of table 1). Recall that the marginal detained defendant earns roughly $948 less per year and has $293 less in UI income and $209 less in EITC income, for a total average annual income loss of $1,450, 10.1 percent of mean earnings in the sample. Following Chetty, Friedman, and Rockoff (2014), we assume that the percentage loss in earnings remains constant over the working life cycle and discount annual earnings at a 3 percent discount rate back to age 34,

the mean age in the sample. Under these assumptions, the marginal detained defendant loses $29,000 in income over a lifetime relative to the marginal released defendant, with almost $19,000 of the lost income due to reduced formal sector earnings—confirming the substantial economic costs to detained defendants at the margin suggested in the recent literature.

POLICY IMPLICATIONS We can use the quasi-experimental estimates from Dobbie, Goldin, and Yang (2018) to evaluate two types of policy reforms. The first policy we consider is shifting the rates in high-detention counties such as Harris County, Texas, with a detention rate of 65 percent, to match those in low-detention counties such as Broward County, Florida, with a detention rate of 22.2 percent. The second policy we consider is the elimination of cash bail, which the available evidence suggests would lower the pretrial detention rate to anywhere from 3 to 10 percent.[6] We note that these simulations can only yield approximate estimates given the imprecision of some quasi-experimental estimates from Dobbie, Goldin, and Yang (2018).

For our first policy simulation, we estimate the number of affected individuals using data from the FBI's Uniform Crime Reporting Program, which shows that there were 346,140 offenses recorded in cities in Harris County in 2009.[7] Reducing Harris County's detention rate from 65 percent to 22.2 percent to match that of Broward County thus implies that up to 148,147 people would not have been detained under our first policy counterfactual. Since the quasi-experimental estimates from Dobbie, Goldin, and Yang (2018) suggest that each newly released individual gains $29,000 in income over their lifetime, this means that reducing Harris County's detention rate from 65 percent to 22.2 percent would increase aggregate income in the county by approximately $4.30 billion over the lifetime of those released defendants.

For our second policy simulation, we estimate the number of affected individuals using the 10.08 million arrests for all offenses in the United States. The current pretrial detention rate for felony defendants is 37.71 percent, according to the 2009 SCPS. If we assume that this was the rate of pretrial detention for all arrested individuals, then this implies that if

6. For example, the outright detention for felony offenses in the 2009 SCPS is 3.9 percent of individuals, while pretrial detention rates were as low as 6 percent following the eradication of money bail in New Jersey. We also observe detention rates as low as 10 percent in parts of country that have traditionally not relied on money bail such as the District of Columbia.

7. The 2009 data from the FBI's Uniform Crime Reporting Program were released as part of the report *Crime in the United States, 2009*. These data were accessed on March 9, 2021; Crime in the United States, https://www2.fbi.gov/ucr/cius2009/documents/index.html.

money bail was eliminated and the pretrial detention rate fell to 10 percent, 2.79 million people would no longer be detained.[8] Again applying the quasi-experimental estimates from Dobbie, Goldin, and Yang (2018), this means that eliminating money bail in the United States could increase aggregate income by up to $80.91 billion per year, assuming the same number of people are no longer detained each year. We note that this is an upper bound on the change in aggregate income as equilibrium effects would tend to dampen the income effects of an increase in labor supply associated with elimination of money bail.

One important caveat is that the quasi-experimental estimates from Dobbie, Goldin, and Yang (2018) do not capture potential spillover effects of pretrial detention on other individuals. These quasi-experimental estimates are also based on the approximately 13 percent of defendants at the margin of release, not the average defendant who might be released. The estimates based on these marginal defendants may not be applicable to the average defendant who would be affected by large-scale policy reforms. We thus tentatively explore whether pretrial detention has an impact on aggregate economic measures using policy-relevant variation in detention rates and an empirical design that also captures potential spillover effects.

III.B. County-Level Detention Rates and Economic Outcomes

We explore the aggregate effects of pretrial detention inclusive of spillover effects by comparing changes in county-level poverty and employment-to-population rates to changes in county-level pretrial detention rates. We take this approach because there is substantial heterogeneity across counties in their change in pretrial detention rates from 2000 to 2009, the latest year of available SCPS data. For example, between 2000 and 2009, counties like Wayne, Michigan, and Franklin, Ohio, experienced over 15 percentage point increases in detention rates compared to counties like Broward, Florida, and Miami-Dade, Florida, which experienced over 10 percentage point decreases in detention rates.

Exploiting this time series variation, we present scatterplots and regression estimates of the following county-level specification:

$$\Delta Y_{c,2000-2010} = \alpha + \beta_1 \Delta Detention_{c,2000-2009} + \beta_2 X_{c,2000} + \varepsilon_c$$

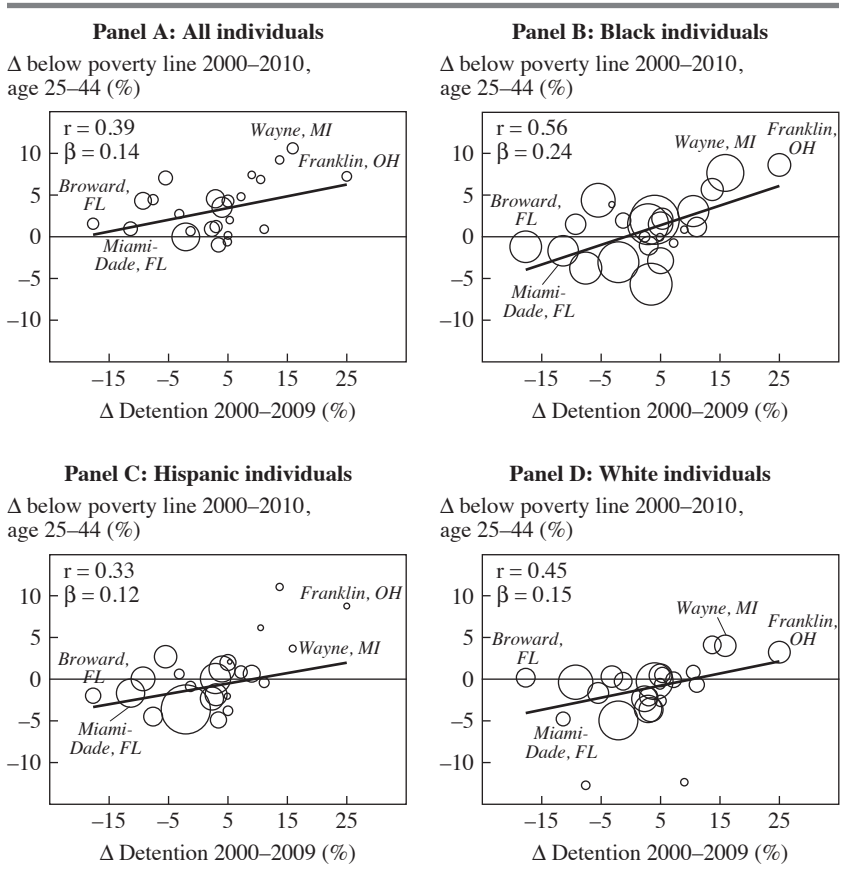
8. We note that the number of people no longer detained would likely be lower than 2.79 million given that the total number of arrests per year includes arrests for repeat offenders. This number is also a rough approximation given that we do not know the pretrial detention rate for individuals charged with misdemeanor offenses, which could be different from the rate reported in the SCPS data.

where $\Delta Y_{c,2000-2010}$ represents the 2000 to 2010 change in economic outcomes in county c (in percentage points), $\Delta Detention_{c,2000-2009}$ represents the 2000 to 2009 change in detention rates in county c (in percentage points), and $X_{c,2000}$ represents a vector of baseline county-level covariates. All results are weighted by the county-level race- and age-specific population.

Before proceeding to our estimates, we note that our county-level estimates should be interpreted with an abundance of caution. For various reasons, these county-level specifications should not be interpreted as precise or causal estimates of the relationship between changes in pretrial detention rates and changes in economic outcomes. For one, we are only able to explore these relationships among twenty-four counties in the SCPS (the best available nationally representative data) for whom we can observe detention rates in both 2000 and 2009. Thus, estimates are very imprecise. Second, we utilize county-level changes in detention rates that are likely endogenous. As such, there may be other county-level changes correlated with changes in pretrial detention rates, such that one should be cautious in interpreting β_1 as a causal effect. Nevertheless, even in the absence of convincing quasi-experimental variation in aggregate pretrial detention rates, we feel that these estimates can provide a useful first step to policymakers and researchers in shedding light on whether individual-level effects in the quasi-experimental literature may translate to aggregate economic outcomes and on the potential scope of spillover effects.

CHANGES IN DETENTION RATES AND POVERTY RATES We begin by examining the relationship between changes in detention rates and changes in poverty rates. In figure 3, we present correlations between the 2000 to 2009 change in pretrial detention rates ($\Delta Detention$) and the 2000 to 2010 change in county-level poverty rates ($\Delta Poverty$), defined as the share of the population below the poverty line. County-level poverty rates are measured using the 2000 Decennial Census and the 2006–2010 American Community Survey five-year estimates. These results show whether counties that experienced greater increases in their pretrial detention rates exhibited different changes in their poverty rates. In all results below, we present correlations and regression estimates of $\Delta Poverty$ and $\Delta Detention$ for prime working-age individuals between the ages of 25 to 44, Black individuals age 25 to 44, Hispanic individuals age 25 to 44, and non-Hispanic white individuals age 25 to 44. We focus on these demographic groups given that the pretrial system is overrepresented by Black defendants and defendants under the age of 45. The size of the circles represents the age- and race-specific county population in 2000. We explore these relationships among twenty-four counties in the SCPS for which we can observe detention rates in

Figure 3. Changes in County Detention and Poverty Rates



Sources: State Court Processing Statistics (SCPS); 2000 Decennial Census; and the American Community Survey.

Note: The change in county detention rates from 2000 to 2009 is measured using the change in the share of arrested felony defendants detained pretrial in the SCPS in the twenty-four counties with data in both 2000 and 2009. The change in county poverty rates from 2000 to 2010 is measured using the change in the poverty rate between the 2000 Decennial Census and the American Community Survey five-year estimates 2006–2010 for individuals age 25–44 in the same twenty-four counties. Correlations and best-fit regression lines are weighted using the applicable race- and age-specific county population total in 2000 as reported in the 2000 Decennial Census. The size of the circles represents the age- and race-specific county population in 2000. See table 2 for additional regression estimates and standard errors.

both 2000 and 2009. Panels A and B of online appendix table A2 present summary statistics for these changes, both unweighted and weighted by the race- and age-specific county population.

Figure 3 reveals that among individuals age 25 to 44, the correlation between Δ*Poverty* and Δ*Detention* is 0.39, suggesting that counties with larger increases in pretrial detention rates also experienced larger increases in poverty rates. For example, two counties with the largest increases in pretrial detention rates over the 2000 to 2009 time period—Wayne, Michigan, and Franklin, Ohio—experienced a growth in the poverty rate from 2000 to 2010 for individuals age 25 to 44 of 10.61 percentage points and 7.24 percentage points, respectively. In contrast, counties with large decreases in pretrial detention rates over the time period, such as Broward, Florida, and Miami-Dade, Florida, experienced a growth in the poverty rate from 2000 to 2010 for individuals age 25 to 44 of only 1.56 percentage points and 0.96 percentage points, respectively.

Consistent with these correlation estimates, a linear regression of Δ*Poverty* and Δ*Detention* with no baseline controls yields a regression coefficient of 0.14, implying that a 10 percentage point increase in the detention rate between 2000 and 2009 is associated with a 1.41 percentage point increase in the poverty rate of prime working-age individuals between 2000 and 2010. This regression coefficient similarly implies that a one standard deviation increase in the change in the detention rate between 2000 and 2009 (9.36 percentage points) is associated with a 1.32 percentage point increase in the change in the poverty rate over a similar time period. Among Black individuals age 25 to 44, the correlation coefficient between Δ*Poverty* and Δ*Detention* is 0.56 with a linear regression coefficient of 0.24, implying that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 2.21 percentage point increase in the change in the poverty rate of Black individuals age 25 to 44 between 2000 and 2010. Among Hispanic individuals age 25 to 44, the correlation coefficient is 0.33 with a linear regression coefficient of 0.13, implying that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 1.17 percentage point increase in the change in the poverty rate of Hispanic individuals age 25 to 44 over the time period. Finally, for non-Hispanic white individuals, the correlation coefficient between Δ*Poverty* and Δ*Detention* is 0.45 with a linear regression coefficient of 0.15, implying that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 1.36 percentage point increase in the change in the poverty rate of non-Hispanic white individuals age 25 to 44 over the time period.

In general, these results reveal a positive relationship between detention rate changes and poverty rate changes, with the largest correlations among Black prime working-age individuals.

In panel A of table 2, we present estimates from a regression of Δ*Poverty* on Δ*Detention* with and without baseline controls in levels. County baseline controls (measured in 2000) include the mean household income, share female, share of single parents, share foreign born, the unemployment rate, EITC exposure, violent crime rates, and total crime rates. We also include the 2000 share of population with a college degree or more, the share of women in the labor force, and the log population, following Charles, Hurst, and Notowidigdo (2016). Estimates are weighted by the county-level race- and age-specific population. Even after accounting for these baseline controls, we continue to find that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 0.98 percentage point increase in the change in the poverty rate of individuals age 25 to 44, a 3.10 percentage point increase in the poverty rate of Black individuals age 25 to 44, a 1.17 percentage point increase in the poverty rate of Hispanic individuals age 25 to 44, and a 0.54 percentage point increase in the poverty rate of non-Hispanic white individuals age 25 to 44 between 2000 and 2010. These regression estimates with controls are again consistent with a positive relationship between detention rate changes and poverty rate changes, with the largest relationship for Black individuals age 25 to 44. We caution, however, that our estimates are imprecisely estimated and that the 95 percent confidence interval includes a range of estimates. For example, the lower range of a 95 percent confidence interval suggests that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 0.96 percentage point decrease in the change in the poverty rate of individuals age 25 to 44 and a 0.36 percentage point increase in the poverty rate of Black individuals age 25 to 44.

CHANGES IN DETENTION RATES AND EMPLOYMENT-TO-POPULATION RATIOS

We now turn to the employment rate, measured by the employment-to-population ratio. In figure 4, we present correlations between the 2000 to 2009 change in pretrial detention rates (Δ*Detention*) and the 2000 to 2010 change in county-level civilian employment rates (Δ*Employment*), defined as the employment-to-population ratio among the civilian population. County-level employment rates are measured using the 2000 Decennial Census and the 2006–2010 American Community Survey five-year estimates. We present correlations and regression estimates of Δ*Employment* and Δ*Detention* for prime working-age individuals between the ages of 25 to 44, and all Black individuals age 16 to 64, Hispanic individuals

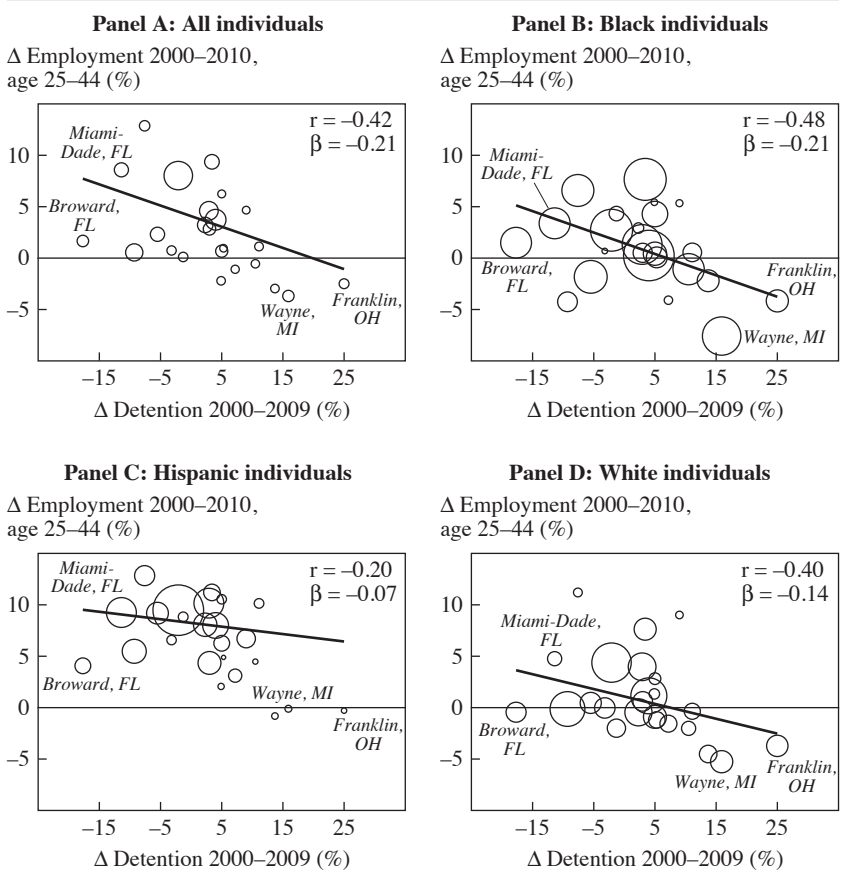
Table 2. County Detention Rates and Economic Outcomes

	All individuals		*Black individuals*		*Hispanic individuals*		*White individuals*	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Panel A: Δ*Poverty* 2000–2009								
Δ*Detention* 2000–2009	0.141	0.105	0.236	0.331	0.125	0.125	0.146	0.058
	(0.072)	(0.106)	(0.077)	(0.149)	(0.064)	(0.074)	(0.070)	(0.078)
	24	24	24	24	24	24	24	24
Panel B: Δ*Employment* 2000–2009								
Δ*Detention* 2000–2009	–0.206	–0.115	–0.208	–0.408	–0.072	–0.056	–0.144	–0.125
	(0.109)	(0.072)	(0.103)	(0.121)	(0.088)	(0.048)	(0.075)	(0.047)
	24	24	24	24	24	24	24	24
Panel C: Mobility at the 25th percentile for parental income								
Detention rate in 1990	0.021	–0.066	–0.032	–0.055	0.006	–0.040	–0.021	–0.112
	(0.026)	(0.027)	(0.019)	(0.023)	(0.022)	(0.019)	(0.027)	(0.036)
	39	39	39	39	39	39	39	39
Panel D: Mobility at the 75th percentile for parental income								
Detention rate in 1990	–0.040	–0.070	–0.025	–0.019	–0.031	–0.045	–0.069	–0.093
	(0.017)	(0.028)	(0.023)	(0.027)	(0.024)	(0.020)	(0.021)	(0.036)
	39	39	39	39	39	39	39	39
Baseline controls	No	Yes	No	Yes	No	Yes	No	Yes

Sources: State Court Processing Statistics; 2000 Decennial Census; American Community Survey; and Opportunity Insights.

Note: Panel A reports estimates of regressions of the change in poverty rates from 2000 to 2010 (in percentage points) on the change in felony detention rates from 2000 to 2009 (in percentage points) in the twenty-four counties with detention data in both 2000 and 2009. Panel B reports estimates of regressions of the change in employment-to-population rates from 2000 to 2010 (in percentage points) on the change in felony detention rates from 2000 to 2009 (in percentage points) in the twenty-four counties with detention data in both 2000 and 2009. Panel C reports estimates of regressions of the income percentiles of children born to parents at the 25th percentile on the felony detention rate in 1990 (in percentage points) in the thirty-nine counties with detention data in 1990. Panel D reports estimates of regressions of the income percentiles of children born to parents at the 75th percentile on the felony detention rate in 1990 (in percentage points) in the thirty-nine counties with detention data in 1990. In panels A–B, the baseline controls include the mean household income, share female, share of single parents, share foreign born, the unemployment rate, EITC exposure, and violent and total crime rates. Baseline controls also include the share of population with a college degree or more, the share of women in the labor force, and log population, following Charles, Hurst, and Notowidigdo (2016). All baseline controls in panels A–B are measured in 2000. In panels C–D, the baseline controls included in the even columns are the Gini coefficient for the bottom 99 percent, high school dropout rate, the share of Black individuals, the share of single mothers, a social capital index, the violent crime rate, and the total crime rate, broadly following Chetty and others (2014). All specifications use the 2000 Census group-specific population in each county as weights, report robust standard errors in parentheses, and report the number of observations below each estimate. See figures 3–6 for additional details on the coding of each outcome measure.

Figure 4. Changes in County Detention and Employment Rates



Sources: State Court Processing Statistics (SCPS); 2000 Decennial Census; and the American Community Survey.

Note: The change in county detention rates from 2000 to 2009 is measured using the change in the share of arrested felony defendants detained pretrial in the SCPS in the twenty-four counties with data in both 2000 and 2009. The overall change in county employment-to-population rates from 2000 to 2010 is measured using the change in the employment-to-population rate between the 2000 Decennial Census and the American Community Survey five-year estimates 2006–2010 for individuals age 25–44 in the same twenty-four counties. The race-specific changes in county employment-to-population rates are measured using individuals age 16–64. Correlations and best-fit regression lines are weighted using the applicable race- and age-specific county population total in 2000 as reported in the 2000 Decennial Census. The size of the circles represents the age- and race-specific county population in 2000. See table 2 for additional regression estimates and standard errors.

age 16 to 64, and non-Hispanic white individuals age 16 to 64. Due to restrictions in the availability of public data at the county level, we are unable to further disaggregate employment rates for demographic groups by narrower age ranges (e.g., Black individuals between the ages of 25 to 44). The size of the circles represents the age- and race-specific county population in 2000. Again, we explore these relationships among twenty-four counties in the SCPS for which we can observe detention rates in both 2000 and 2009. Summary statistics on these changes are presented in panels A and C of online appendix table A2.

Figure 4 reveals that among individuals age 25 to 44, the correlation between $\Delta Employment$ and $\Delta Detention$ is −0.42. Counties with large increases in pretrial detention rates, such as Wayne, Michigan, and Franklin, Ohio, experienced employment rate decreases for individuals age 25 to 44 of 3.69 percentage points and 2.49 percentage points, respectively. In contrast, counties with large decreases in pretrial detention rates over the time period, such as Broward, Florida, and Miami-Dade, Florida, experienced employment rate increases for individuals age 25 to 44 of 1.67 percentage points and 8.59 percentage points, respectively. A linear regression of $\Delta Employment$ and $\Delta Detention$ with no baseline controls yields a regression coefficient of −0.21, implying that a 10 percentage point increase in the change in the detention rate between 2000 and 2009 is associated with a 2.06 percentage point decrease in the change in the employment rate of prime working-age individuals between 2000 and 2010. This regression coefficient similarly implies that a one standard deviation increase in the change in the detention rate between 2000 and 2009 (9.36 percentage points) is associated with a 1.93 percentage point decrease in the change in the employment rate over a similar time period.

Among Black individuals age 16 to 64, the correlation coefficient between $\Delta Employment$ and $\Delta Detention$ is −0.48 with a linear regression coefficient of −0.21. This regression coefficient implies that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 1.95 percentage point decrease in the change in the employment rate of Black individuals age 16 to 64 between 2000 and 2010. Among Hispanic individuals age 16 to 64, the correlation coefficient between $\Delta Employment$ and $\Delta Detention$ is −0.20 with a linear regression coefficient of −0.07, implying that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 0.67 percentage point decrease in the change in the employment rate of Hispanic individuals age 16 to 64. Finally, for non-Hispanic white individuals age 16 to 64, the correlation coefficient between $\Delta Employment$ and

Δ*Detention* is –0.40 with a linear regression coefficient of –0.14, implying that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 1.35 percentage point decrease in the change in the employment rate of non-Hispanic white individuals age 16 to 64. Thus, there is a negative association between detention rate changes and employment rate changes, particularly among Black working-age individuals.

In panel B of table 2, we present estimates from a regression of Δ*Employment* on Δ*Detention* with and without baseline controls in levels. We use the same set of baseline controls as described above in our regressions with changes in poverty rates. We weight these regressions with the relevant county-level population for each racial and age group. With baseline controls, we find that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a 1.08 percentage point decrease in the change in the employment rate of individuals age 25 to 44, a 3.82 percentage point decrease in the employment rate of Black individuals age 16 to 64, a 0.52 percentage point decrease in the employment rate of Hispanic individuals age 16 to 64, and a 1.17 percentage point decrease in the employment rate of non-Hispanic white individuals age 16 to 64 between 2000 and 2010. As with our above results on poverty rates, these regression results suggest that local changes in detention rates are generally associated with changes in aggregate economic well-being as measured by employment rates, particularly for Black individuals. We again caution that our estimates are imprecisely estimated and that the 95 percent confidence interval includes a range of estimates. For example, a 95 percent confidence interval suggests that a one standard deviation increase in the change in the detention rate between 2000 and 2009 is associated with a –2.40 to 0.24 percentage point change in the employment rate of individuals age 25 to 44 and a –6.04 to –1.60 percentage point change in the employment rate of Black individuals age 16 to 64 between 2000 and 2010.

POLICY IMPLICATIONS We now return to our back-of-the-envelope calculations exploring how changes in the pretrial system could affect changes in poverty rates and employment rates. These calculations should be interpreted cautiously given the imprecise and noncausal nature of our exploratory estimates. The first policy we consider is shifting detention rates in a high Δ*Detention* county such as Franklin, Ohio, where pretrial detention rates increased 25 percentage points between 2000 and 2009, to match a low Δ*Detention* county such as Broward, Florida, where pretrial detention rates *decreased* by 17.7 percentage points between 2000 and 2009. The

second policy we consider is again the elimination of cash bail, where we assess what might have happened if all counties had reduced detention rates to only 10 percent in 2009 as compared to the actual increase in detention rates between 2000 and 2009.

Starting with the first policy simulation, if Franklin, Ohio, had reduced its Δ*Detention* to match that of Broward, Florida, over the same period, our regression estimates imply that there would have been a 4.48 (with controls) to 6.02 (without controls) percentage point decrease in Franklin's Δ*Poverty* for individuals age 25 to 44 between 2000 and 2010 on top of the actual observed change in poverty. Given that the actual poverty rate in Franklin increased by 7.24 percentage points over this time period, Franklin's counterfactual poverty rate would have increased by only 1.22 to 2.77 percentage points. The effects of this simulation are even larger among minority individuals. Among Black individuals age 25 to 44, if Franklin, Ohio, reduced its Δ*Detention* to match that of Broward, Florida, our regression estimates imply an additional 10.09 to 14.15 percentage point decrease in Franklin's Δ*Poverty* for Black individuals age 25 to 44 between 2000 and 2010 on top of the actual observed change in poverty. Since Franklin's actual poverty rate increased by 8.68 percentage points for this demographic group during this time period, the counterfactual poverty rate for Black prime working-age individuals would have decreased by 1.41 to 5.47 percentage points.

We can do the same simulation for employment rates. If Franklin, Ohio, reduced its Δ*Detention* to match that of Broward, Florida, our regression estimates imply a counterfactual employment rate increase of 2.42 to 6.31 percentage points compared to the actual decrease of 2.49 percentage points. Among working-age Black individuals, the counterfactual employment rate change for this demographic group would have been an increase of 4.72 to 13.27 percentage points as compared to the actual decrease of 4.16 percentage points.

We next consider what might happen if all counties eliminated money bail in 2009 such that detention rates were only 10 percent relative to the actual increase in detention rates between 2000 and 2009. If all counties had reduced their 2009 detention rates to 10 percent, this change (Δ*Detention*) would have represented a 31.33 percentage point decrease from the mean population weighted county detention rate in 2000 as measured in the SCPS data. In actuality, Δ*Detention* between 2000 and 2009 was 0.67 percentage points. Thus, our estimates in figure 3 imply that, on average, poverty rates for individuals age 25 to 44 in all counties would have decreased by 3.35 to 4.51 percentage points more than they actually changed if counties

eliminated money bail in 2009. Between 2000 and 2010, the weighted average poverty rate across all counties in the SCPS increased by 2.83 percentage points. Thus, if counties had eliminated money bail relative to actuality, our estimates suggest that counterfactual poverty rates over this same period would have instead decreased by 0.52 to 1.68 percentage points. For Black individuals age 25 to 44, our estimates imply that if counties had eliminated money bail relative to actuality, the counterfactual poverty rates would have shown a decrease of 6.83 to 9.88 percentage points compared to the actual increase of 0.73 percentage points.

Again, we emphasize that these simulations are based on imprecise estimates and magnitudes should not be taken literally. If we used the lower end of a 95 percent confidence interval, for example, our estimates suggest that if counties had eliminated money bail relative to actuality, counterfactual poverty rates over this same period would have instead increased by 2.84 to 6.16 percentage points for all individuals age 25 to 44 and increased by 0.09 to 1.57 percentage points for Black individuals age 25 to 44.

These changes may also yield economically large decreases in racial gaps in poverty rates given that reductions in detention have a greater differential impact on Black versus white prime working-aged individuals, although again we note that our estimates are measured with considerable noise. For example, in 2010, the population-weighted average poverty rate was 19.3 percent for Black individuals and 6.9 percent for white individuals. Therefore, the Black-white racial gap was 12.43 percentage points in 2010. If counties had eliminated money bail, our estimates predict that there would have been an additional change in the $\Delta Poverty$ of −4.66 to −5.51 percentage points for white individuals and −7.56 to −10.61 percentage points for Black individuals compared to actuality. Using the lower end of these estimates for both groups, the counterfactual 2010 white poverty rate would have been 2.2 percent and the counterfactual 2010 Black poverty rate would have been 11.7 percent. Thus, if counties eliminated money bail, the counterfactual racial gap in 2010 would have only been 9.53 percentage points, which is 23 percent smaller than the actual racial gap.

We now turn to similar policy simulations for employment rate changes. Between 2000 and 2010, the actual weighted average change in employment rates across all counties in the SCPS was a 3.94 percentage point increase for all individuals age 25 to 44 and a 1 percentage point increase for Black individuals age 16 to 64. If these counties had eliminated money bail in 2009 relative to the actual increase in detention rates over the time period, the counterfactual employment rate change across all counties

would have been a 7.63 to 10.54 percentage point increase for all individuals age 25 to 44 and a 7.67 to 14.08 percentage point increase for Black individuals age 16 to 64.

As before, we emphasize that these simulations are based on imprecise estimates and contain a wide range of possible estimates. If we used the lower end of a 95 percent confidence interval, our estimates suggest that if counties had eliminated money bail relative to actuality, counterfactual employment rates over this same period would have instead increased by 12.15 to 17.36 percentage points for all individuals age 25 to 44 and increased by 17.04 to 24.62 percentage points for Black individuals age 16 to 64. Using the upper end of a 95 percent confidence interval suggests much smaller counterfactual employment rate increases of 3.1 to 3.71 percentage points for all individuals age 25 to 44 and 4.17 to 9.42 for Black individuals age 16 to 64.

Eliminating money bail could also yield economically significant decreases in racial gaps in employment rates. If counties had reduced detention rates to 10 percent via elimination of money bail relative to reality, our estimates suggest that the counterfactual employment-to-population ratio would have been 76.87 percent for white individuals and 67.71 percent for Black individuals in 2010, implying a counterfactual racial gap in 2010 of 9.16 percentage points, 22.5 percent smaller than the actual 2010 racial gap of 11.82 percentage points. In sum, these exploratory simulations suggest that eliminating money bail may decrease poverty rates and increase employment rates, particularly among working-age Black individuals.

COMPARING DIRECT INDIVIDUAL-LEVEL AND AGGREGATE MACROECONOMIC ESTIMATES A natural question may be whether the aggregate macroeconomic effects (which include both direct and spillover effects) we find in this section can be explained by the direct individual-level estimates described above in section III.A. Here, we do a very crude comparison of these estimates for employment, noting that this exercise is highly speculative given the noncomparability of the estimates. For one, the direct individual-level estimates represent only local average treatment effects for defendants at the margin of release and may be inapplicable to inframarginal defendants affected by policy reforms. Second, the direct estimates are based on annual labor market changes, while the aggregate macroeconomic estimates capture cumulative steady-state changes over the course of a decade. Third, the aggregate macroeconomic estimates relying on county-level changes are noncausal in nature and have large standard errors. Therefore, we do not think that direct comparisons between these estimates are justified given the existing state of research. Nevertheless,

a rough calibration leads us to tentatively conclude that spillover effects are an area worthy of further exploration.

Recall that for each person no longer detained, Dobbie, Goldin, and Yang (2018) estimate that the probability of employment in the formal sector increases by 9.4 percentage points each year. Mapping this individual-level probability to an aggregate measure, such as the employment-to-population ratio, depends on what share of the relevant working-age population is at risk of pretrial detention. Based on McCauley (2017), estimates from the National Longitudinal Survey of Youth (NLSY) 1997 suggest that the cumulative arrest probability by age 28 for all respondents is 32 percent, which we use as a benchmark for the size of the at-risk population. If we assumed a 100 percentage point reduction in the detention rate for this at-risk population (effectively going from universal to no detention), the estimates from Dobbie, Goldin, and Yang (2018) imply that the aggregate employment rate would correspondingly rise by up to 3.01 percentage points (9.4 percentage points times 0.32) through direct effects. Turning to aggregate estimates, our county-level estimates of the association between changes in detention rates and changes in employment rates with baseline controls imply that a 100 percentage point reduction in the detention rate would increase the prime working-age employment rate by 11.5 percentage points (see panel B of table 2). However, given the large standard errors, within a 95 percent confidence interval, our county-level estimates could also imply that a 100 percentage point reduction in the detention rate would decrease the prime working-age employment rate by 2.6 percentage points. Given this wide range of potential estimates, spillover effects may be present but cannot be definitively identified with the existing data.

We can similarly conduct a crude comparison of direct individual-level and aggregate estimates for Black individuals. Among Black respondents, the cumulative probability of arrest by age 28 is 40 percent (McCauley 2017), which we take as a rough benchmark for the size of the Black at-risk population. If we assumed a 100 percentage point reduction in the detention rate for this at-risk population, the estimates from Dobbie, Goldin, and Yang (2018) imply that the aggregate employment rate for Black individuals would correspondingly rise by 3.76 percentage points (9.4 percentage points times .40) through direct effects. However, our aggregate estimates of the association between changes in detention rates and changes in employment rates for Black individuals with baseline controls imply that a 100 percentage point reduction in the detention rate would increase the Black working-age employment rate by 40.8 percentage points (see panel B of table 2). Again, however, large standard errors mean that a large range

of estimates are possible. For example, within a 95 percent confidence interval, we cannot rule out that a 100 percentage point reduction in the detention rate would increase the Black working-age employment rate by 17.1 percentage points. This back-of-the-envelope calculation suggests potentially large spillover effects but we caution that these comparisons are highly speculative.

III.C. County-Level Detention Rates and Intergenerational Mobility

We conclude this section by considering the relationship between pretrial detention and intergenerational mobility among children, an important example of an intergenerational spillover effect. We estimate the intergenerational effects of pretrial detention by comparing county-level 1990 pretrial detention rate levels and measures of intergenerational mobility obtained from Opportunity Insights, as used by Chetty and others (2018). Based on SCPS data, in 1990, counties like Fulton, Georgia, and Orange, California, have the highest rates of pretrial detention with rates around 70 percent or higher, while counties like Suffolk, Massachusetts, and Essex, New Jersey, have the lowest rates of detention, with detention rates generally below 10 percent. We explore these relationships among thirty-nine counties in the SCPS for whom we can observe detention rates in 1990.[9]

Exploiting this cross-sectional variation, we present scatterplots and regression estimates of the following county-level specification:

$$IM_{c,2014} = \alpha + \beta_1 * Detention_{c,1990} + \beta_2 * X_{c,2000} + E_c$$

where $IM_{c,2014}$ represents the predicted mean percentile rank of income for children born between 1978 and 1983 to parents at the 25th percentile in the national household income distribution when they are age 31 to 37 (as measured in 2014 to 2015) in county c. $Detention_{c,1990}$ represents the 1990 detention rate in county c (in percentage points), and $X_{c,2000}$ represents baseline county-level covariates. We specifically choose to correlate mobility with the 1990 county pretrial detention rate as this captures the exposure that a child would experience at age 7 to 12. Any relationship between this pretrial detention rate and mobility likely reflects an intergenerational spillover effect given that these children are too young to be detained in adult pretrial systems. We caution that β_1 should generally not be interpreted as a causal estimate given that we are unable to control

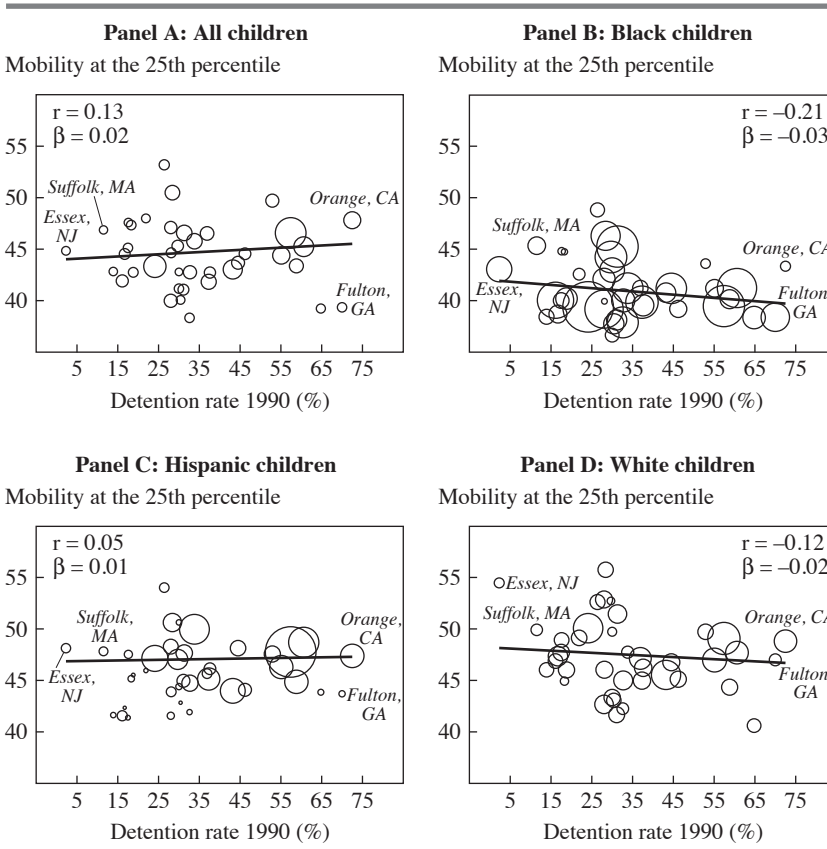
9. Summary statistics on 1990 detention rates and measures of intergenerational mobility are presented in panels A, D, and E of online appendix table A2.

for all potential differences between low- and high-detention counties, but we again feel these estimates are helpful suggestive evidence in the absence of convincing quasi-experimental variation in aggregate pretrial detention rates during this time period.

Figure 5 presents these findings and reveals a positive correlation of 0.13 between a county's 1990 rate of pretrial detention and intergenerational mobility for all children born to parents at the 25th income percentile and a linear regression coefficient of 0.02. However, this correlation becomes negative once one focuses in on Black children born to parents at the 25th income percentile. Among Black children, the correlation is −0.21 and the linear regression coefficient is −0.03. Counties with very high levels of pretrial detention rates in 1990, such as Fulton, Georgia, have a predicted mean percentile rank for Black children of 38.39. In contrast, counties with low levels of pretrial detention in 1990, such as Suffolk, Massachusetts, have a predicted mean percentile rank for Black children of 45.36. These regression estimates imply that for Black children, a one standard deviation increase in the 1990 detention rate (16.9 percentage points) is associated with a decrease in the predicted mean percentile rank of 0.54. For Hispanic children, a one standard deviation increase in the 1990 detention rate is associated with an increase in the predicted mean percentile rank of 0.1 and for non-Hispanic white children, a one standard deviation increase in the 1990 detention rate is associated with a decrease in the predicted mean percentile rank of 0.35. These results suggest that a characteristic of a high-mobility county may be its rate of pretrial detention, although we again note that our estimates are imprecise and noncausal in nature.

In panel C of table 2, we present estimates from a regression of mobility on 1990 detention rates with and without baseline controls. County baseline controls include the Gini coefficient for the bottom 99 percent, high school dropout rate, share black, share single mothers, social capital index, and violent and total crime rates, following Chetty and others (2014). We weight these regressions with the relevant county-level population for each racial and age group. After accounting for these baseline controls, we find that a one standard deviation increase in the 1990 detention rate (16.9 percentage points) is associated with a decrease in the predicted mean percentile rank of 1.12 for all children, a decrease in the predicted mean percentile rank of 0.93 for Black children, a decrease in the predicted mean percentile rank of 0.68 for Hispanic children, and a decrease in the predicted mean percentile rank of 1.89 for non-Hispanic white children. In figure 6 and panel D of table 2, we present analogous results measuring mobility for

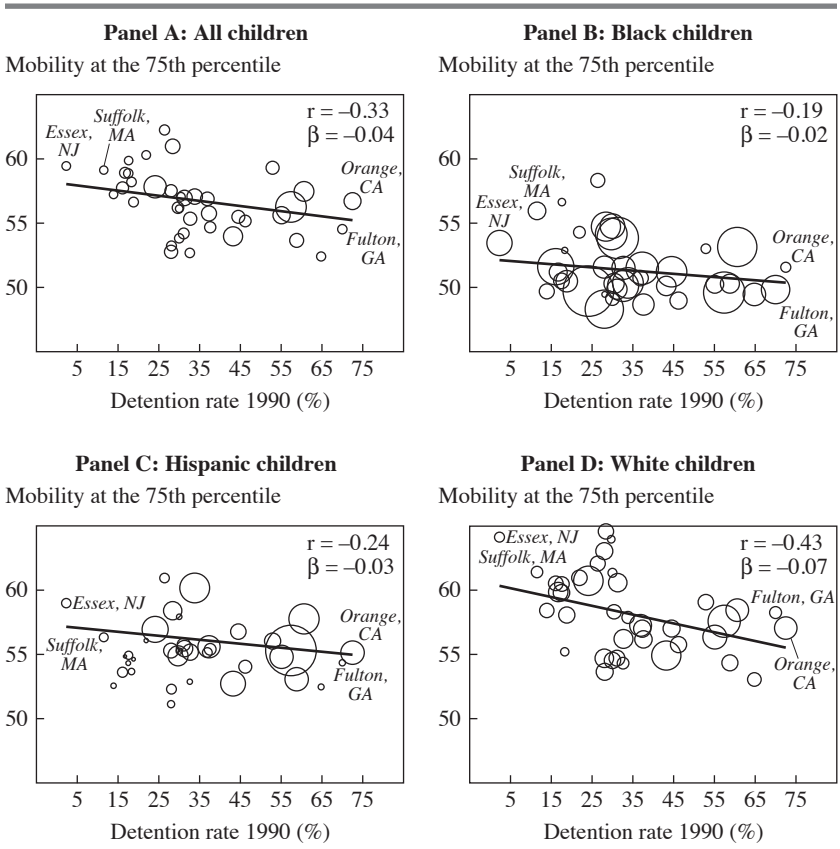
Figure 5. County Detention Rates and Mobility at the 25th Percentile for Parental Income



Sources: State Court Processing Statistics (SCPS); Opportunity Insights.

Note: The county detention rate is measured using the share of arrested felony defendants detained pretrial in 1990 in the SCPS for thirty-nine of the nation's seventy-five most populous counties. The income percentiles for children born 1978–1983 with parents at the 25th percentile are measured using the predicted mean percentile rank for children in the individual distribution of household income in 2014–2015 born to parents at the 25th percentile in the national household income distribution in each corresponding county. Correlations and best-fit regression lines are weighted using the applicable race-specific county population total as reported in the 2000 Decennial Census. The size of the circles represents the race-specific county population in 2000. See table 2 for additional regression estimates and standard errors.

Figure 6. County Detention Rates and Mobility at the 75th Percentile for Parental Income



Sources: State Court Processing Statistics (SCPS); Opportunity Insights.

Note: The county detention rate is measured using the share of arrested felony defendants detained pretrial in 1990 in the SCPS for thirty-nine of the nation's seventy-five most populous counties. The income percentiles for children born 1978–1983 with parents at the 75th percentile are measured using the predicted mean percentile rank for children in the individual distribution of household income in 2014–2015 born to parents at the 75th percentile in the national household income distribution in each corresponding county. Correlations and best-fit regression lines are weighted using the applicable race-specific county population total in 2000 as reported in the 2000 Decennial Census. The size of the circles represents the race-specific county population in 2000. See table 2 for additional regression estimates and standard errors.

children born to parents at the 75th income percentile, where we continue to find a general negative association between a county's 1990 pretrial detention level and mobility of children of all races.

POLICY IMPLICATIONS We can again use this type of cross-sectional evidence to simulate the two types of policy counterfactuals utilized previously. We first evaluate the impact on individuals of a change in detention rates, shifting rates in high-detention counties to match those in low-detention counties, before assessing what would happen if all counties reduced detention rates to only 10 percent via elimination of money bail relative to actuality.

For example, if Fulton, Georgia, reduced its detention rate to match the detention rate in Suffolk, Massachusetts, in 1990, there could be up to a 1.25 percentile decrease or a 3.84 percentile increase in the mean predicted percentile rank for all children born to parents at the 25th income percentile. Compared to Fulton's actual mean percentile rank of 39.35, its counterfactual percentile rank for all children would be 38.10 to 43.19. For Black children born to parents at the 25th income percentile, there would be an associated 1.84 to 3.20 percentile increase in the mean predicted percentile rank. Compared to Fulton's actual mean percentile rank of 38.39 for this demographic group, its counterfactual percentile rank for Black children would be 40.24 to 41.59.

Increases in intergenerational mobility would also occur if detention levels for all counties were reduced to 10 percent, as could be achieved by eliminating money bail. For intergenerational mobility, the mean predicted percentile rank of children across all counties in the SCPS is 44.82 for all children and 40.89 for Black children born to parents at the 25th income percentile. Applying our cross-sectional estimates, if these counties had reduced their 1990 detention rates to 10 percent, the counterfactual mean predicted percentile rank across all counties would have been 44.18 to 46.81 for all children and 41.84 to 42.54 for all Black children.

In addition, these simulations suggest that reforms like the elimination of money bail may also yield improvements in racial gaps in intergenerational mobility. For example, the mean predicted percentile rank is 47.43 for white children and 40.89 for Black children born to parents at the 25th percentile. Thus, the white-Black racial gap in mobility is 6.54 percentile ranks. As mentioned above, if counties eliminated money bail, our estimates suggest that the counterfactual mean predicted percentile rank for these same groups could instead be 41.84 to 42.54 for Black children and 48.05 to 50.81 for white children. Using the lower end of these counterfactual estimates, this policy reform, while benefiting both

racial groups, could also potentially reduce racial gaps in intergenerational mobility to 6.21 percentile ranks.

IV. Conclusions and Areas for Future Work

The US pretrial system has dramatically expanded over the past several decades and affects more than 10 million arrested individuals each year. The increasingly high rate of monetary bail coupled with the low financial resources of many arrested individuals has resulted in high rates of pretrial detention among these individuals, particularly for low-income minority populations. Much work remains to be done to understand the economic consequences of this pretrial system. While some recent research has started to measure the individual-level effects on individuals detained at the margin, rigorous work studying the potential spillover effects on families and community members is much needed.

This paper describes several pieces of evidence that can provide helpful guidance for policymakers. First, we document the significant direct consequences of pretrial detention on individual economic outcomes such as formal labor market attachment and the receipt of social benefits such as UI and the EITC. Second, we exploit county-level changes to show that these adverse consequences are also present in aggregate measures of economic well-being that incorporate spillover effects on other individuals. Finally, we provide more tentative evidence that pretrial detention may reduce the economic mobility of children. Put together, these three pieces of evidence indicate that reducing the scope of the pretrial system, such as through the elimination of money bail, is likely to generate significant economic returns for both directly affected individuals and the communities they live in.

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