Local Labor Market Inequality in the Age of Mass Incarceration

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Abstract: We contend that the rise of mass incarceration in the United States can be framed through the lens of stratification economics, which views race- and class-based discrimination as a rational attempt on behalf of privileged groups to preserve their relative status and the material benefits which that status confers. Using the first (to our knowledge) local-level data set on incarceration rates by race, we explore the relationship between income inequality, poverty, and incarceration at the commuting zone level from 1950 to the present. Consistent with Alexander's (2010) hypothesis that expansion of the penal system and the rise of "tough on crime" policy were an effort by privileged groups to drive a wedge into working class political coalitions formed out of the Civil Rights Movement, we find that labor markets with greater inequality experienced larger increases in the overall incarceration rate. Further, we find that *relative* rates of poverty play a key role in explaining differential effects of mass incarceration across race. Areas where white poverty rates were large relative to non-white poverty rates experienced no significant change in white incarceration, but an *expansion* of non-white incarceration. These findings have implications for policies related to economic and judicial systems.

Keywords: Inequality, Poverty, Mass Incarceration, Stratification Economics
JEL Codes: Z13, R11, D31

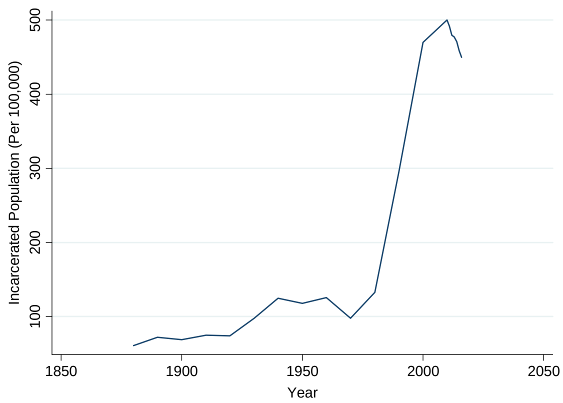
I. Introduction

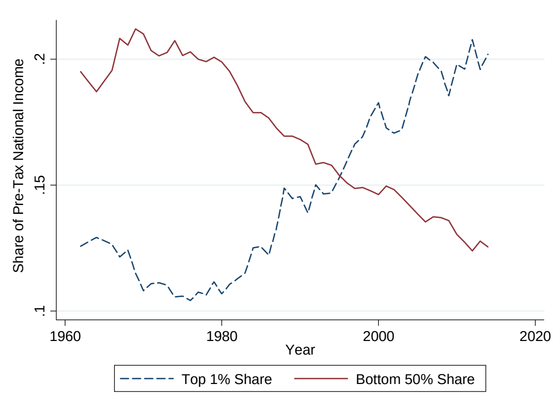
From 1970 to 2010 the number of persons incarcerated in State and Federal prisons in the United States increased from 100 per 100,000 to nearly 500 per 100,000. To contextualize the magnitude of this increase, note that the average incarceration rate in State and Federal facilities from 1880 to 1970 was approximately 91 persons per 100,000, with single-year incarceration rates never exceeding 150. Addressing the expansion of the U.S. prison population over this period, Alexander (2010) writes that "[t]he American penal system has emerged as a system of social control unparalleled in world history" (p.8). Despite holding only 5% of the world's total population, the United States now holds nearly 25% of the world's prisoners (Darity, Hamilton, and Zaw, 2016).

The economic and social consequences of the post-1970 prison boom—particularly among low income households and people of color—are well documented. Formerly incarcerated individuals face worse job prospects, lower rates of wealth accumulation, a higher likelihood of engaging in future criminal activity, and increased instability in family life (Pager, 2003; Alexander, 2010; Western and Pettit, 2010; Darity, Hamilton, and Zaw, 2016; Liu, 2018). Despite a large body of research on the consequences of mass incarceration, little has been written by economists about the economic correlates of the prison boom[1]. This is surprising, because at least one well-known secular trend suggests itself as an explanatory factor: the rise in economic inequality in the post-1980 period. It is well-established that nearly all the gains in income and wealth in the United States after 1980 were appropriated by households at the top of the income distribution (Piketty, 2014; Saez and Zucman, 2016; Piketty, Saez, and Zucman, 2018) and that the racial wealth gap is both persistent and significant (e.g., Hamilton and Darity, 2010; Hamilton and Darity, 2017). Figure (1) plots the share of pre-tax income going to both the top 1% and bottom 50% of the income distribution from 1962 to 2014, side-by-side with the incarceration rate. While the incarceration rate skyrocketed, the share of national income claimed by the bottom 50% of the distribution fell from approximately 22% to 12%, while the share claimed by the top 1% rose from 10% to nearly 21%.

[1] A search of recent issues of the *American Economic Review* revealed no articles with titles or abstracts containing the phrase "mass incarceration." Only four papers with titles or abstracts containing the word "incarceration" were found.

Figure 1: Inequality and Incarceration





(1.a) Incarceration Rate, 1880-2016 **(1.b) Inequality, 1962-2014**

Notes: Incarceration data from the Bureau of Justice Statistics. Inequality data from the World Top Incomes Database. Figure (1.b) plots data on pre-tax national income shares.

The concomitant rise of inequality and incarceration is suggestive about the political economy of the prison boom. Alexander (2010) makes the case that mass incarceration arose as a means of re-establishing a racialized social order after the downfall of Jim Crow during the Civil Rights Movement. In particular, Alexander (2010) shows that "tough on crime" policy and the War on Drugs were used as a "racial bribe," intended to drive a wedge into newly formed multi-racial working-class political coalitions:

> Just as race had been used at the turn of the century by Southern elites to rupture class solidarity at the bottom of the income ladder, race as a national issue had broken up the Democratic New Deal 'bottom up' coalition—a coalition dependent on substantial support from all voters, white and black, at or below the median income. (p. 47).

The attempt of white elites to maintain and intensify the existing system of racialized social control by adopting policies expanding the penal system in response to perceived threats—the fall of Jim Crow, the formation of new working-class political coalitions—is exactly the kind of behavior predicted by the field of stratification economics. Stratification economics—in contrast to the Neoclassical economic theory of discrimination, which characterizes discrimination as an irrationality that will eventually be priced out of the market—argues that race- and class-based discrimination represent a rational attempt by privileged groups to maintain their relative status and the material benefits which that status confers (Darity, 2005; Darity, Hamilton, and Stewart, 2015). Viewed through this lens, the simultaneous rise of mass incarceration and inequality is not only not surprising, it is expected. As economic inequality increases, so do the returns to privilege. As a result, local elite have an incentive to increase their stratification efforts—including via more restrictive penal policy—potentially leading to higher incarceration rates in areas with greater economic inequality.

In this paper, we expand on the understanding of mass incarceration as a system of racialized social control—a system which "depends for its survival on the tangible and intangible benefits that are provided to those who are responsible for the system's maintenance and administration"

(Alexander, 2010, p. 72)—by examining the relationship between incarceration rates and inequality in local labor markets. Using Census microdata, we construct the first (to our knowledge) complete data set on incarceration rates by race at the local labor market level, spanning the period from 1950 to 2010. Local labor market conditions have been increasingly acknowledged as a key determinant of an individual's lifetime outcomes (Chetty et al., 2014; Amior and Manning, 2018), and are an important backdrop upon which to assess the correlates of mass incarceration. We validate our incarceration rate estimates using a recently released data set on prison population by county-of-commitment from the Vera Institute of Justice, which covers prison populations for some—but not all—counties in the United States from 1983 onward.

Using our estimates of the incarceration rate, we examine the relationship between local inequality, poverty, and incarceration at the commuting zone level from 1950 to 2010. We examine if—and to what extent—the expansion of incarceration in the post-1970 period was larger in areas with greater local labor market inequality. Consistent with both the predictions of stratification economics and Alexander's (2010) hypothesis that the expansion of the penal system and the accompanying rise of "tough on crime" policy were an effort by privileged groups to drive a wedge into working class political coalitions formed out of the Civil Rights Movement, we find that labor markets with higher levels of inequality experienced larger increases in the overall incarceration rate. Further, we find that *relative* rates of poverty play a key role in explaining the differential effects of mass incarceration across race. Areas where white poverty rates were large relative to non-white poverty rates experienced no significant change in white incarceration rates, but an *expansion* of non-white incarceration rates.

These findings are striking and shed light on the *social* consequences of economic inequality. Our results suggest that the negative social consequences of rising inequality are disproportionately borne by non-white individuals. We argue that these distributional inequities have implications for public policies related to economic and judicial systems including for our understanding of the "Formerly Incarcerated Reenter Society Transformed Safely Transitioning Every Person" or "First Step" Act of 2018 which is being addressed in this special journal issue.

The rest of the paper is organized as follows. Section II discusses the varying theoretical perspectives that have been adopted to explain mass incarceration, before reviewing the empirical literature. Section III discusses the construction of the data and the estimation strategy. Section IV presents and discusses the results. Section V concludes.

II. Literature and Theoretical Considerations

A. Mass incarceration as rational response to rising crime (The Neoclassical Approach)

Much of the economic literature on incarceration views rising incarceration rates as a rational response to rising crime rates (Frost and Clear, 2018). We refer to this as the Neoclassical approach to mass incarceration. The Neoclassical approach prescribes punishment in the form of incarceration following spikes criminal activity in the fashion laid out by Becker (1968). Becker's model of crime and punishment starts with the economic concept of "costs" framed within the context of criminal punishment and probabilities. The model ultimately reduces incarceration to the equilibrium of supply and demand in a market for crime, assuming rational utility maximizing agents (often acting in isolation) on both the "crime" and "punishment" sides of the market.

Early empirical literature finds mixed results when examining whether crime decreases in the presence of changes to the severity of punishment. Benson, Rasmussen, and Kim (1998) argue that the incentives of police bureaucrats are not fully accounted for in these papers. They present a permutation of the traditional production function modeling in which the assumption of reductions in crime necessarily stem from increases in police resources and budgets. The authors exploit variation in both crime rates and budgets, finding evidence of substitution of enforcement across different types of crime as a result.

More recent research suggests that—if the goal of criminal punishment is to reduce the future supply of criminal behavior, consistent with the Neoclassical approach—mass incarceration is inconsistent with its *raison d'etre.* Using quasi-random variation in punishment severity driven by the assignment of judges to 35,000 juvenile offenders, Aizer and Doyle (2015) find that incarceration leads to lower high school completion rates and higher *adult* incarceration rates, including for violent crimes.

B. Mass incarceration as a system of racialized social control (The Stratification Economics Approach)

In contrast to the Neoclassical approach, which in some models imagine a straight line from criminal behavior to incarceration, a growing body of research links politics, public opinion, and other social and institutional factors to incarceration rates. In the chapter on mass incarceration in *The Oxford Handbook of Sentencing and Corrections*, Simon (2012) lists time, geography, and institutions as the major determinants of incarceration rates. Among theories related to politics, public opinion, and institutional features of the legal and criminal justice system, Simon (2012) describes "racialized threat" as a prominent explanation for mass incarceration. Authors in this literature cast "the prison as the new urban ghetto and the policies that led to mass incarceration as the new Jim Crow" (Frost and Clear, 2018). In this reasoning, mass incarceration is synonymous with attempts at social control of marginal populations (by stratifications such as social group, economic circumstance, health and the like). We identify this approach to mass incarceration as the "Stratification Economics" approach, which we explore and expand upon in this paper as an appropriate framework in which to view distinctive economic relationships to incarceration by race.

Research on police incentives helps frame the stratification economics argument as applied to mass incarceration. Much of the police incentives literature examines data following Ronald Reagan's "war on drugs" declaration in 1982 and the subsequent Comprehensive Crime Act of 1984—particularly its provisions in terms of forfeiture laws (e.g., Benson, Rasmussen, and Sollars, 1995). Incentives were put into place for law enforcement to profit from property "forfeited" due to involvement in criminal activity. An examination of the determinants of non-capital police expenditure reveals that such expenditure is increasing in property crime, property value, and confiscations—all unsurprising given the institutional environment. Economic incentives also came through federal grants to local law enforcement agencies after the revision of a federal aid statute under the Edward Byrne Memorial State and Local Law Enforcement Assistance Program in 1986. These institutional changes led authors to describe the phenomenon of "policing for profit" (Blumenson and Nilsen, 1998). More recent literature has re-examined the effects of changes to police incentives over time and has concluded that forfeiture laws have additional unintended consequences such as reallocations of police funding, increased drug arrest rates, and increased heroin prices (consistent with a supply cost effect) (Baicker and Jacobson, 2007). Other literature alleges that police have incentives to facilitate confessions even if they are false (Kassin et al., 2010) and likewise for convictions (Koppl and Sacks, 2013).

Further work on the war on drugs provides insight as to the effects of police incentives, some seemingly diverse, unexpected, and tangential. Blumenson and Nilsen (2002), for example, discuss multiplicative effects of the war on drugs coupled with zero tolerance programs in secondary schools and loan denial programs at the college level. They cite the Drug Free Student Loans Act of 1998 as contributing to an institutional framework wherein drug offenses resulted in the denial of education in addition to more direct legal consequences. Unintended and consequential impacts of the war on drugs in the health arena include increases in HIV risk associated with the criminalization of the act of carrying syringes (and drug paraphernalia more broadly) and exclusions from the SSI federal aid program due to drug-related disqualifications (Bluthenthal et al., 1999). These findings are suggestive of the mitigating and otherwise effecting roles of public policies including those at lower levels of geography. Expanding on this idea, Lynch (2012) describes the war on drugs as multileveled: starting with federal sentencing but coupled with state and local level initiatives, either supporting or detracting from the federal efforts. Some states developed drug law implementation programs whereas others pushed back with initiatives to decriminalize. Further complexity comes from local-level drug courts and drug diversion programs and changes to policing. These patterns and observations motivate our attention to lower level geographic differences in experiences by race.

Work on inequality and its intersections with race, crime, and incarceration is also related to our hypotheses. Altonji and Blank (1999) provide a meta-analysis of the literature on race and gender in the labor market. Cain (1986) provides an earlier survey on labor market discrimination. Smith and Welch (1979) show differences in both the mean and spread of the wage distribution within and across racial groups. Charles and Guryan (2008) provide a data-driven test of Becker's model of discrimination. These authors find that black-white relative wages increase with white population and with several indicators of discriminatory attitudes drawn from the General Social Survey.

Theories behind the mechanisms responsible for gaps in wages between black and white workers range from skill differences to aspects of discrimination driving a wedge between worker outcomes. Pena (2018), in a study on differences in cognitive skill, debunks previously unobservable skill differences as drivers of the racial wage gap. Manduca (2018) attributes the seemingly relatively constant racial wage gap over the last 50 years to growing income inequality. In other words, income inequality counteracted upward mobility through education and labor channels.

Bayer and Charles (2018) also focus on inequality, pointing out that many past labor studies focus only on the employed subpopulation. These authors show that when the un- and under-employment are considered the median black-white earnings gap is increasing with relative gains of black men at the top of the income distribution but also increases in incarceration of black men at the bottom of the distribution. Western and Pettit (2005) indicate the importance of unemployment and changes in employment (sample selection related to employment) on the racial wage gap, though they focus on incarcerated men instead of the larger population.

Among treatments of inequality and incarceration, Western and Wildeman (2008) paint a picture of racial differences in incarceration of much higher magnitudes than several other social indicators including unemployment, nonmarital childbearing, infant mortality, and wealth. The authors argue that there are substantial life course effects associated with this including detrimental outcomes for long-run family and economic life. Pettit and Gutierrez (2018) add that incarceration differences by race result in other social effects including changes in voting behavior, political engagement, and overall trust in the legal system within communities. Presidential rhetoric in public speeches is even found to influence the black arrest rate (Yates and Whitform, 2009), thus suggesting interrelationships with politics and elections.

Risk of imprisonment is found in the empirical literature to be stratified by race with blacks having lifetime incarceration probabilities many times that of whites with Latinos in the middle (Pettit and Western, 2004; Western and Pettit, 2010). Wakefield and Uggen (2010) discuss how the literature indicates that vocational and educational programs in prisons do little to rehabilitate the incarcerated or decrease income inequality. Instead, evidence suggests that prisons reinforce existing inequities across groups.

Taking a step back, there also is substantial literature linking inequality and crime in various contexts and settings. Fajnzylber, Lederman, and Loayza (2002), for example, find positive associations between inequality and crime across countries. Likewise, Kelly (2000) finds the same for violent crime within the U.S. Choe (2008) finds a positive effect of relative income inequality on the specific economic crime of burglary and robbery but not violent crime nor property crime. Scorzafave and Soares (2009) find positive associations for pecuniary crime including property crime in the Brazilian context. These findings are complicated, however, by applications drawn from time series data and econometrics that find that inequality may *decrease* crime if income inequality is associated with increased demand for policing which could reduce the return to crime (Chintrakarn and Herzer, 2012). In earlier work, Brush (2007) also finds a negative association in time series data but a significant positive association in cross sectional data. Finally, Lofstrom and Raphael (2016) write that “poor and minority communities have disproportionately experienced both the decline in crime and the increase in criminal justice sanctioning” (p. 104) though argue that these patterns could hypothetically be unrelated, speaking to the potential importance of multidimensional policy.

III. Data and Estimation Strategy

A. Data

Incarceration rates for community zones

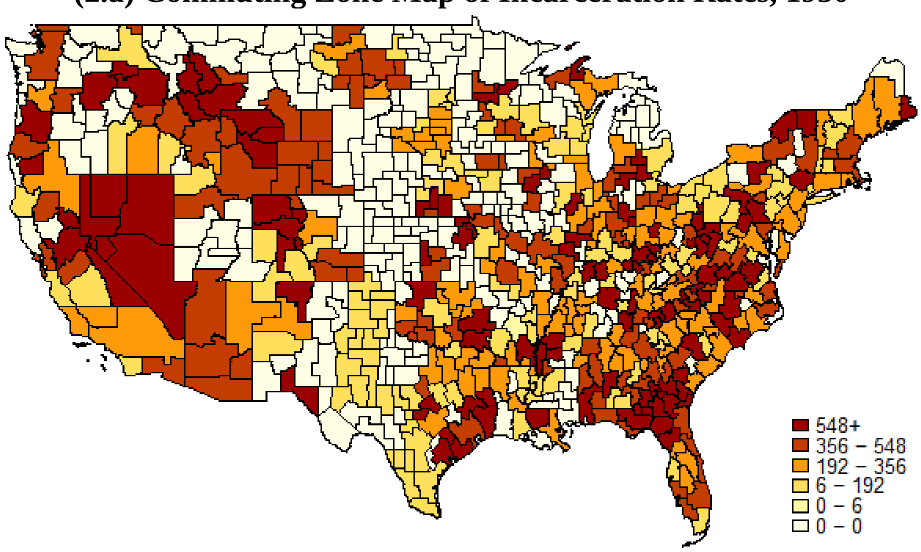
Data on commuting zone-level incarceration rates are constructed using the IPUMS Decennial Census Microdata for the years 1950 to 2010 (Ruggles et al., 2019). We focus on commuting zones because of our interest in the relationship between local labor market inequality and incarceration outcomes. Commuting zones are clusters of counties characterized by strong commuting ties and were initially defined by Tolbert and Sizer (1996) with the express purpose of constructing a geographic unit that best captured the notion of a local labor market.

Commuting zones have been shown to be an appropriate geographic level in which to analyze questions of long-run wellbeing (e.g., Chetty, Hendren, Kline, and Saez, 2014; Chetty and Hendren, 2018). Our sample frame consists of the 722 commuting zones making up the contiguous United States. We focus on the population of working-aged individuals between 15 and 64.

In each sample year, the IPUMS data classifies all housing units as falling into one of three categories: households, group quarters, or vacant units. Institutionalized individuals—including those incarcerated in State or Federal prisons or local correctional facilities—are classified as residing in group quarters. For the years 1950 to 1980, the Census specifically designates whether an individual was housed in a correctional institution. For each of these years, we estimate the number of incarcerated individuals in a given commuting zone by totaling the number of individuals housed in correctional institutions. We aggregate the individual-level microdata to commuting zones following Autor and Dorn (2013)[2].

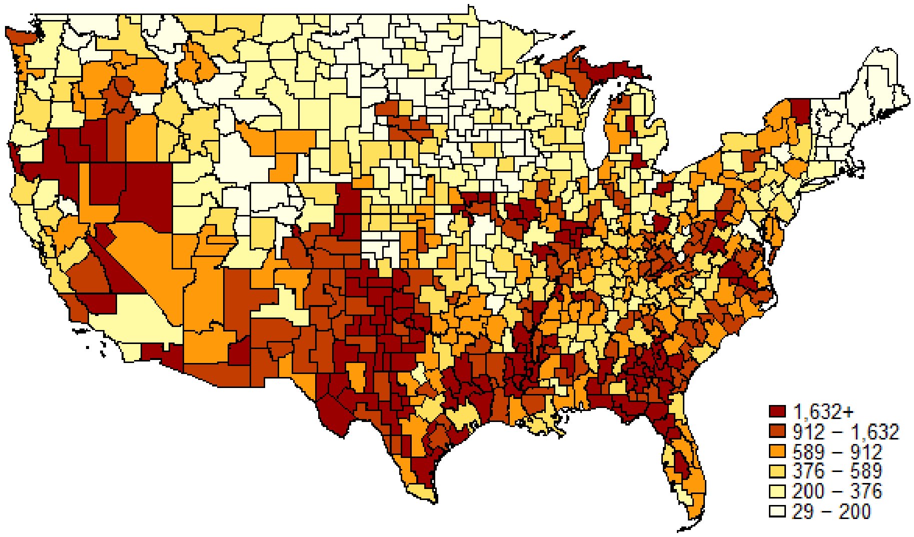
From 1990 onward, the Census indicates whether an individual residing in group quarters was institutionalized but does not specify correctional institutions in detail. Thus, for the 1990 to 2010 samples we apply an adjustment factor based on the proportion of institutionalized individuals residing in correctional institutions in 1980. We calculate a unique correction factor for each state-by-race group—defined as the share of the institutionalized population belonging to each subgroup that was housed in a correctional institution in 1980. We then multiply this correctional factor by the total number of institutionalized individuals in a commuting zone to arrive at our estimate for the years 1990 to 2010. Figure (2) presents a map of our commuting zone-level incarceration rates—defined as the number of incarcerated individuals per 100,000—for the years 1950 and 2010.

Figure 2: Mapping the Incarceration Rate
(2.a) Commuting Zone Map of Incarceration Rates, 1950



[2] For 1960, which is left out of the Autor and Dorn (2013) sample, we use the commuting zone crosswalk provided by Evan Rose, made available here: https://ekrose.github.io/resources/.

(2.b) Commuting Zone Map of Incarceration Rates, 2010



Notes: Figures plot the number of incarcerated persons per 100,000 population at the commuting zone-level. Incarceration estimates constructed using the IPUMS/Census microdata. Commuting zone crosswalks obtained from Autor and Dorn (2013).

Before discussing the spatial trends in incarceration implied by Figure (2), we validate our incarceration rate estimates by examining (A): whether our estimates replicate the movement in incarceration rates over time implied by the aggregate data, and (B): whether our estimates are suitably representative at the local level. To proceed with the validation exercises we make use of two alternative measures of incarceration. First, we use the aggregate incarceration rate—measured by the total number of people held in State or Federal prisons or local correctional facilities—per 100,000 people compiled by the Bureau of Justice Statistics (BJS). The Bureau of Justice Statistics data are compiled from several sources including the Annual Probation Survey, the Annual Parole Survey, the Annual Survey of Jails, the Census of Jail Inmates, and the National Prisoner Statistics Program. As our Census-based estimate of the incarceration rate focuses on the working-age population, we make use of the BJS data from 1980 onward, for which aggregate incarceration rates for those 18 and older are calculated separately, allowing for a closer comparison between the two rates.

Second, we use a recently released data set on incarceration rates by county of admission from the Vera Institute of Justice[3]. The Vera Institute data compile county-level estimates of prison populations using the Bureau of Justice Statistics National Corrections Reporting Program. This data is then merged with county jail population estimates from the Census of Jails and the Annual Survey of Jails to construct a national data set allowing the examination of jail and prison populations at the county-level. The Vera prison estimates are available from 1983, and the Vera jail estimates from 1970. The data cover the working aged population 15 to 64. An important limitation of the Vera data concerns the lack of availability for many counties in certain years. This limitation results from the voluntary nature of the National Corrections Reporting Program. As an example, in 1983—the first year of the Vera prison data—county-level prison populations are only available in twenty states, and not every county within each state is represented. This means that the overall geographic coverage of the Vera data is less than that which is achievable with the IPUMS/Census data. By our estimate, the average number of commuting zones

[3] http://trends.vera.org/incarceration-rates.

available in the Vera data on a decennial basis over the 1980-2010 period—after aggregating from the county level—is approximately 562.[4].

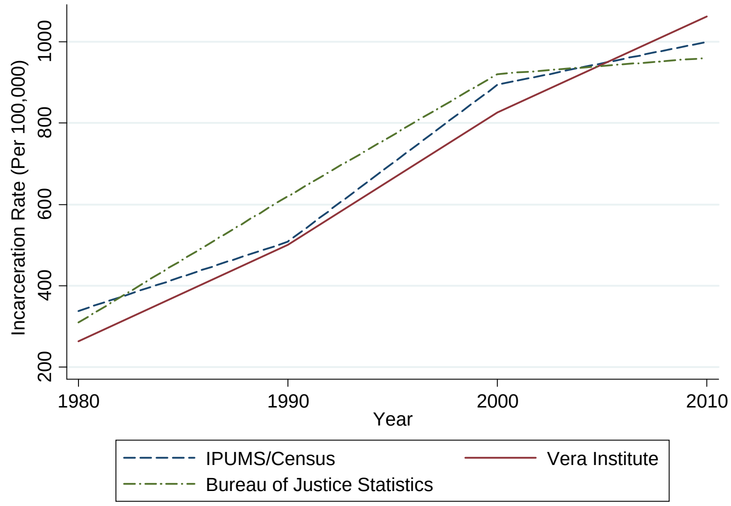
Despite these shortcomings, the Vera data nonetheless constitute an important source of external validation for the IPUMS/Census estimates, primarily because of the difference in where prisoner location is recorded in the two sources. It is well known that the Census reports an individual's location based on their "usual residence." The residence rule results in the location of incarcerated persons coinciding with the location of the facility they are in, rather than their "true" home address outside of prison. In contrast, the Vera data estimates prison population by county of admission, rather than facility location. At the state-level, this distinction should not make much difference. Federal prisoners make up only a small proportion of the overall incarcerated population, such that state-level totals should be roughly the same whether the admission location or holding location is used in the count. However, as geographic detail is refined the distinction between admission location and holding location becomes increasingly important. At very low levels of geographic aggregation counts will potentially differ greatly depending on which location rule is used. The Vera thus allows us to make a crucial check of whether the IPUMS/Census data are suitably representative at the commuting zone level. If commuting zones are at a sufficiently high level of geographic aggregation—such that the impact of using alternative location rules is muted—the Vera Institute data and the Census data should be highly correlated[5].

We perform a validation exercise by comparing aggregate trends in incarceration across our three data sources from 1980 onward. Specifically, we compare the aggregate totals from the Bureau of Justice Statistics with annual averages across commuting zones from the IPUMS/Census data and Vera data. Figure (3) presents the results of this comparison.

[4] This number may overstate the reliability of the Vera data, as it is likely that some commuting zone totals are underestimated due to missing totals in the underlying county-level data. Reporting to VERA is voluntary and not all states/counties participate in the program. This selection is avoided in the Census data.

[5] Further, in Section IV we show that similar parameter estimates to the ones in our main econometric specification are obtained if one uses the Vera Institute incarceration rate as the dependent variable in an otherwise identical regression specification.

Figure 3: Data Validation, Comparison of Aggregate Trends



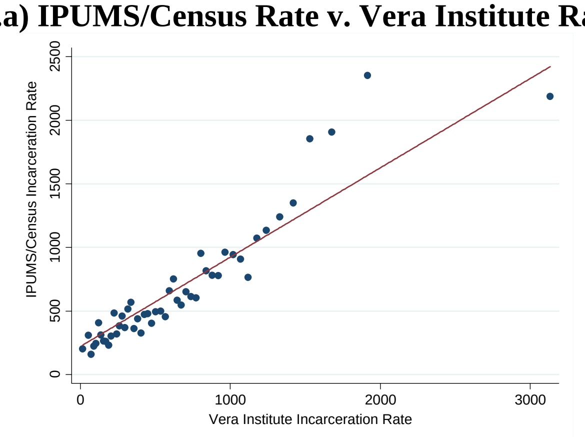
Notes: Figure presents estimates of the total number of incarcerated persons—including individuals in State or Federal Prisons and local correctional facilities—per 100,00 population, from each alternative data source.

The trends presented in Figure (3) indicate that both the IPUMS/Census data and the Vera Institute data successfully replicate the aggregate incarceration trends in the Bureau of Justice Statistics data. The ex-ante correlation between all three measures at the aggregate level over the 1980-2010 time period is close to 0.98.

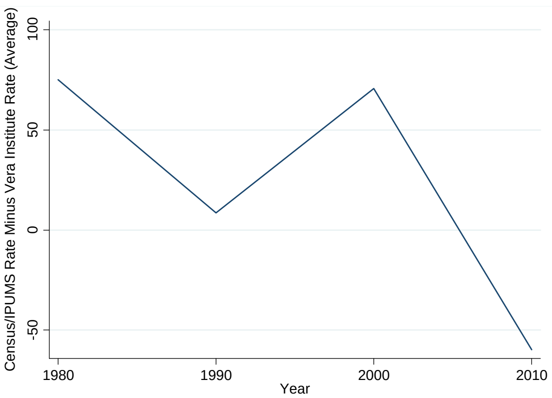
Next, we assess the extent to which the IPUMS/Census data is suitably representative at the commuting zone level. To do this we adopt two approaches. First, we present a binned scatterplot of the IPUMS/Census data against Vera data to examine the relationship between the two across all time periods. Second, we calculate the difference between the IPUMS/Census data and the Vera data for each commuting zone and plot the change in the median and average of this difference over time across all commuting zones to assess whether there are serially correlated measurement differences between the two. Figure (4) presents each of these plots.

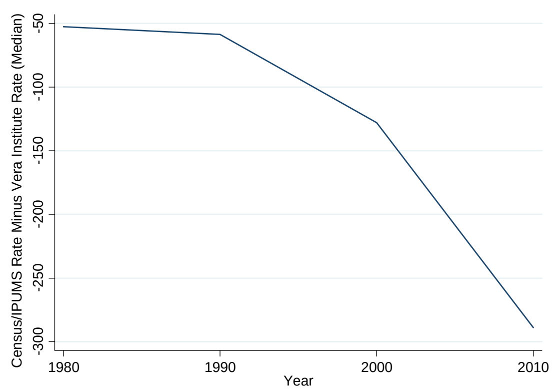
Figure 4: Data Validation, Local Representation

(4.a) IPUMS/Census Rate v. Vera Institute Rate



(4.b) Average Difference **(4.c) Median Difference**





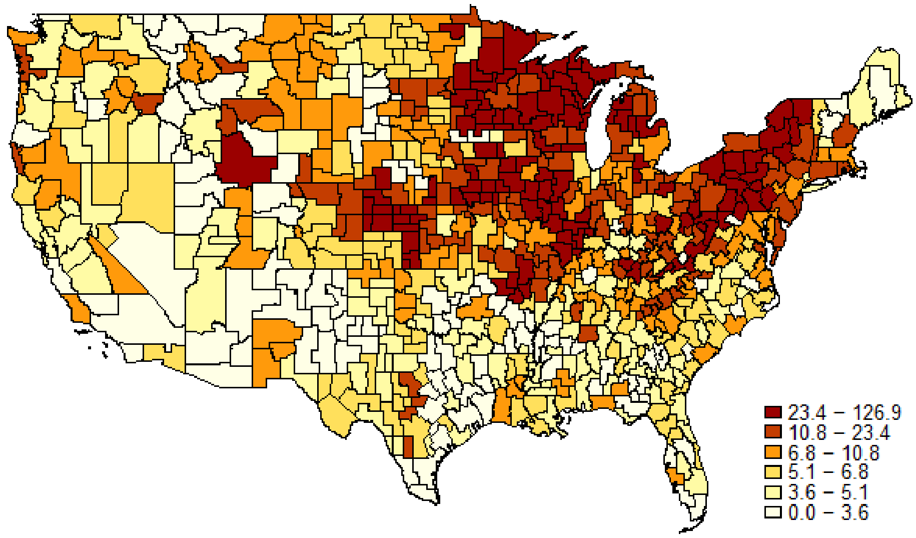
Notes: Figure (4.a) Presents a binned scatterplot of the IPUMS/Census incarceration rate against the Vera Institute incarceration rate. Figures (4.b) and (4.c) present the average and median differences between the Census rate and the Vera rate, respectively.

Figure (4.a) provides *prima facie* evidence that the Census incarceration measure is highly representative at the commuting zone level, indicating that the Census incarceration measure and the Vera incarceration measure are highly correlated across commuting zones. However, despite the strong, positive correlation between the two measures there are nonetheless differences between the Census incarceration estimates and the Vera incarceration estimates over time. For example, while Figure (4.b) indicates that the average difference between the two estimates was positive up until 2000, Figure (4.c) shows that the median difference has been negative over the entire time period in which both estimates were available. Further, while the average difference between the two measures is small relative to the sample average incarceration rate (the average difference over the whole sample is 23 people per 100,000), the median has been increasing in absolute value (becoming increasingly negative) over time. These results suggest that while the Census measure may underestimate the incarceration rate in most commuting zones, there are nonetheless large outliers in the tails of the distribution which drive the average up, possibly due to the location of state and federal prison facilities. In Section IV, we adopt two alternative weighting schemes as sensitivity checks on our estimation in order to address the differences in measured incarceration rates between the Vera data and the Census data. We find that these weights make little qualitative difference to our overall results. The combination of these tests with the strong *ex-ante* correlation between the Census data and the Vera data lead us to conclude that the Census data are acceptably representative at the commuting zone level.

Given that our estimates of commuting zone-level incarceration rates both match aggregate trends and can be externally validated using an alternative source of data on incarceration, what can we say about changes in the spatial pattern of incarceration over time? First, it is clear from Figure (2) that the geographic area that experienced the largest expansion in incarceration rates between 1950 and 2010 was the Southern United States—in particular, the gulf states including Texas. This pattern is not surprising. It is well known that the Civil Rights movement faced heavy political backlash in the South, a stronghold area of Jim Crow. At the national level, Kuziemko and Washington (2018) show that the exodus of Southern whites from the Democratic Party between 1958 and 1980 is entirely explained by the exodus of “racially conservative” whites following the Civil Rights initiatives of the 1960’s. Expansion of the prison population in these states is consistent with Alexander's (2010) description of mass incarceration as a response by local elites to a loss of social control.

However, despite higher *overall* incarceration rates in Southern states, *relative* rates of incarceration—the ratio of non-white incarceration to white incarceration—are higher in Northern and Midwestern states. Figure (5) maps the relative incarceration rate across commuting zones for the year 2010. This figure suggests that—to the extent that incarceration rates expanded in the South, they did so for both whites and non-whites. In contrast, expansion of the penal system in Northern states appears to have concentrated largely on non-white populations. Insofar as Alexander's (2010) hypothesis focuses on the disproportionate impact of the prison boom on people of color, Figures (4) and (5) suggest it is largely a Northern story. In Section IV, we attempt in greater detail to disentangle the factors that contribute to the differential geographic trends in incarceration by race.

Figure 5: Commuting Zone Map of Relative Incarceration Rates, 2010

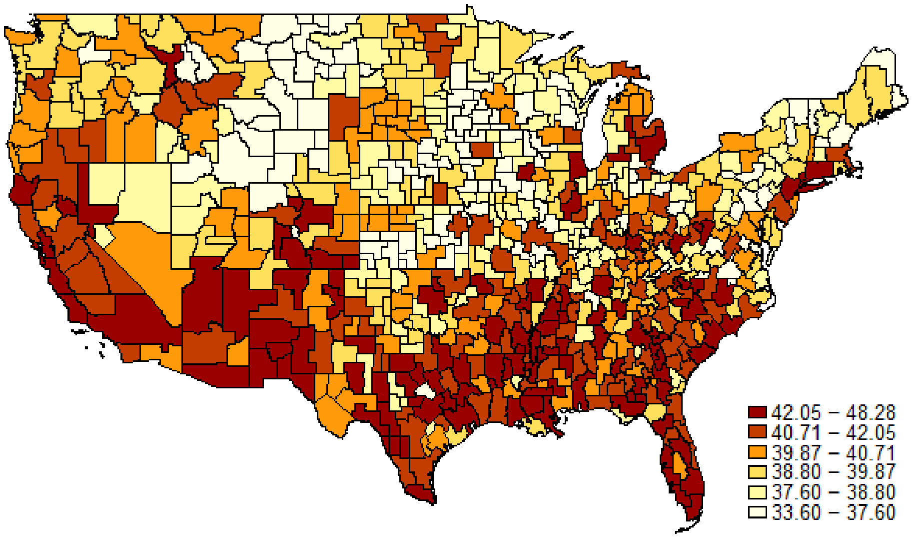


Notes: Figure plots the ratio of the non-white incarceration rate to the white incarceration rate at the commuting zone-level. Incarceration estimates constructed using the IPUMS/Census microdata. Commuting zone crosswalks obtained from Autor and Dorn (2013).

Local-level inequality measures

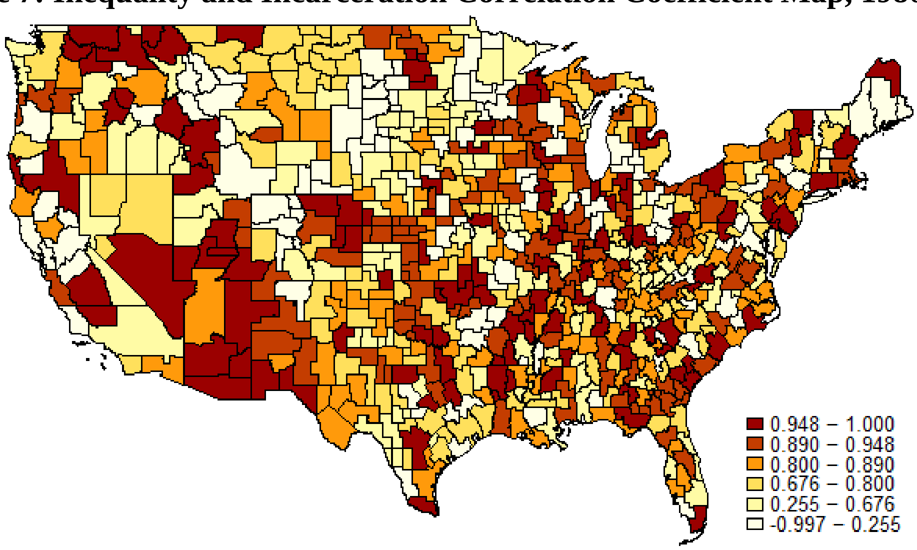
Commuting zone-level measures of inequality are constructed using IPUMS Census Data (Ruggles et al., 2019). For each commuting zone, we compute the Gini coefficient across households using total household income, which includes the total pre-tax personal income or losses from all sources for all persons residing in a given household over the previous year. Due to top-coding in the IPUMS data, we expect that our estimates of the Gini coefficient will understate the true extent of inequality. However, what is important for our analysis is not necessarily the level of inequality itself, but variation in inequality across commuting zones and over time, both of which should be preserved even in the presence of top-coding. Figure (6) presents a map of the Gini coefficient across commuting zones for the year 2010. The map shows a clear spatial pattern, indicating higher average levels of inequality in Southern commuting zones. Jointly, Figures (6) and (2.b) offer *prima facie* support for the idea that inequality and incarceration may be correlated across space, suggesting an overlap between high inequality and high incarceration rate commuting zones, especially in the South. Figure (7) formalizes this insight, plotting the sample correlation coefficient for inequality and incarceration in the period after 1970 for all commuting zones.

Figure 6: Commuting Zone Map of Gini Coefficient for Household Income, 2010



Notes: Figure plots the Gini coefficient for household income at the commuting zone-level. Gini coefficient estimates constructed using the IPUMS/Census microdata. Commuting zone crosswalks obtained from Autor and Dorn (2013).

Figure 7: Inequality and Incarceration Correlation Coefficient Map, 1980-2010



Notes: Figure plots the sample correlation coefficient for the Gini coefficient for household income and the overall incarceration rate at the commuting zone-level for the post-1970 period. Gini coefficient estimates constructed using the IPUMS/Census microdata. Incarceration estimates constructed using the IPUMS/Census microdata. Commuting zone crosswalks obtained from Autor and Dorn (2013).

Crime statistics

In order to evaluate the relationship between inequality and incarceration in a way that addresses Alexander's (2010) claim that mass incarceration arose as an attempt by privileged whites to re-establish social control after the fall of Jim Crow, it is important to interrogate alternative mechanisms that might explain the relationship between inequality and incarceration. The most obvious of these mechanisms is an increase in the rate of criminal activity in response to rising inequality. If rising incarceration rates are simply a response to an increase in criminal activity, there would appear to be little evidence for a stratification-based explanation of mass incarceration. Thus, in our analysis we include a measure of the violent crime rate as an additional control variable. We calculate the crime rate in each commuting zone as the number of violent crimes per 100,000 population, using commuting-zone data on the number of violent crimes from the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) Statistics (Kaplan 2019).

Additional local-level economic and demographic indicators

Sample means for the main variables of interest and control variables are presented in Table (1). Additional variables include commuting zone population shares by race, the share of residents in a commuting zone with at least a bachelor's degree, employment shares by industry (with industries defined according to the 1990 Census Bureau industrial classification scheme)[6], the unemployment rate, population, and decadal population growth. The table presents sample means for the entire 1950-2010 period.

Table 1: Sample Means, 1950-2010

Variable	Mean	Std. Deviation	Source
Incarceration Rate (Per 100,000)	426.19	493.96	IPUMS
White Incarceration Rate (Per 100,000)	249.22	298.28	IPUMS
Non-White Incarceration Rate (Per 100,000)	2,048.14	3,778.18	IPUMS
Crime Rate (Per 100,00) (1960-2010)	444.43	318.09	UCR
Gini Coefficient	35.9	5.13	IPUMS
White Poverty Rate (%)	12.4	8.6	IPUMS
Non-White Poverty Rate (%)	30.0	15.33	IPUMS
White Population Share (%)	79.85	15.3	IPUMS
Black Population Share (%)	10.04	8.73	IPUMS
Hispanic Population Share (%)	7.01	10.5	IPUMS
Asian Population Share (%)	2.06	3.2	IPUMS
American Indian Population Share (%)	0.45	1.43	IPUMS
Other Race Population Share (%)	0.58	0.85	IPUMS
Bachelor's Degree (%)	19.38	13.8	IPUMS

[6] To economize on space, we report sample means for only select industries. Full data available upon request.

Unemployment Rate (%)	4.5	2.2	IPUMS
Household Income	$47,939.22	19,153.06	IPUMS
Commuting Zone Population	2,823,229	3,798,615	BEA
Decadal Commuting Zone Population Growth (%)	13.9	15.7	BEA
Manufacturing Emp. Share (%)	19.8	10.08	IPUMS
Retail Emp. Share (%)	16.48	1.9	IPUMS
Agriculture Emp. Share (%)	4.06	6.2	IPUMS
N = 5,054			

Notes: Sample means weighted using commuting zone population.

B. Estimation Strategy

In order to assess: A) the impact of inequality on the post-1970 increase in incarceration across commuting zones, and B) whether non-white incarceration rates rose more in areas with larger concentrations of poor white households—in a fashion consistent with Alexander's (2010) description of mass incarceration as a "racial bribe"—we exploit variation in both incarceration rates and inequality across time and across commuting zones using a two-way fixed effects approach:

$$Incarceration_{ct} = \beta_1 * treat_c + \beta_2 * After_t^{1970} + \beta_3 * (treat_c \times After_t^{1970}) + \boldsymbol{X}_{ct}^T \beta + \lambda_c + \delta_t + \epsilon_{ct} \qquad (1)$$

where $Incarceration_{ct}$ is the incarceration rate in commuting zone *c* in year *t*, $treat_c$ is a continuous variable giving the average value of either the household income Gini coefficient or the ratio of white-to-non-white poverty for the post-1970 period, $After_t^{1970}$ is a dummy variable taking a value of 1 after 1970—which we identify as the start of the period of mass incarceration—and 0 otherwise. The regression coefficient on the interaction between $treat_c$ and $After_t^{1970}$—β_3—is therefore our parameter of interest, giving an estimate of the average impact of inequality on incarceration after 1970. The remaining variables are: $\boldsymbol{X}_{ct}^T$—a vector of controls, λ_c—a commuting zone-specific fixed-effect, δ_t—a year-fixed effect, and ϵ_{ct}—an idiosyncratic error term. In addition to (1), we estimate the following flexible specification which allows the treatment effect to vary by year:

$$Incarceration_{ct} = \beta_1 * treat_c + \delta_t + \sum_t (\delta_t \times treat_c) + \boldsymbol{X}_{ct}^T \beta + \lambda_c + \epsilon_{ct} \qquad (2)$$

where the interaction between δ_t and $treat_c$ gives the impact of a unit increase in the Gini coefficient or relative poverty rate on incarceration in year *t*.

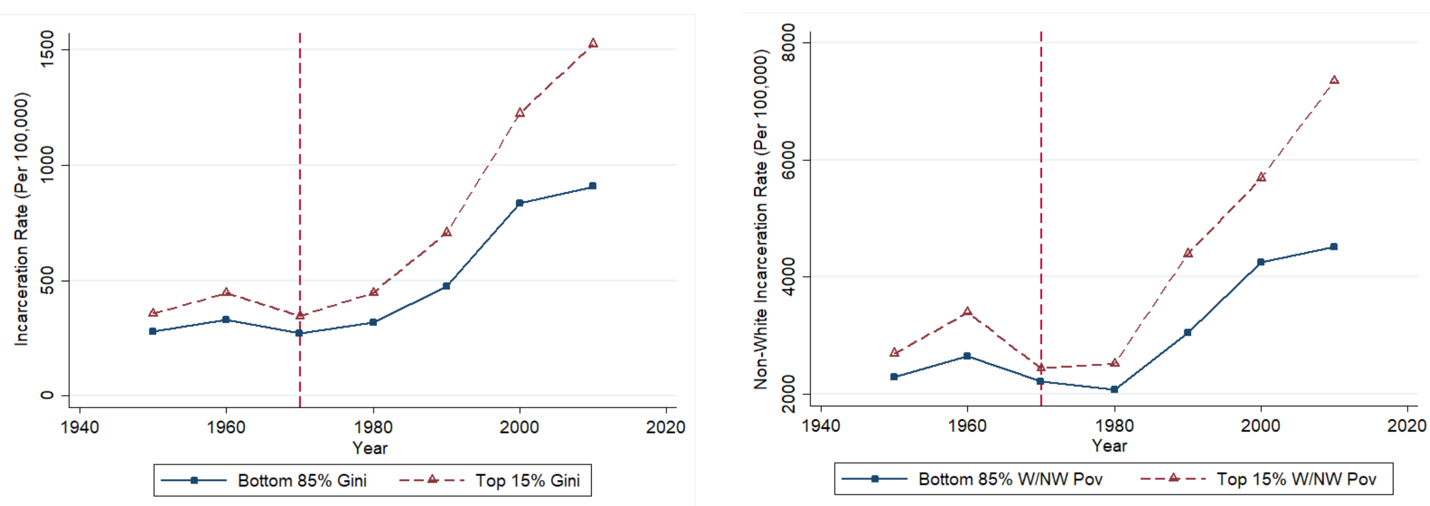
In addition to controlling for the impact of crime on incarceration as a possible channel through which inequality impacts prison populations, it is important to control for labor market outcomes that might otherwise be correlated with both inequality and incarceration. To address this concern the above specification adjusts for time-varying local levels of unemployment, education, income, population, population growth, and industrial composition, as well as demographic controls for population shares by race. Thus, although our treatment variable—average inequality in the post-1970 period—is not randomly assigned to commuting zones, we believe that we are able to adjust for most of the confounding backdoor paths that could bias our estimate of the impact of inequality on incarceration. Nonetheless, we are cautious about overemphasizing the causal nature of the estimates obtained from the above specification, emphasizing their reduced form nature and focusing on the bigger picture implied by the relationships we observe.

A requirement of the difference-in-differences-type specifications in Equations (1) and (2) is that the treatment and control groups satisfy the parallel trends assumption. This assumption requires that trends in the dependent variable across the treatment and control groups be parallel in the pre-treatment period. If this assumption is violated our estimate of β_3 will be biased. In Equation (2), this amounts to requiring that the interaction term between the inequality variable and year-fixed effect in pre-treatment years be statistically insignificant and close to zero. A formal test of the parallel trend assumption in the framework of Equation (1) can be implemented via a placebo test that restricts the treatment window to the pre-treatment period and assigns an alternate "placebo" treatment date. In Section IV, we implement both tests. Insight can be gained prior to formal hypothesis testing by plotting the outcome variable across treatment and control groups. Because we have a continuous variable, we divide the sample into "high treatment" and "low treatment" groups, where commuting zones in the top 15% of the distribution of the treatment variable are included in the former. Figure (8) plots trends in the overall incarceration rate as well as the non-white incarceration rate for "high" and "low" treatment values of inequality and relative poverty, respectively. The figure is suggestive of parallel trends across groups in the pre-treatment period, as well as being indicative of a divergence in outcomes across the two groups following the start of mass incarceration.

Figure 8: Incarceration Trend Plots by Level of Inequality and Relative Poverty

8(a) Incarceration by Gini

8(b) Non-White Incarceration by Ratio of White/Non-White Poverty



IV. Results

A. Inequality

Table (2) presents results from our initial estimates of Equation (1). We first look at the effect of overall inequality—as measured by the average commuting zone household income Gini coefficient in the post-1970 period—on commuting zone-level incarceration rates. Standard errors are clustered at the state level. All regressions include controls for population shares by race, total population, population growth, average household income, unemployment, average education, and employment shares by industry. Column (2) adds time-fixed effects. Column (3) adds both time- and commuting zone-fixed effects. Column (4) weighs observations by the average commuting zone population over the sample period. Column (5) adjusts for the violent crime rate. Columns (6) and (7) use an alternative weighting scheme based on the similarity between the IPUMS/Census incarceration- rate measure and the Vera Institute incarceration rate

Table 2: Estimation Results, Inequality and Overall Incarceration

	(1) $Incarceration$	(2) $Incarceration$	(3) $Incarceration$	(4) $Incarceration$	(5) $Incarceration$	(6) $Incarceration$	(7) $Incarceration$
$(Gini_c \times After_t^{1970})$	65.94^{***} (20.79)	94.85^{***} (28.05)	65.83^{***} (15.59)	61.98^{***} (14.80)	56.42^{***} (16.69)	33.16^{***} (11.96)	48.31^{***} (16.92)
$Crime_{ct}$	—	—	—	—	0.210^{**} (0.103)	0.114^{*} (0.059)	0.264^{**} (0.122)
N	5,054	5,054	5,054	5,054	4,332	4,320	4,296
Demographics	Y	Y	Y	Y	Y	Y	Y
Industry	Y	Y	Y	Y	Y	Y	Y
Economy	Y	Y	Y	Y	Y	Y	Y
CZ FE	N	N	Y	Y	Y	Y	Y
Year FE	N	Y	Y	Y	Y	Y	Y
Vera Weights 1	N	N	N	N	N	Y	N
Vera Weights 2	N	N	N	N	N	N	Y
Population Weights	N	N	N	Y	N	N	N

Notes: Standard errors in parenthesis, clustered at the state-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is incarceration rate (per 100,000). Inequality is measured by the commuting zone-level household income Gini coefficient. All regressions include controls for population shares by race, total population, population growth, average household income, unemployment, average education, and employment shares by industry. Column (2) adds time-fixed effects. Column (3) adds both time- and commuting zone-fixed effects. Column (4) weights observations by the average commuting zone population over the sample period. Column (5) adjusts for the violent crime rate. Columns (6) and (7) use alternative weighting schemes based on the similarity between the IPUMS/Census incarceration rate measure and the Vera Institute incarceration rate measure, where commuting zones with IPUMS/Census incarceration rate estimates closer in magnitude to the Vera Institute estimates are given higher weights.

measure, where commuting zones with IPUMS/Census incarceration rate estimates closer in magnitude to the Vera Institute estimates are given higher weights. The alternative weighting schemes are constructed as follows. In Column (6), observations are weighted according to the sample mean of the inverse of the absolute value of the difference between the IPUMS/Census incarceration rate estimate and the Vera Institute incarceration rate estimate. In Column (7), observations are weighted by the absolute value of the within-commuting-zone correlation between the IPUMS/Census measure and the Vera Institute measure. Note that neither of these weighting schemes require observations for the Vera Institute measure in all years, and thus allow for the inclusion of a wider range of commuting zones than would be available in the Vera data in any given year.

The results suggest a statistically significant positive relationship between inequality and the rate of incarceration after 1970. Averaging across all columns, a one-unit increase in the average post-1970 household income Gini coefficient in a commuting zone is associated with an approximately 61-point increase in the incarceration rate (an increase of 61 people per 100,000). For a commuting zone at the sample mean post-1970 incarceration rate of 684.94, this represents a nearly 10% increase. This result appears robust to the inclusion of a wide variety of controls, including commuting zone- and year-fixed effects. Importantly, this result is robust to the inclusion of the violent crime rate, which suggests that the increase in incarceration after 1970 was not simply a rational response to rising crime. Holding the crime rate constant, an increase in commuting zone inequality nonetheless translates into higher incarceration rates after 1970. However—consistent with the Neoclassical approach—the commuting-zone level crime rate has a statistically significant, positive effect on the incarceration rate.

Even if we consider the specification with the smallest estimated effect magnitude (33.16)—Column (6), which accounts for differences in measured incarceration (due to the recorded location of prisoners) between the Census data and the Vera Institute data—the impact of inequality on incarceration is nonetheless highly statistically and economically significant, suggesting the positive relationship between inequality and incarceration is not driven merely by measurement error in the Census incarceration data.

Table (3) extends our estimates of Equation (1) over different race and ethnicity categories. We focus on differences in our parameter estimates across white non-Hispanic, non-white non-Hispanic, Hispanic, and Black non-Hispanic race/ethnicity groupings.

Table 3: Estimation Results, Inequality and Incarceration by Race

	(1)	(2)	(3)	(4)
	$White, Non - Hispanic$	$Non - White, Non - Hispanic$	$Hispanic$	$Black, Non - Hispanic$
$(Gini_c \times After_t^{1970})$	26.76***	429.4**	91.34	418.4
	(9.122)	(167.3)	(109.5)	(330.8)
$Crime_{ct}$	0.0663	0.392	0.570	-0.817
	(0.0432)	(0.545)	(0.443)	(1.267)
$White_{ct}$	-9.030**	22.34	30.13	29.16
	(4.241)	(80.16)	(26.07)	(105.0)
N	4,332	4,332	4,280	4,303
Demographics	Y	Y	Y	Y
Industry	Y	Y	Y	Y
Economy	Y	Y	Y	Y
CZ FE	Y	N	Y	Y
Year FE	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is incarceration rate (per 100,000) by race/ethnicity category. Inequality is measured by the commuting zone-level household income Gini coefficient. All regressions include controls for the white population share, total population, population growth, average household income, unemployment, average education, and employment shares by industry.

The results in Table (3) suggest an unit increase in the average household income Gini coefficient in the post-1970 period has a larger impact on non-white, non-Hispanic incarceration than white incarceration, where the magnitudes of the coefficients suggest the bulk of the effect is driven by the impact of inequality on incarceration of black individuals. A unit increase in the average post-1970 Gini results in an increase in incarceration of approximately 429 persons per 100,000 for non-white, non-Hispanic individuals. The coefficient for black, non-Hispanic individuals is of similar magnitude—although less precisely estimated. In contrast, the same increase in the Gini coefficient increases the incarceration rate for white individuals by only 27 persons per 100,000, an effect smaller in magnitude than the average overall effect estimated in Table (2). These results add to the growing body of work suggesting that the *social* consequences of inequality are not borne equally across race/ethnicity groups, with emphasis on the negative social impact of economic inequality on minorities and people of color.

In addition to the main results presented in Table (3), it is worth drawing attention to the estimated regression coefficients on the crime rate and white population share. In every specification the coefficient on the crime rate variable is positive but statistically insignificant, weakly consistent with the neoclassical hypothesis. Second, the white population share variable has a statistically significant negative impact on white, non-Hispanic incarceration rates. This result suggests that commuting zones where a large share of the population is white incarcerate white individuals at lower rates. In contrast, the effect of the white population share is positive, but statistically insignificant, for all non-white groups[7]. Thus, the racial composition of a commuting zone appears to have an independent effect on incarceration. In the next sub-section, we extend this dimension of the analysis to explore whether or not differences in the economic standing of various race/ethnicity groups have differential impacts on incarceration rates by race in a fashion consistent with Alexander (2010)'s description of tough on crime policies as a "racial bribe."

B. Relative poverty

Table 4: Estimation Results, Relative Poverty and Incarceration by Race

	(1)	(2)	(3)	(4)
	$White, Non - Hispanic$	$Non - White, Non - Hispanic$	$Hispanic$	$Black, Non - Hispanic$
$(RelPov_c \times After_t^{1970})$	-0.577	35.55***	-11.93	76.10***
	(0.874)	(8.588)	(8.865)	(21.83)
$Crime_{ct}$	0.0868*	0.865	0.609	-0.203
	(0.0787)	(0.553)	(0.446)	(1.337)
$White_{ct}$	-10.68**	2.603	22.92	16.48
	(4.558)	(81.10)	(29.09)	(99.19)
N	4,332	4,332	4,280	4,303
Demographics	Y	Y	Y	Y

[7] The interpretation in this coefficient is an increase in the white population share *relative to the omitted category*—in this case, all non-white groups. When additional population shares by race are included in the regression, such that the omitted category shifts to black, non-Hispanics, the effect of the white population share on the incarceration rate of non-white, non-Hispanics becomes statistically significant.

Industry	Y	Y	Y	Y
Economy	Y	Y	Y	Y
CZ FE	Y	N	Y	Y
Year FE	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is incarceration rate (per 100,000) by race/ethnicity category. $RelPov_c$ measures the white poverty rate as a percentage of the non-white poverty rate. All regressions include controls for the white population share, total population, population growth, average household income, unemployment, average education, and employment shares by industry.

Table (4) presents regression coefficients from estimating Equation (1) using relative poverty—measured as the ratio of the white poverty rate to the non-white poverty rate in a given commuting zone—as the variable of interest. Specifically, $RelPov_c$ measures the white poverty rate as a percentage of the non-white poverty rate, such that a unit increase in $RelPov_c$ implies a one-percent increase in the white poverty rate relative to non-white poverty. The results in Columns (1) and (3) of Table (4) indicate that as the white poverty rate increases—as a percentage of the black poverty rate—there is no effect on post-1970 incarceration for either white, non-Hispanic or Hispanic groups. In contrast, an increase in white relative poverty is associated with statistically significant, positive increases in both non-white, non-Hispanic incarceration rates and black, non-Hispanic incarceration rates. A one-percent increase in the white relative poverty rate after 1970 corresponds to an increase in the incarceration rate of 35.55 points (35.55 persons per 100,000) for the entire non-white, non-Hispanic group, and an increase of 76.10 points (76.1 persons per 100,000) for black, non-Hispanics. This result suggests that the spatial incidence of mass incarceration for non-whites is greater in local labor markets with a larger *relative* concentration of poor whites—the group which Alexander (2010) describes as a target of a "racial bribe" intended to disrupt working class political coalitions after the demise of Jim Crow. While this result does not provide direct confirmation of Alexander's (2010) hypothesis, it adds to the body of circumstantial evidence in support, indicating that incarceration of non-whites expanded more in areas with higher relative white poverty rates.

C. Additional sensitivity checks

We begin the sensitivity analysis by extending the main specification to Equation (2), which allows year-by-year interactions between year fixed-effects and the variable of interest. This specification allows for flexible dynamics in measuring the impact of inequality and/or relative poverty on incarceration in the post-1970 period by varying the treatment effect by year. Table (5) presents the results. We report results from specifications including the crime rate variable, although we also run each of the regressions without it. We find no meaningful difference in the estimated regression coefficients on our variables of interest in either case. Columns (1)-(4) present results using average post-1970 Gini coefficient as the variable of interest. Columns (5)-(7) present results using the average post-1970 ratio of the white poverty rate to the non-white poverty as the variable of interest.

Table 5: Estimation Results, Year-by-Year Effects

	(1) *Overall*	(2) *White, Non − Hispanic*	(3) *Non − White, Non − Hispanic*	(4) *Black, Non − Hispanic*	(5) *White, Non − Hispanic*	(6) *Non − White, Non − Hispanic*	(7) *Black, Non − Hispanic*
$(Gini_c \times \delta_{1970})$	3.790 (18.91)	4.275 (13.73)	-99.43 (235.6)	411.4 (391.6)	—	—	—
$(Gini_c \times \delta_{1980})$	0.734 (14.94)	5.966 (11.67)	150.9 (159.2)	493.7* (289.5)	—	—	—
$(Gini_c \times \delta_{1990})$	36.57* (18.71)	18.43 (14.60)	369.7* (216.7)	647.4 (463.6)	—	—	—
$(Gini_c \times \delta_{2000})$	116.7*** (35.49)	51.78** (21.44)	517.0 (310.0)	715.6 (525.8)	—	—	—
$(Gini_c \times \delta_{2010})$	148.5*** (36.44)	61.49*** (19.63)	628.5** (304.3)	834.3 (538.0)	—	—	—
$(RelPov_c \times \delta_{1970})$	—	—	—	—	-0.458 (0.790)	0.0268 (14.25)	-4.602 (24.29)
$(RelPov_c \times \delta_{1980})$	—	—	—	—	-1.051 (0.807)	2.038 (12.98)	1.478 (24.64)
$(RelPov_c \times \delta_{1990})$	—	—	—	—	-0.586 (1.166)	44.13** (20.57)	77.71 (48.27)
$(RelPov_c \times \delta_{2000})$	—	—	—	—	-0.429 (1.413)	43.88*** (15.47)	121.5*** (34.76)
$(RelPov_c \times \delta_{2010})$	—	—	—	—	-1.252 (1.402)	57.30*** (18.35)	104.3** (42.68)
N	4,332	4,332	4,332	4,303	4,332	4,332	4,303
Demographics	Y	Y	Y	Y	Y	Y	Y
Industry	Y	Y	Y	Y	Y	Y	Y
Economy	Y	Y	Y	Y	Y	Y	Y
CZ FE	N	N	Y	Y	Y	Y	Y
Year FE	N	Y	Y	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is incarceration rate (per 100,000) by race/ethnicity category. All regressions include controls for the white population share, total population, population growth, average household income, unemployment, average education, violent crime rate, and employment shares by industry.

Table (5) supports the results found previously: namely, higher post-1970 inequality is associated with greater incarceration rates across all race/ethnicity groups (although the effect is statistically insignificant when black, non-Hispanics and Hispanics are considered separately), but greater relative poverty for whites is only statistically significantly associated with incarceration for non-whites, and for black, non-Hispanics in particular. The results in Table (5) also shed new light on the temporal dimension of the relationship between inequality, poverty, and mass incarceration. The estimates suggest that the effect of local labor market inequality on incarceration occurs predominantly after 1980. This finding is consistent with the overall trajectory of incarceration depicted in Figure (1) which—despite indicating an initial uptick in incarceration between 1970 and 1980—illustrates that the lion's share of the increase in incarceration in the post-1970 period occurs between 1980 and 2010.

Finally, Table (5) lends initial support for the parallel trends assumption required for difference-in-differences. For difference-in-differences estimates to be valid, trends in the outcome variable must be parallel across treated and untreated groups in the period prior to the treatment. In this case, with a continuous treatment indicator variable (local labor market inequality) what is required is that trends in incarceration be the same across commuting zones for varying levels of inequality for all years prior to 1970. In Table (5), this amounts to requiring the interaction term featuring the 1970 year-fixed effect and the variable of interest be statistically insignificant and close to zero. This condition is satisfied across all columns. The regression coefficient on the variable of interest is statistically insignificant and small in magnitude in all cases when interacted with the 1970 year-fixed effect. However, in order to test the parallel trends assumption more formally, we nonetheless implement an additional placebo test across "high" and "low" treatment groups.

Table (6) presents the results of the placebo test. To implement the test, we separate the sample into "high" and "low" treatment groups—commuting zones in the top 15% of the sample distribution for inequality or relative poverty are counted as "high" treatment, as in Figure (8)—and then re-estimate Equation (1) using a placebo treatment date (1960), restricting the sample to the period prior to the treatment (all years before—and including—1970).

Table 6: Placebo Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$Overall$	$White, Non-Hispanic$	$Non-White, Non-Hispanic$	$Black, Non-Hispanic$	$White, Non-Hispanic$	$Non-White, Non-Hispanic$	$Black, Non-Hispanic$
$(HighGini \times After_t^{1960})$	28.42 (46.76)	63.57 (38.45)	157.2 (455.4)	589.2 (1271.5)	—	—	—
$(HighPov \times After_t^{1960})$	—	—	—	—	46.06 (61.26)	494.1 (866.8)	1342.0 (1999.6)
N	2,166	2,166	2,153	2,075	2,166	2,153	2,075
Demographics	Y	Y	Y	Y	Y	Y	Y
Industry	Y	Y	Y	Y	Y	Y	Y
Economy	Y	Y	Y	Y	Y	Y	Y
CZ FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is incarceration rate (per 100,000) by race/ethnicity category. All regressions include controls for the white population share, total population, population growth, average household income, unemployment, average education, and employment shares by industry.

The parallel trends assumption is satisfied in every case. For both the inequality treatment and the relative poverty treatment the trend in incarceration for the high treatment group is statistically insignificantly different from the low treatment group in the period prior to the prison boom.

A final robustness check concerns the degree to which selection on unobservables influences our coefficient estimates. Recognizing that assignment into the treatment category (i.e., the level of inequality) is potentially non-random, identifying the causal effect of mass incarceration on commuting zones of differing levels of inequality will—at minimum—require parallel trends in the pre-treatment period. The results in Table (6) suggest this requirement is satisfied. However, we can further assess the degree to which selection on unobservables influences our results by applying the test developed by Oster (2019).

Oster (2019) shows that information about the degree of selection on *observables* can be used to bound coefficient estimates in the presence of selection on unosbervables. In particular, Oster (2019) shows that under the assumption of equal selection—that the degree of selection on unobservables is proportionate to the degree of selection on observables—the bias-adjusted regression coefficient is given by:

$$\beta_{bias\ adjusted} = \beta_{long} - (\beta_{short} - \beta_{long}) \frac{R^2_{\max} - R^2_{long}}{R^2_{long} - R^2_{short}}$$

Where β_{long} and R^2_{long} are the regression coefficient and R-squared estimate obtained from a regression including a full battery of controls, β_{short} and R^2_{short} are the regression coefficient and R-squared estimate from a restricted regression including only the inequality terms, and R^2_{max} is the maximum R-squared. Oster (2019) suggests $R^2_{\max} = 1.3 \times R^2_{long}$ as a conservative estimate for R^2_{max}, as only 45% of non-randomized results surveyed in that work survive this threshold.

Table 7: Oster (2019) Bias-Adjusted Coefficients

Inequality on Overall Incarceration	Inequality on Non-White, Non-Hispanic Incarceration	Relative Poverty on Non-White, Non-Hispanic Incarceration
$\beta_{bias\ adjusted}$ = 58.79	$\beta_{bias\ adjusted}$ = 462.49	$\beta_{bias\ adjusted}$= 34.70
Original Estimate: Table (2), Column (3)	Original Estimate: Table (3), Column (2)	Original Estimate: Table (4), Column (2)

Table (7) presents the bias-adjusted coefficients for three of our key results: the effect of inequality on overall incarceration, the effect on non-white, non-Hispanic incarceration, and the effect of relative poverty on non-white, non-Hispanic incarceration. The adjusted coefficients suggest that selection on unobservables is not a primary driver of our results. The estimated effect of inequality on overall incarceration and the effect of relative poverty on non-white, non-Hispanic incarceration are only slightly attenuated. In contrast, the effect of inequality on non-white, non-Hispanic incarceration is slightly elevated. Taken in conjunction with the results in Table (6), Table (7) suggests our estimates are robust to concerns about selection on unobservables.

V. Discussion and Conclusion

In this article, we explore the extent to which labor market inequality is associated with incarceration at the local level between 1950 and 2010. Using IPUMS/Census microdata to construct estimates of the commuting zone incarceration rate—verifying our estimates with recently released data on incarceration by county of admission from the Vera Institute of Justice—we examine the impact of inequality on the overall incarceration rate in a commuting zone, as well as the varying impact of inequality on incarceration by race. While income inequality is associated with higher rates of incarceration for all race and ethnicity groups (although not always in statistically significant fashion), the effect is largest for non-white, non-Hispanic individuals. Further, when we examine the relationship between relative poverty and incarceration, we find a strong, positive relationship between white relative poverty (measured as the ratio of the white poverty rate to the non-white poverty rate) and non-white incarceration, particularly of non-Hispanic blacks. However, we find no relationship between white relative poverty and white incarceration. Our results have several important implications.

First, our results are broadly consistent with Alexander's claims in *The New Jim Crow* that mass incarceration and the "War on Drugs" arose following the Civil Rights Movement as attempts by white elites to re-establish the system of racialized social control that was disrupted when Jim Crow was dismantled. The positive relationship between inequality and incarceration fits within the theoretical framework for understanding racial inequality coming out of stratification economics (Darity, 2005; Darity, Hamilton, and Stewart, 2015), which suggests that race- and class-based discrimination represent a rational attempt by privileged groups to maintain their relative status and the material benefits which that status confers. To the extent that rising economic inequality confers greater rents to people in positions of privilege, local elites have an incentive to increase their stratification efforts—including via more restrictive penal policy. Further, the positive relationship between local incarceration rates for non-whites and white relative poverty is consistent with Alexander's (2010) argument that at least one of the political motivations for the rise of tough on crime policy after the Civil Rights Movement was to function as a racial bribe that attracted poor whites away from newly formed, otherwise diverse, working class political coalitions.

Second, our results speak to the uneven social burden of rising economic inequality and the importance of positional concerns in determining who bears that burden. Although both whites and non-whites experienced an increase in incarceration as a result of rising inequality during the post-1970 prison boom, the magnitude of that increase—and the impact of inequality as a mechanism determining the incidence of that increase—was disproportionately born by racial and ethnic minorities. To the extent that our results on the relationship between relative poverty and non-white incarceration—although reduced form—reflect behavior motivated by race-based positional concerns on behalf of poor whites, they suggest that: (1) Comparisons across groups may matter as much for positional concerns as comparisons across individuals, and (2) Robert Frank's (2005) claim that "positional externalities cause large and preventable welfare losses" is an understatement, given that additional group-based social multiplier effects are involved. Our results are also consistent with other new research on the possible far-reaching effects of perceived social status changes. For example, Siddiqi, Sod-Erdene, Hamilton, Cottom, and

Darity (2019) relate rising white mortality rates to changing perceptions regarding relative group status and concomitant changes in psychological and physiological stress.

Finally, our results suggest links between economic inequality, race, and the criminal justice system, offering important insight for both those who would undertake future criminal justice reform efforts and those who work on aggregate economic policy. Our results therefore are relevant for the starting point discussions initiated with the First Step Act of 2018 and for the importance of continued "steps" toward effective and equitable prison and criminal justice reforms. We argue here that heterogeneities affect the intersections of economic inequality and prison outcomes. Reforms that aim to reverse course on mass incarceration are unlikely to have long-term success in the absence of actions that (a) assuage the identity-driven positional tensions which led poor whites to lend their support to the policies and politicians initially responsible for mass incarceration, and (b) address inequality so as to eliminate the rents associated with positions of racial and economic privilege. As long as hierarchies based on race and economic status confer benefits to those at the top, there will be an incentive for individuals to support institutions that maintain and enforce the existing pattern of resource allocation. Rather than issues to be addressed separately, economic inequality and racial inequality—including racial disparities in the criminal justice system—must be treated as part of the same problem.

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