2026

The Economic Losses from Arrest and Conviction Records

A Research Report from The Clean Slate Initiative

Researched and Authored by Elizabeth Johnson, Data Scientist

THE CLEAN SLATE INITIATIVE

The Economic Losses from Arrest and Conviction Records

A Research Report from The Clean Slate Initiative

Researched and Authored by Elizabeth Johnson, Data Scientist

Table of Contents

Executive Summary

This report quantifies the economic consequences of arrest and conviction records in the United States, focusing on individuals who experienced legal system contact in early adulthood[1] and remained crime-free for over a decade.

Using data from the National Longitudinal Survey of Youth 1997 (NLSY97) and a rigorous propensity score matching methodology, we estimate how records are associated with lower employment and earnings over time.

Our analysis shows that arrest and conviction records are associated with substantial and persistent reductions in earnings. Even after accounting for observable differences between individuals with records and those without legal system involvement, significant economic penalties remain across all record types and are particularly large for those with felony conviction records.

At the individual level, expected annual earnings are reduced by:

$4,400/yr

14.9%

for people with arrest or non-conviction records

$6,400/yr

17.8%

for people with misdemeanor convictions

$9,300/yr

31.8%

for people with non-violent felony convictions

$9,800/yr

36.7%

for people with violent felony convictions

These penalties persist throughout prime working years (ages 26–40) and accumulate into substantial losses. Over this fourteen-year period, individuals lose an estimated:

$72,000

arrest or non-conviction records

$102,000

misdemeanor conviction records

$150,000

non-violent felony conviction records

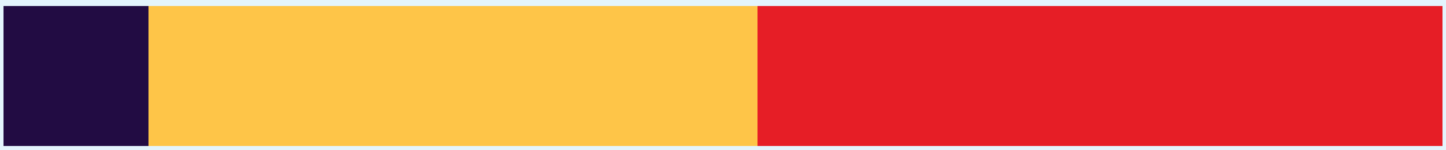
$159,000

violent felony conviction records

These penalties are likely to continue accumulating beyond age 40 and may have long-term consequences. Reduced earnings during a person's prime working years limit their ability to accumulate assets, invest in education or training, build retirement savings, and support family members. These effects can compound over time, extending beyond the individual and shaping family stability and generational wealth.

Individual losses translate into major economic impact as 1 in 3 adults have an arrest or conviction record in the United States. We estimate that arrest and conviction records are associated with

$440 billion in lost annual earnings nationwide.



$44.7 billion
People with
arrest or non-conviction records

$187.7 billion
People with
misdemeanor conviction records

$208.2 billion
People with
felony conviction records

These losses are not evenly distributed. They vary across regions, with the South accounting for the largest share due to population size, and the West contributing disproportionately due to higher wages on average. **Disparities are also evident across race and sex: Black individuals and women with records experience larger relative earnings penalties compared to their peers without legal involvement, compounding existing labor market inequalities.**

> The findings underscore that economic exclusion linked to arrest and conviction records is not limited to incarceration or conviction. **Even non-conviction records, where no guilt was established, are associated with meaningful and lasting financial harm.**

From a policy perspective, these results highlight the importance of reducing barriers to employment for people with records with as short a waiting period as possible because even after more than a decade of remaining crime-free, people are being denied meaningful access to opportunities and continue to be punished economically.

Clean Slate automated record sealing policies offer a scalable mechanism to mitigate these losses by limiting public access to old records so that people no longer face significant barriers to opportunities such as education, housing, and employment. While such policies cannot eliminate all structural barriers, they represent a critical step toward improving labor market access, increasing economic mobility, and strengthening the broader economy.

Introduction: How Records Create Economic Barriers

Around 70 to 100 million (1 in 3) adults in the United States have an arrest or conviction record.[2]

As a result, contact with the legal system is not just common, it plays a major role in shaping the nation's workforce. Today, employers routinely use background checks to screen applicants,[3] and an arrest or conviction record can become a lasting barrier to employment. Even long after someone has completed their sentence and remained crime-free, it can continue to limit access to opportunities, not just for individuals, but also their families.[4]

The consequences of these barriers are well documented. Research shows that employers are significantly less likely to hire individuals with records, including those with only arrest or non-conviction histories.[5] Moreover, having a record can reduce job interview callbacks by 50 percent, regardless of qualifications or experience.[6] These patterns restrict economic mobility and limit access to stable employment, with effects that extend beyond individuals to their families and communities.[7] Reduced earnings erode household stability, weaken intergenerational mobility, and diminish local economic activity.

These obstacles occur in an increasingly tight labor market, where the labor force participation rate has been declining for over two decades, and many industries and regions continue to experience persistent worker shortages.[8] Given these circumstances, the systematic marginalization of millions of working-age individuals possessing arrest or conviction histories is more than an obstacle to individual growth; it represents a significant inefficiency in the national labor market.

At scale, the widespread exclusion of tens of millions of adults from full participation in the labor market is a substantial drag on national economic growth. The Center for Economic Policy Research found that the national economy loses about $78 to $87 billion in annual GDP due to employment barriers faced by people with felony convictions.[9] The Brennan Center for Justice extends their analysis to include people with misdemeanor conviction records, finding that $372.3 billion of earnings is lost annually by those with convictions experienced early in life.[10] Both of these reports likely underestimate the true extent of economic losses associated with records and system involvement because they do not capture the hardships of those with arrest or non-conviction records.

Using data from the National Longitudinal Survey of Youth 1997 (NLSY97), we examined the long-term earning trajectories for people with old arrest or conviction records from their early adulthood and focused specifically on those who have remained crime-free for over a decade. We then captured the earnings losses associated with arrest or conviction records by comparing these individuals to statistically similar people without any legal involvement. This report offers a more comprehensive and policy-relevant estimate of the economic losses associated with legal system involvement and arrest or conviction records by expanding the scope of analysis beyond felony convictions to include the full spectrum of arrest and convictions records.

Arrest and Conviction Records Close Doors to Employment Opportunities

94% of employers use background checks to screen out applicants with records.

50% reduction in job interview callbacks by having a record, regardless of qualifications or experience.

Limited access to jobs means greater likelihood of:

- Decreased household stability
- Weakened intergenerational mobility
- Diminished local economic activity

The Clean Slate Initiative (CSI) defines a Clean Slate policy as one that automates record sealing upon eligibility of the record, includes arrest and misdemeanor conviction records among those eligible, and strongly recommends that the law include at least one felony record.[11] As an organization, we are committed to passing and implementing policies that effectively restrict access to eligible records across all relevant criminal history databases to ensure credit reporting agencies and state or federal governments produce accurate background checks.

Clean Slate automated record sealing policies have the potential to reduce barriers to employment and reconnect millions of individuals into the formal labor market on an unprecedented scale — evidenced by results in states where the policies have been effectively implemented.

> In Pennsylvania, **1,665,000** people have had their record fully or partially sealed through the state's Clean Slate policy, while Michigan and Connecticut have fully or partially sealed records for **912,000** and **171,000** people, respectively.[12]

While Clean Slate policies may not entirely counteract deep-seated structural inequities or the lasting repercussions of legal system contact, they are vital for enhancing economic mobility and labor force involvement. In a labor market where employers face ongoing hiring challenges, reducing unnecessary barriers to employment is not only a matter of fairness, but also a strategy for strengthening the nation's workforce and supporting long-term economic growth.

Individual Losses from Arrest and Conviction Records

This study estimates the differences in annual workforce participation and earnings for people with old arrest or conviction records from early adulthood[13] that have remained crime-free for over a decade.

By using data from the National Longitudinal Survey of Youth 1997 (NLSY97), we categorized survey respondents into four hierarchical record types based on the most severe offense a person self-reported being arrested and/or convicted of. These categories include arrest or non-conviction, misdemeanor conviction, non-violent felony conviction, and violent felony conviction.

We then captured the difference in workforce participation and earnings from people's prime working years, between the ages of 26 and 40, associated with these arrest or conviction records by comparing these individuals to statistically similar people without any legal involvement and arrest or conviction record. For more information on the methods used in this report please refer to the Methodology section.

This study finds that people with arrest or conviction records experience decreased workforce participation and lower annual earnings after arrest or conviction compared to their peers without legal system involvement. These earnings losses persist throughout one's prime working years and accumulate into substantial losses despite remaining crime-free.

> I was working at a job for a temp agency for over eight months when I was encouraged to apply to fill the role full-time. After submitting my application, they fired me on the spot because the background check revealed my decades-old record. I made a mistake in 1992, and it's still affecting my employment opportunities to this day — even though I've been out of trouble ever since.
>
> **— Kimberly, Oklahoma**

WORKFORCE PARTICIPATION

While existing literature confirms that arrest and conviction records create employment obstacles and diminish job stability, our findings reinforce the barriers individuals must overcome to achieve the economic independence every person deserves.

We found that people with arrest or conviction records have lower rates of employment[14] than people without records or legal involvement in almost all cases. **Those with felony conviction records experience larger and more severe differences in employment** than those with arrest or non-conviction and misdemeanor conviction records.

Of the people with records that are employed, we found that regardless of record severity, people are working fewer weeks in a year than their peers without records. Again, **people with felony conviction records are losing the most work (over a month and a half)**, while those with arrest or non-conviction and misdemeanor conviction records lose nearly a month of work compared to peers without records.

Annual earnings are shaped by an individual's employment status and amount of work completed. These two factors vary between people with and without arrest or conviction records. This contextualization helps clarify the challenges faced by individuals with records that are often intensified by the prevalence of background checks and the existence of over 42,000 federal, state, and local laws that create legal barriers for individuals with records, especially in employment.[15]

On average, 84.2% of people with arrest or non-conviction records were employed in a given year compared to 85.7% of their peers without a record. While this difference in employment (-1.5 percentage points) is minimal, it is statistically significant[16] and suggests barriers to employment among these two groups may differ, despite no guilt being established in one's case. Those employed with arrest or non-conviction records worked 3.8 weeks, or nearly one month, less in a given year compared to their peers.

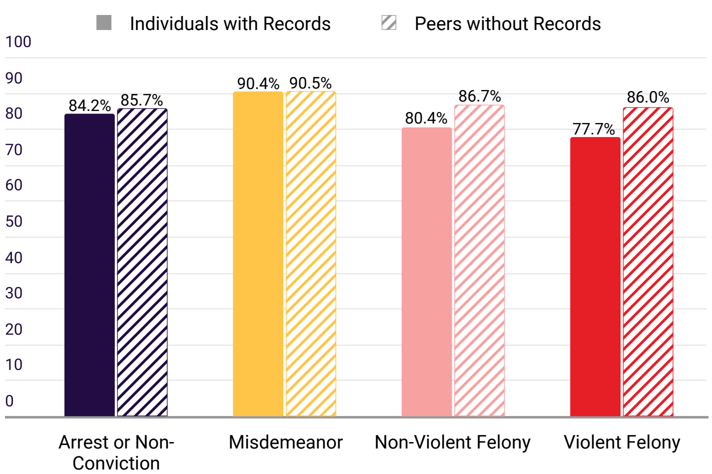
At first glance, employment rates are nearly identical for people with misdemeanor convictions (90.4%) and their peers without records (90.5%). But matched comparisons reveal a different pattern: people with misdemeanor convictions were, on average, 1.9 percentage points less likely to be employed during their prime working years (Appendix Table 23, page 53). Among those employed, they also worked just over three fewer weeks per year than their matched peers.

People with felony convictions face the most severe differences in workforce participation compared to their peers without records. Employment rates for those with non-violent and violent felony conviction records were the lowest among all record types, 80.4% and 77.7%, respectively. Additionally, the difference between these groups and their peers without records were the most severe, -6.3 and -8.3 percentage points, respectively.

All people with records, except those with misdemeanor convictions, had lower average annual employment rates than their peers. People with misdemeanor conviction records also had higher employment rates than those with arrest or non-conviction records. This may reflect unobserved differences across the samples, such as post-conviction sentencing. For example, employment requirements for people on probation could contribute to higher employment rates among those with convictions compared to those whose record did not carry the same requirements.

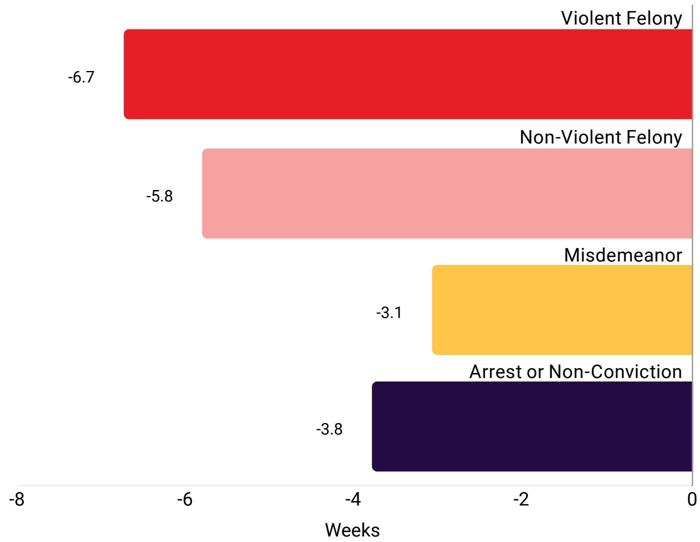
Average Annual Employment in Prime Working Years

For People with an Arrest or Conviction Record and their Peers without a Record



Difference in Weeks Worked Among Employed People During Prime Working Years

For People with an Arrest or Conviction Record and their Peers without a Record



Workforce participation outcomes also varied by race and sex. We found that **Black people with records had lower employment rates than white and Hispanic people across every record type**. Across all race groups, people with misdemeanor convictions were employed at higher rates than those with arrest or non-conviction records. However, white people with non-violent felony conviction records also had higher employment rates than white people with arrest or non-conviction records, a trend that does not extend to Black and Hispanic people. As hypothesized above, this could be due to differences in post-conviction sentencing type and its associated requirements, if any. These differences could be more pronounced for white people because of pre-existing racial inequities in sentencing that favors the use of probation for white people, as opposed to people of color with similar convictions.[17]

Employed Black people with records also worked fewer weeks in a year compared to employed white and Hispanic people with the same record types. Although white workers with records were more likely to be employed during a given year than Hispanic workers with similar records, employed Hispanic workers tended to work more weeks per year than employed white workers.

These patterns suggest important differences in workforce participation across racial groups, but additional research is needed to better understand the factors driving them, including differences in employer practices and labor market experiences. Because the sample sizes for Hispanic respondents in our study are small, and because some estimates are less stable, the Hispanic subgroup results should be interpreted with caution and should not be overgeneralized without further analysis.[18]

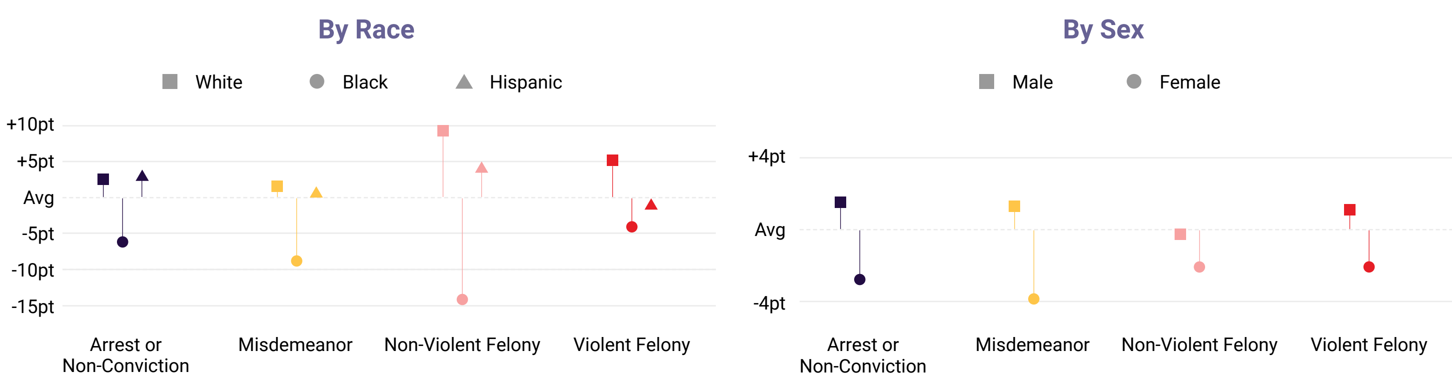
In the United States, women have historically participated in the labor force at lower rates than men.[19] The employment differences observed in our study across male and female respondents with arrest or conviction records underscores this historic national trend.

This study uses the sex variable available in the NLSY97, which identifies respondents as male or female. We recognize that sex and gender are distinct. Because the survey does not provide gender identity measures for this analysis, we use sex-based categories while acknowledging that respondents may identify with any gender.

Across all record categories, female respondents had lower employment rates and worked fewer weeks per year than male respondents with similar records. These disparities in employment appear to be so substantial that men with records often experience better employment outcomes than women who have never been arrested or convicted. In nearly every category, with the exception of violent felony conviction records, men with records were employed at higher rates than women without records. This highlights the need to better understand how arrest and conviction records intersect with sex and gender based inequities in employment.

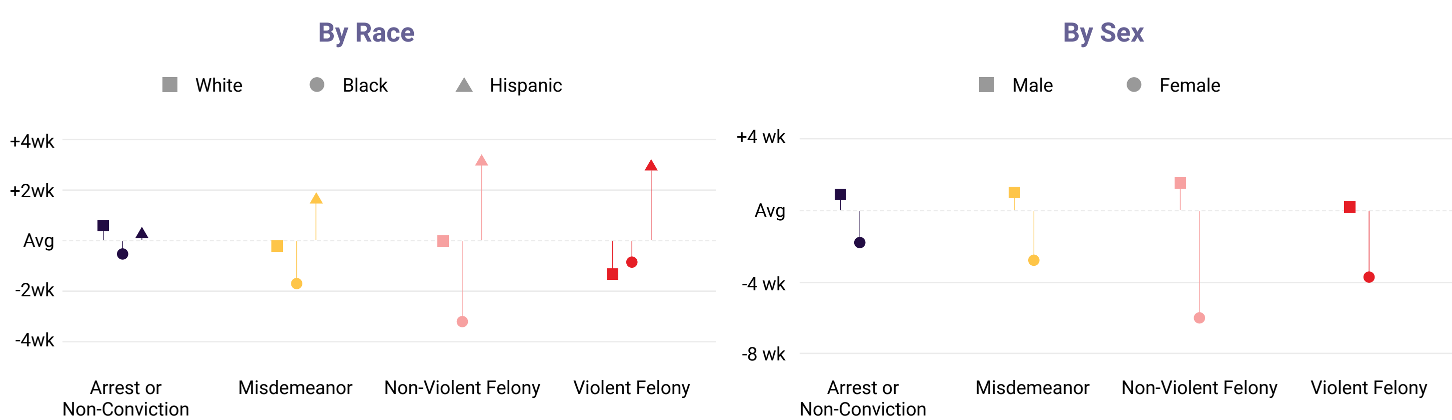
Average Annual Employment Rates in Prime Working Years

For People with an Arrest or Conviction Record



Weeks Worked Among Employed People During Prime Working Years

For People with an Arrest or Conviction Record



ANNUAL EARNINGS LOSSES

Workforce participation measures alone do not fully capture the economic consequences of arrest or conviction records. Even when people with records remain employed, they may still experience lower wages, fewer opportunities for stable or higher-paying work, and interrupted career advancement.

To better understand these broader economic impacts, we estimated expected annual earnings for all individuals in the study, including those who were unemployed or not currently working, rather than relying only on reported earnings among employed respondents.[20] These estimates compare people with arrest or conviction records to peers with no reported legal system involvement or record. The resulting differences represent estimated earnings penalties associated with arrest or conviction records, capturing both reduced workforce participation and lower earnings among those who are employed.

Expected earnings losses were substantial across all record types and increased with record severity. Respondents with arrest or non-conviction records experienced an estimated annual earnings penalty of 14.9%, which is roughly -$4,400. Respondents with misdemeanor convictions experienced an estimated 17.8% (-$6,400) reduction in expected annual earnings. Respondents with non-violent felony convictions lost an estimated 31.8% (-$9,300) of expected annual earnings, while respondents with violent felony convictions experienced the largest estimated penalty, with expected earnings reduced by 36.7% (-$9,800).

Estimated Monetary Penalty

Average annual dollar losses associated with arrest or conviction records

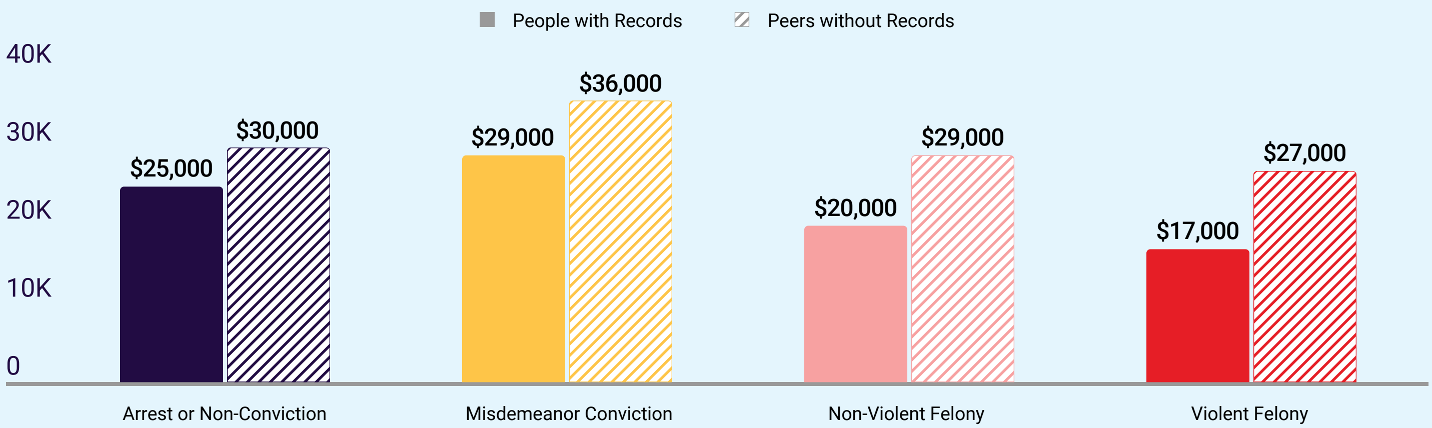
$4,400/yr
14.9%
for people with arrest or non-conviction records

$6,400/yr
17.8%
for people with misdemeanor convictions

$9,300/yr
31.8%
for people with non-violent felony convictions

$9,800/yr
36.7%
for people with violent felony convictions

Average Annual Earnings for People with Records vs. Peers without Records



Respondents with violent felony convictions experience the largest penalty, reducing their expected earnings by over a third.

These findings indicate that arrest and conviction records are associated with substantial earnings losses. Importantly, the estimated penalty for arrest or non-conviction records shows that earnings losses are not limited to people with convictions. **Even in the absence of a conviction, legal system contact is associated with lower expected annual earnings.**

There is limited prior research quantifying the impacts of arrest or non-conviction records on earnings specifically. These records can still create digital footprints that appear in background check systems and may be used by employers, landlords, educational institutions, or licensing bodies when making decisions.

> While the current analysis does not directly measure how an arrest or non-conviction record may affect employer behaviors or employment decisions, the size of the estimated earnings penalty suggests that non-conviction records should not be considered economically harmless.

These are records where people were found not guilty of a crime, arrested but not prosecuted in a court of law, or successfully completed a deferred judgement and had charges dismissed. Some states have laws and systems in place that either seal these types of records at disposition or do not allow for credit reporting agencies to include these on background checks. According to the Collateral Consequences Resource Center, 21 states and D.C. have some automatic process to expunge or seal adult non-conviction records.[21] However, these automatic relief mechanisms are not always comprehensive as they may be prospective only, exclude certain dispositions or charges, or have not been implemented.[22] Regardless, survey research shows that hiring professionals report being unwilling to hire people even with just non-conviction records,[23] and in combination with our findings further underscores the importance of sealing arrest and non-conviction records to ensure people are not being limited economically by an offense they were never found guilty of committing.

Someone with an arrest or non-conviction record could have been detained pretrial. Removing someone from their community and daily life can impact one's ability to attend their job, earn money and/or continue their education.[24] While this analysis controls for all future arrests or convictions, these factors were not investigated in detail and it is important to note that various attributes of the legal system can play a role in decreasing someone's economic stability and mobility regardless of conviction.

Our estimates for misdemeanor and felony convictions are broadly consistent with prior research. The Brennan Center for Justice estimates that individuals with misdemeanor convictions experience approximately a 16% earnings loss associated with their legal system involvement, which aligns with our final estimate of 17.8% and falls well within our 95% confidence interval of -27.5% to -7.1% for this group.[25] The Brennan Center further estimates that felony convictions are associated with earnings losses ranging from 22% to 52%, depending on offense and sentence type. Our estimates for non-violent and violent felony convictions (31.8% and 36.7%, respectively) fall within this range, supporting the external validity of our results and suggesting that our estimates are conservative relative to the upper bound of prior findings.

Our findings show that **people with felony convictions experience the most severe earnings penalty after obtaining a record**. People with non-violent felony convictions earn 68.2 cents for every dollar their peer without a record earns. People with violent felony convictions earn 63.3 cents for every dollar their peer without a record earns. These findings suggest that including felony convictions in Clean Slate automated sealing policies would benefit those facing the harshest economic barriers created by their legal history and conviction record.

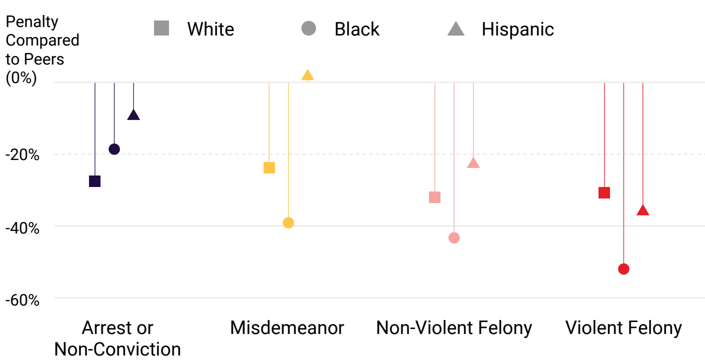
Annual earnings and earnings penalties among those with arrest or conviction records also varied by race and sex. **Additionally, Black respondents with conviction records experienced larger earnings penalties than white or Hispanic respondents in several record categories. Black respondents with violent felony convictions experienced the largest estimated penalty, losing over half (51.8%) of expected annual earnings relative to their Black peers with no reported legal system involvement or record.** Black respondents with misdemeanor convictions and non-violent felony convictions also experienced large estimated penalties, at 39.0% and 43.1%, respectively.

White respondents with arrest or non-conviction records experienced the largest estimated penalty in that record category, at 27.5%. The comparatively large estimated earnings penalty among white respondents with an arrest or non-conviction record may reflect the fact that these estimates are measured relative to same-race peers without records, who likely have substantially higher baseline earnings on average than Black or Hispanic people without records. In the previous subsection, we reported that white respondents with arrest or non-conviction records remained relatively engaged in the workforce, with higher employment rates and weeks worked in a given year. Nevertheless, they may still experience reductions in wages, occupational quality, or opportunities for advancement following legal system contact. Prior research suggests that employers may interpret records through existing racial stereotypes and assumptions about criminality, which can shape how records affect applicants across racial groups.[26] At the same time, Black respondents in this study experienced substantially larger estimated earnings penalties across all record categories, particularly for felony convictions, suggesting that more serious forms of legal-system involvement may compound existing racial inequalities in labor-market outcomes.

Hispanic respondents showed smaller and less consistent estimated penalties in several categories, including a positive estimated difference for misdemeanor convictions. Because the sample sizes for Hispanic respondents in our survey are small, and because some estimates are less stable, the Hispanic subgroup results should be interpreted with caution and should not be overgeneralized without further analysis.[27]

Earnings Penalties in Prime Working Years, by Race

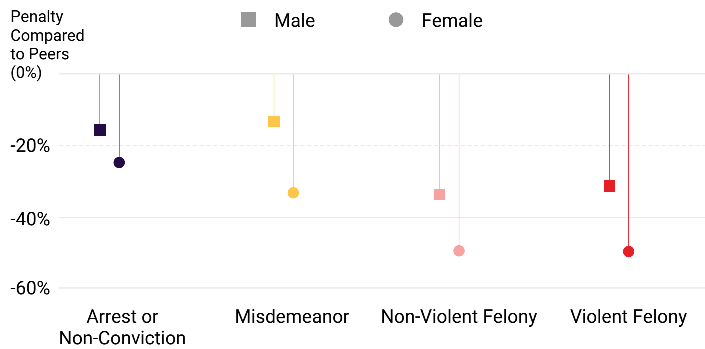
For People with an Arrest or Conviction Record



Female respondents with records experienced larger estimated annual earnings losses than male respondents with the same record type. The differences were especially large for conviction records. For example, female respondents with misdemeanor convictions experienced a 33.0% estimated earnings penalty, compared to 13.4% among male respondents. Female respondents with non-violent and violent felony convictions experienced estimated penalties of 49.1% and 49.4%, respectively.

Earnings Penalties in Prime Working Years, by Sex

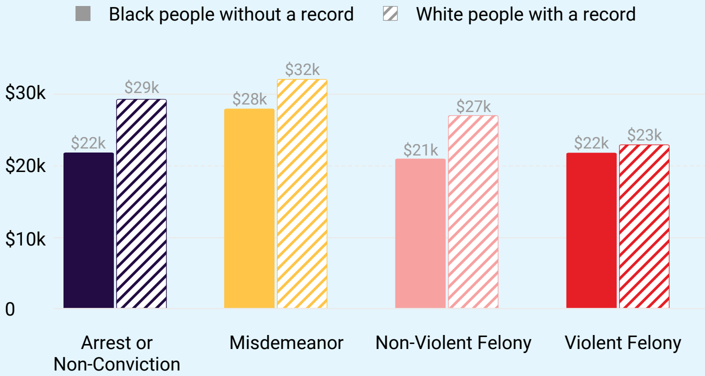
For People with an Arrest or Conviction Record



Female respondents with arrest or conviction records earn substantially less than their male counterparts with the same record type, resulting in a pay gap that is larger than the gender pay gap in the broader labor market.[28] Female respondents with arrest or non-conviction records earned about 63 cents for every dollar earned by male respondents with the same record type. Female respondents with conviction records earned roughly 50 to 53 cents for every dollar earned by male respondents with the same conviction type.

While this analysis could not explore the intensified impact of race on our sex-based findings, The Clean Slate Initiative (CSI) recognizes the importance of intersectionality in creating varied outcomes, particularly for groups like Black women. Future publications will apply similar methods to examine how the association between arrest and conviction records and lost earnings differs across various race and sex groups.

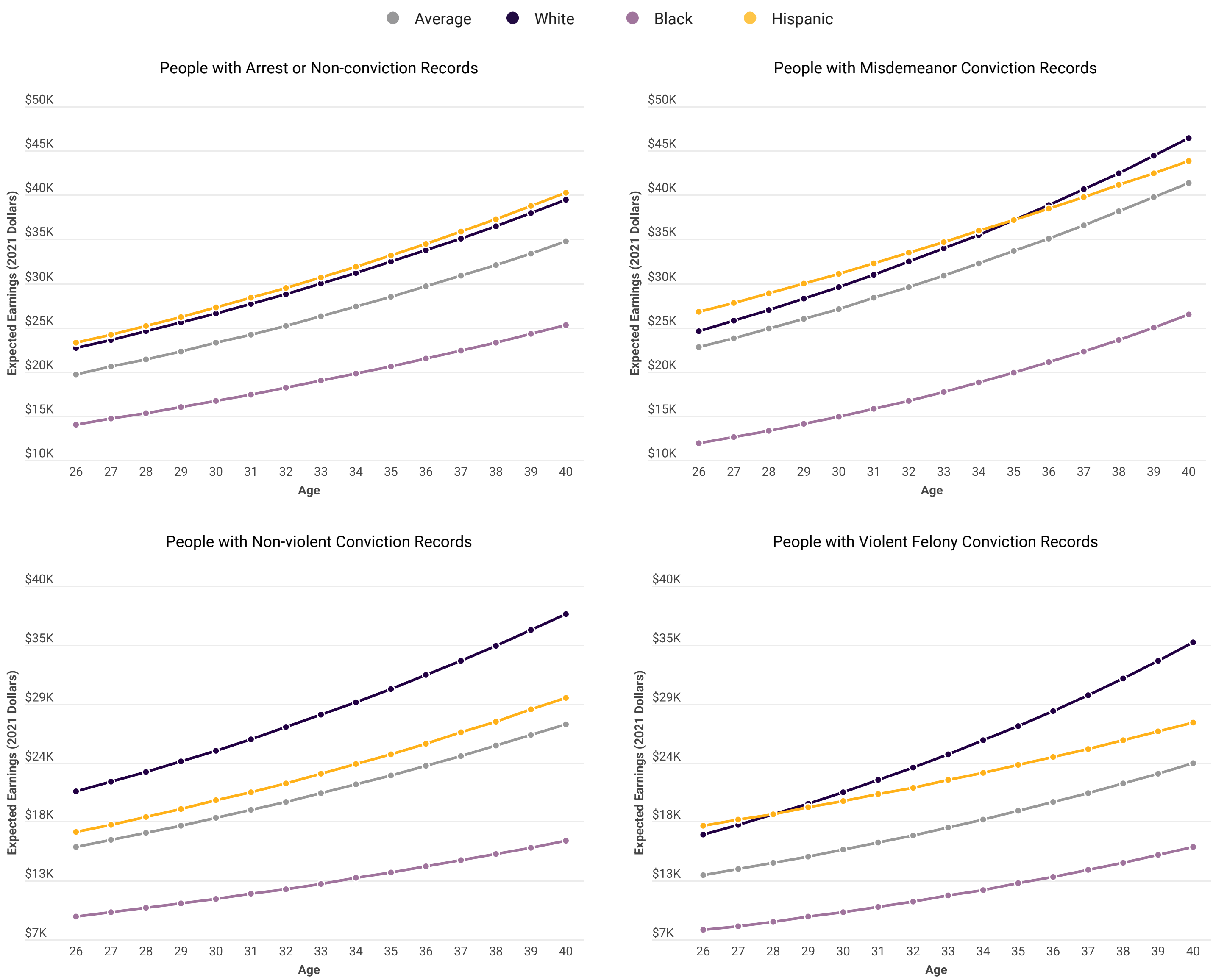
Expected Earnings of Black People without Records vs. White People with Records



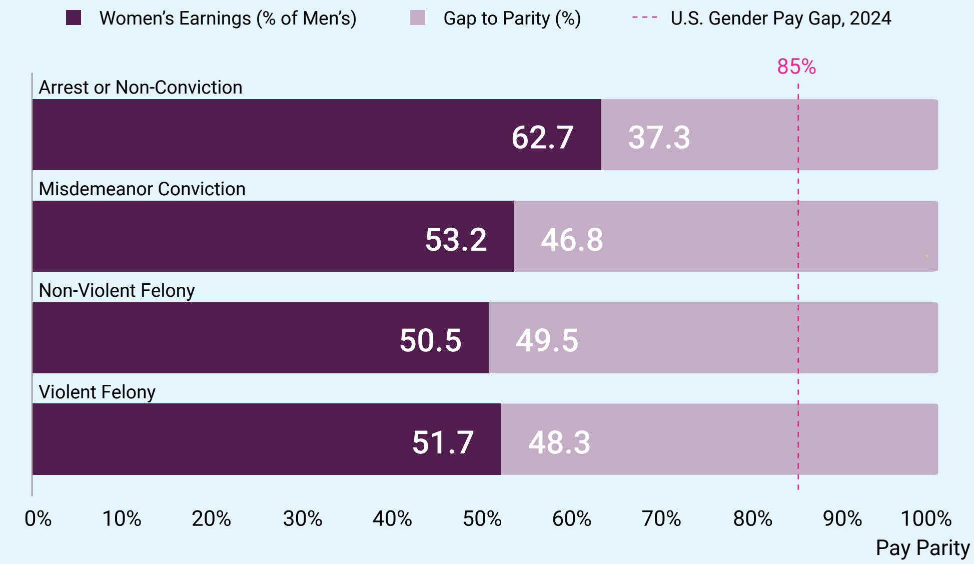
One of the key findings from the race-specific analysis is that Black respondents without records had lower expected earnings than white respondents with records. This shows that racial inequality in earnings exists for Black individuals even in the absence of a record, and that arrest or conviction records can deepen pre-existing labor-market disparities.[29]

Between the larger annual earnings penalties and lower baseline earnings among Black respondents, Black respondents with arrest or conviction records earn substantially less than those with the same record type and different race.

Expected Earnings in Prime Working Years, by Record Type and Race

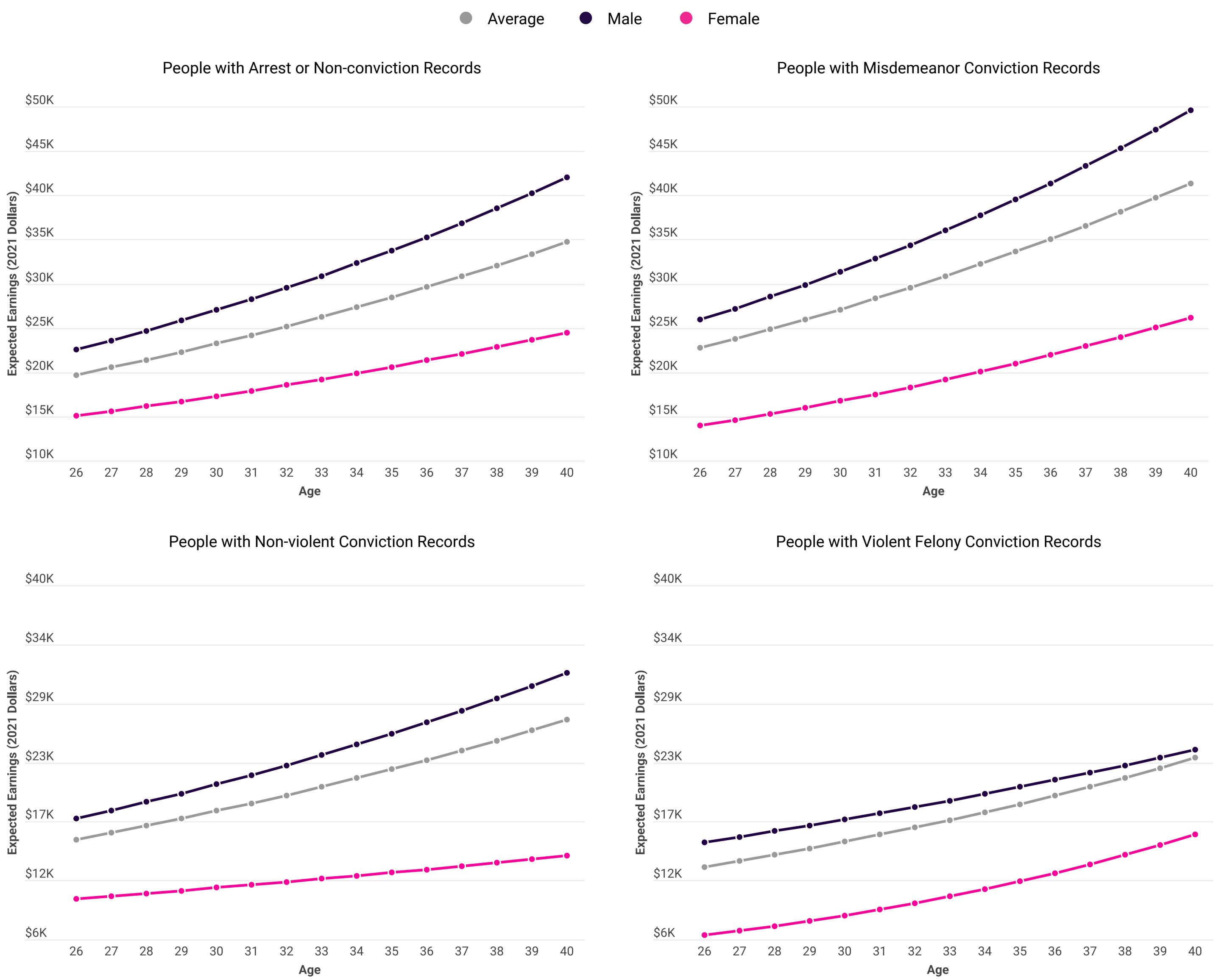


Wage Gap Among Male and Female Respondents with Arrest or Conviction Records



The gender pay gap findings suggest that **records may interact with existing sex-based labor-market inequality** in ways that deepen economic disadvantage.

Expected Earnings in Prime Work Years, by Record Type and Sex



CUMULATIVE EARNINGS LOSSES

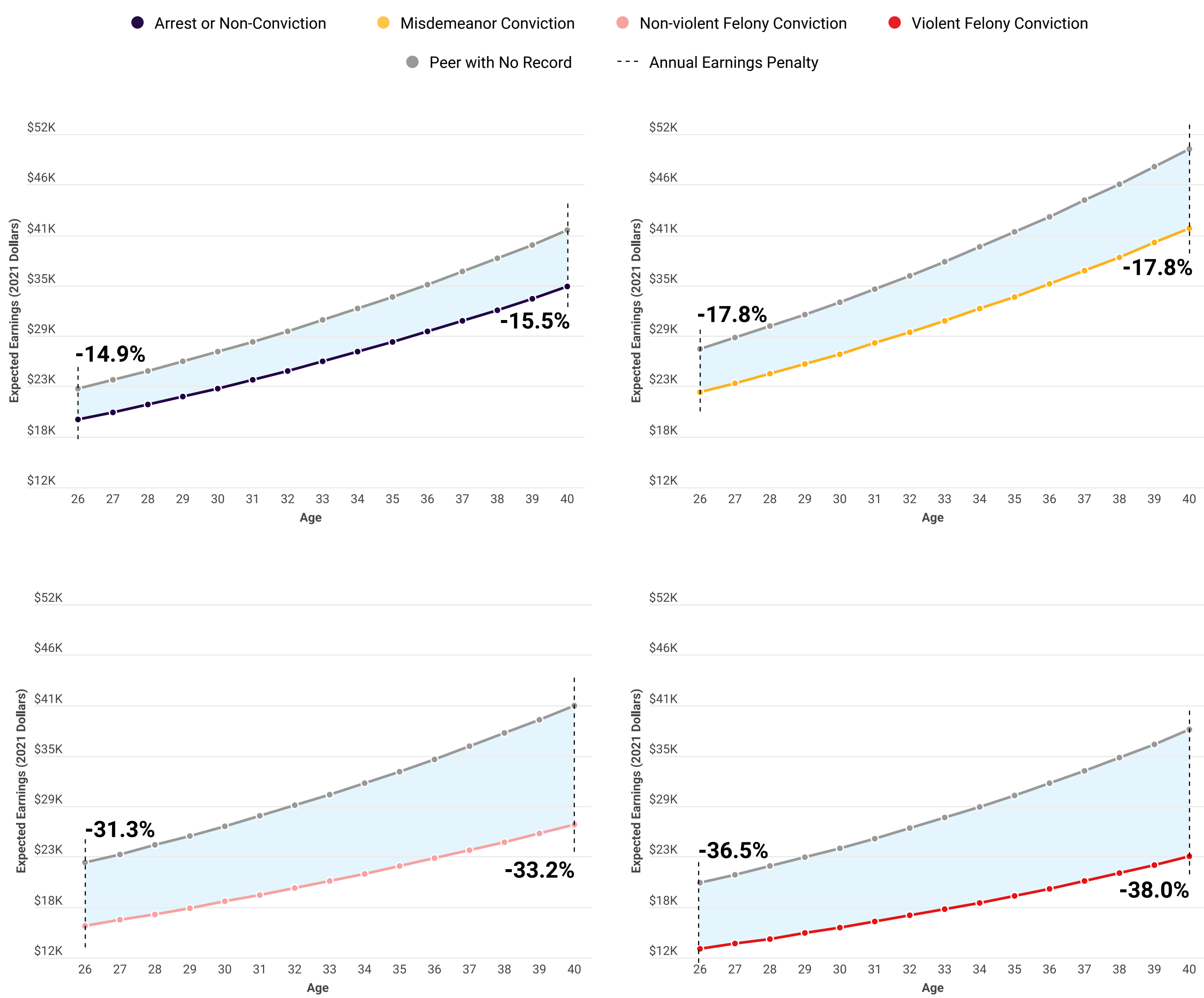
Earnings penalties are apparent immediately after a person obtains a record and remains across the fourteen year time period that constitutes one's prime working years. In some cases these penalties worsen over time, regardless of these respondents remaining crime free and their records getting older.

Respondents with arrest or non-conviction and misdemeanor conviction records showed relatively stable penalties over time. However, **respondents with felony conviction records experienced larger penalties, and those penalties worsened modestly during prime working years**.

This suggests that the earnings differences associated with felony convictions may persist and compound as individuals move through their prime working years, and this pattern will likely continue into one's late career.

Clean Slate automated record sealing policies that include some eligible felony convictions and have a reasonable waiting period could be **a valuable solution to reducing these worsening economic conditions.**

Expected Earnings in Prime Working Years, by Age and Record Type



Across the fourteen year time period we studied, cumulative individual-level earnings losses were substantial. People with violent felony convictions lost an estimated $159,000 on average between ages 26 and 40. In the most extreme cases, people with violent felony convictions can lose up to nearly a quarter million dollars ($232k) in the span of 14 years. People with non-violent felony convictions lost an estimated $150,000. People with misdemeanor convictions lost an estimated $102,000, while people with arrest or non-conviction records lost an estimated $72,000.[30]

The relatively similar cumulative losses for non-violent and violent felony convictions suggest that felony status itself may carry substantial labor-market consequences, even when offense categories differ.

However, this analysis cannot determine whether employers respond similarly to violent and non-violent felony convictions, nor can it isolate the specific mechanisms driving these patterns. More research is needed to better understand how employers interpret different types of felony records.

In the previous sub-section, we reported that Black respondents with arrest and conviction records often experience larger annual earnings penalties compared to white respondents with the same records. When these annual losses accumulate over prime working years, they translate into substantial reductions in lifetime earnings and economic opportunity.

Black respondents often lose as much as or more than white respondents over their prime working years despite earning substantially less throughout those years. This finding is particularly striking because these losses occur on top of already-lower earning levels, further widening the existing racial disparities in income and wealth accumulation.

Among respondents with arrest or non-conviction records, Black people were estimated to lose approximately $71,000 in earnings between ages 26 and 40, compared to $177,000 among their white peers While cumulative losses were larger for white respondents in this category, the difference should be interpreted in the context of substantially different earnings levels. By age 40, Black respondents with arrest or non-conviction records are expected to earn approximately $25,000 annually, compared to nearly $39,000 for white respondents with the same record type. As a result, even smaller cumulative losses can represent a significant economic burden for Black individuals and their families.

The pattern differs for people with conviction records. Black people with misdemeanor convictions were estimated to lose $178,000 in earnings between ages 26 and 40, compared to $165,000 among their white peers. Because

Black respondents without records had substantially lower expected earnings than their white counterparts, these losses represented a larger share of the earnings Black respondents would otherwise have been expected to earn. These findings suggest that misdemeanor convictions may have particularly severe long-term economic consequences for Black individuals.

The cumulative losses associated with felony convictions were substantial for both Black and white respondents and generally exceeded those observed for misdemeanor convictions. Importantly, Black people with any type of felony conviction earn less than half as much as their white peers throughout their prime working years. Despite these substantially lower earnings, Black respondents with non-violent felony convictions were estimated to lose $147,000 in earnings during this period, while white respondents with the same records lost $206,000. Losses were even greater among respondents with violent felony convictions, with Black respondents losing an estimated $189,000 over their prime working years compared to $177,000 among white respondents.

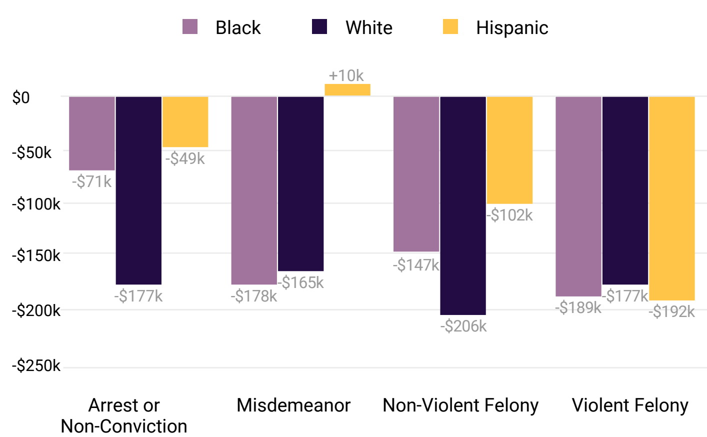
> Together, these findings suggest that **felony convictions are associated with some of the largest cumulative earnings losses observed in this study**. They also highlight how felony convictions can deepen existing racial inequalities, as **Black respondents experience substantial earnings losses despite already earning less than half as much as their white peers with the same records.**

Examining cumulative losses as a share of the earnings individuals would have been expected to earn without a record reveals an even clearer pattern. **Black respondents experienced the largest proportional losses in every conviction category examined.** Black respondents with violent felony convictions lost an estimated 52.6% of their expected prime-working-year earnings, compared to 32.2% among white respondents with the same records. Similarly, Black respondents with non-violent felony convictions lost 44.2% of expected earnings, compared to 32.6% among white respondents. **These findings suggest that although white respondents sometimes experienced larger dollar losses, conviction records often imposed a greater relative economic burden on Black respondents.**

The patterns observed among Hispanic respondents with arrest or conviction records differs from those observed among Black respondents with records. For arrest or non-conviction and misdemeanor conviction records, Hispanic respondents generally had annual earnings comparable to, or slightly higher than, their white peers. Additionally, Hispanic respondents with these record types had more similar earnings to their same-race peers without records. As a result, the cumulative earnings losses for Hispanic people with these record types were generally smaller than those observed among white and Black respondents.

The earnings trajectories of Hispanic respondents diverge from those of white respondents with felony convictions. Hispanic respondents with non-violent felony convictions earned approximately 19.6% less than white respondents with the same records and were estimated to lose $102,000 in earnings between ages 26 and 40. Hispanic respondents with violent felony convictions earned approximately 9.4% less than comparable white respondents and experienced cumulative losses of approximately $192,000 during their prime working years. Although these findings suggest that felony convictions may be associated with substantial long-term economic consequences for Hispanic individuals, the Hispanic subgroup estimates should be interpreted with caution.[31]

Cumulative Earnings Loss by Race and Record Type



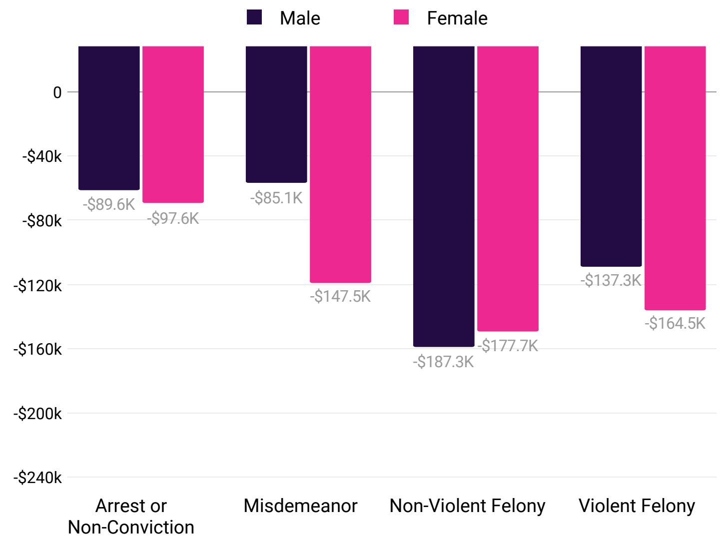
The cumulative earnings losses associated with arrest and conviction records also differed substantially between male and female respondents. Women generally experienced larger cumulative losses than men despite having lower expected earnings to begin with. Among respondents with arrest or non-conviction records, women were estimated to lose approximately $98,000 in earnings between ages 26 and 40, compared to $90,000 among men. The disparity was even larger for misdemeanor convictions, where women were estimated to lose $147,000 during their prime working years compared to $85,000 among men. For felony convictions, cumulative losses were substantial for both groups. Women with non-violent felony convictions were estimated to lose $178,000 in earnings between ages 26 and 40, while men lost approximately $187,000. Among respondents with violent felony convictions, women lost approximately $165,000 compared to $137,000 among men.[32]

Examining these losses relative to expected earnings reveals an even larger disparity.

Female respondents experienced greater proportional losses than male respondents across every record category examined.

Women with arrest or non-conviction records lost an estimated 25.1% of the earnings they would have been expected to earn during their prime working years, compared to 16.0% among men. The differences were even more pronounced for conviction records. Women with misdemeanor convictions lost approximately one-third (33.5%) of their expected earnings, compared to 13.4% among men. For felony convictions, women lost nearly half of their expected earnings during prime working years, including 49.9% for non-violent felony convictions and 51.0% for violent felony convictions. Together, these findings suggest that arrest and conviction records may compound existing labor-market disadvantages faced by women, resulting in disproportionately large long-term earnings losses relative to similarly situated women without records.

Cumulative Earnings Loss by Sex and Record Type



State and National Economic Losses from Arrest and Conviction Records

In 2024, CSI published a Data Model and accompanying Data Dashboard[33] that estimates the number of people with arrest or conviction records nationwide and across all 50 states and the District of Columbia.

This study uses these population estimates in combination with the individual-level earnings penalties outlined in the previous section, to estimate the aggregate lost earnings from various record types on the larger economy.

To estimate national losses, we used the average earnings penalty associated with each record type from the entire survey sample. To estimate state losses, we used the average earnings penalty associated with each record type based on the U.S. Census Region a respondent self-reported living in during their prime working years. For more information on the methods used to compute state-level economic losses, please refer to the Methodology section of this report.

NATIONWIDE LOSSES

Nationally, arrest and conviction records are associated with an estimated $440 billion in lost earnings annually. Although individuals with arrest or non-conviction records have no convictions, they account for an estimated $44.7 billion in lost annual earnings, 10.2% of the overall nationwide losses. This finding underscores that the economic costs of legal system involvement are not limited to conviction records only.

Individuals with misdemeanor convictions account for the single largest share of aggregate earnings losses. We estimate that misdemeanor conviction records are associated with $187.7 billion in lost annual earnings, 42.6% of the overall nationwide losses. This reflects both the large number of people with misdemeanor conviction records and the substantial individual-level earnings penalty associated with this record type.

Felony convictions account for an additional $208.2 billion in lost annual earnings. Non-violent felony convictions account for the majority of these lost earnings, approximately $148.2 billion of that total and 33.6% of the total nationwide losses. While violent felony convictions account for approximately $60.0 billion or 13.6% of the total nationwide losses. Although individuals with violent felony convictions experience the largest individual-level earnings penalties, the larger population of individuals with non-violent felony convictions results in greater aggregate losses.

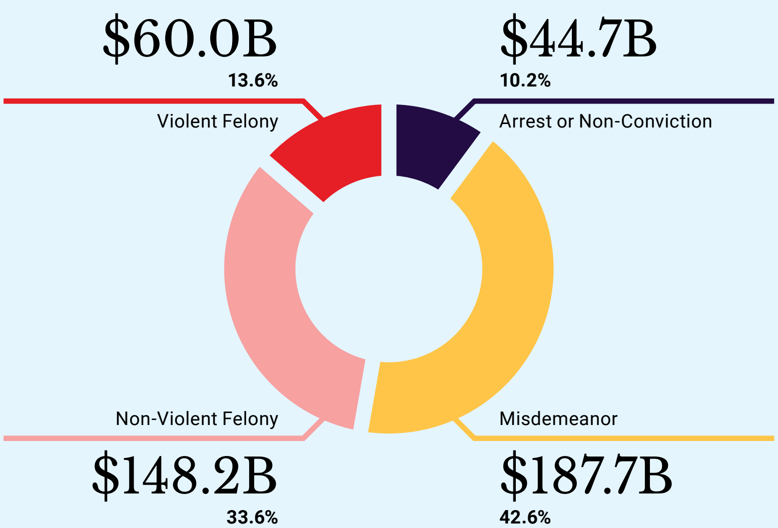
Taken together, these estimates show that records are associated with substantial macroeconomic consequences. **Lost earnings affect individuals and families directly, but they also reduce economic activity in communities, lower consumer spending, and reduce tax revenue.**

Table 12. National Annual Earnings Lost from Arrest or Conviction Records

	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Average Annual Earnings	$25,000	$29,000	$20,000	$17,000
Average Annual Earnings of Peers	$30,000	$36,000	$29,000	$27,000
Earnings Penalty	-14.9%	-17.8%	-31.8%	-36.7%
Cumulative Earnings Lost	**$44.7 Billion**	**$187.7 Billion**	**$148.2 Billion**	**$60.0 Billion**

Cumulative Earnings Lost ($ Billions)

Misdemeanors drive the highest total volume of loss.



The data reveals a stark reality: records of all kinds carry economic consequences that, at scale, translate into billions in lost earnings for families and communities.

Together, these losses drain $440 billion from the national economy each year.

STATE LOSSES

By applying regional earnings penalties[34] to state population estimates from CSI's Data Model, we estimated the cumulative annual earnings lost across all 50 states and the District of Columbia. These state-level estimates reveal that lost earnings are highly concentrated in a relatively small number of large states. **Only eight states account for roughly half of the estimated $440 billion in annual earnings losses nationwide.**

The top eight states by estimated annual earnings lost are California, Florida, Texas, New York, Georgia, Arizona, Pennsylvania, and New Jersey. These rankings largely align with the size of each state's population with arrest or conviction records as we are unable to account for state-level factors that may impact earnings losses post arrest or conviction like pre-existing sealing laws. However, Arizona and New Jersey contribute more to national lost earnings than their population rank alone would suggest. Ohio, by contrast, has the seventh-largest population with arrest or conviction records but does not appear among the top eight states by earnings lost, largely because the regional earnings penalties and earnings of peers without records applied to Ohio are lower than the other largest states.

Top 8 States by Annual Earnings Lost from Arrest and Conviction Records

Earnings Rank	State	Annual Earnings Loss	Cumulative % of Total	Population with a Record Rank
1	California	$63.8 billion	14.5%	1
2	Florida	$40.5 billion	23.7%	2
3	Texas	$36.4 billion	31.9%	3
4	New York	$20.9 billion	36.7%	4
5	Georgia	$18.5 billion	40.9%	5
6	Arizona	$14.9 billion	44.3%	8
7	Pennsylvania	$14.7 billion	47.6%	6
8	New Jersey	$11.7 billion	50.3%	10

The cumulative statistics presented here **do not account for the potential effect of state or local policies.**

Because state-level identifiers are not available in the public-use NLSY97, regional earnings differentials were extrapolated to their corresponding states. States with long-standing policies that limit employer access to records, seal records to public-use, or restrict the use of arrest and conviction records in employment decisions may experience different impacts than estimated here.

Even with these limitations, the state-level results highlight that the economic costs associated with records are widespread and substantial. In many states, estimated annual lost earnings exceed several billion dollars. **In every state, with the exception of Vermont, the annual lost earnings associated with arrest or conviction records exceeds $1 billion.** This represents a meaningful drag on local economies, families, and tax bases.

Annual Earnings Lost from Arrest and Conviction Records, by State

Listed in Ascending Order

<$1B

VT

$1 - $7B

ND, DC, SD, DE, ME, RI, MT, WY, WV, AK, NH, HI, NE, KS, ID, MS, UT, OK, AL, IA, CT, NM, AR, SC, MD, NV, OR, MI, IN, MA, WA, MN, LA, KY

$8B - $15B

VA, TN, WI, MO, CO, NC, OH, IL, NJ, PA, AZ

$16B - $31B

GA, NY

$32B - $64B

TX, FL, CA

State-level Annual Earnings Losses from Arrest or Conviction Records

In Billions of Dollars

State	Arrest or Non-conviction	Misdemeanor Convicted Population, No Felonies	Non-violent Felony Convicted Population	Violent Felony Convicted Population	Cumulative Earnings Lost
Northeast	**$8.4 Billion**	**$30.5 Billion**	**$18.0 Billion**	**$8.5 Billion**	**$65.3 Billion**
Connecticut	-0.48	-1.51	-2.17	-0.53	-4.70
Maine	-0.20	-0.87	-0.30	-0.17	-1.55
Massachusetts	-0.64	-1.49	-2.85	-2.39	-7.37
New Hampshire	-0.30	-1.45	-0.25	-0.10	-2.10
New Jersey	-1.33	-5.18	-3.85	-1.35	-11.71
New York	-3.41	-11.03	-4.53	-1.98	-20.94
Pennsylvania	-1.74	-8.12	-3.36	-1.51	-14.74
Rhode Island	-0.19	-0.73	-0.40	-0.27	-1.58
Vermont	-0.07	-0.15	-0.28	-0.16	-0.65
North Central	**$8.2 Billion**	**$32.7 Billion**	**$23.8 Billion**	**$10.3 Billion**	**$75.0 Billion**
Illinois	-1.07	-3.97	-4.72	-1.54	-11.29
Indiana	-0.80	-2.04	-3.32	-1.14	-7.30
Iowa	-0.40	-2.78	-1.12	-0.21	-4.51
Kansas	-0.32	-0.96	-0.84	-0.50	-2.62
Michigan	-1.13	-2.05	-2.71	-1.29	-7.18
Minnesota	-0.66	-3.56	-1.91	-1.29	-7.43
Missouri	-1.01	-5.30	-2.60	-0.69	-9.59
Nebraska	-0.28	-1.46	-0.47	-0.19	-2.41
North Dakota	-0.06	-0.71	-0.27	-0.09	-1.13
Ohio	-1.46	-2.93	-4.21	-2.57	-11.17
South Dakota	-0.20	-0.61	-0.38	-0.09	-1.27
Wisconsin	-0.80	-6.34	-1.25	-0.68	-9.07
South	**$15.5 Billion**	**$62.2 Billion**	**$67.0 Billion**	**$26.6 Billion**	**$171.3 billion**
Alabama	-0.54	-1.89	-1.59	-0.45	-4.47
Arkansas	-0.31	-2.96	-2.07	-0.43	-5.76
Delaware	-0.11	-0.37	-0.64	-0.16	-1.27
District of Columbia	-0.12	-0.20	-0.53	-0.29	-1.15
Florida	-2.74	-15.22	-15.02	-7.51	-40.49
Georgia	-1.29	-3.17	-9.59	-4.40	-18.46
Kentucky	-0.85	-3.61	-2.49	-0.58	-7.53
Louisiana	-0.64	-3.51	-2.73	-0.62	-7.50
Maryland	-0.88	-0.71	-2.86	-1.55	-6.01

State-level Annual Earnings Losses from Arrest or Conviction Records (cont'd)

In Billions of Dollars

State	Arrest or Non-conviction	Misdemeanor Convicted Population, No Felonies	Non-violent Felony Convicted Population	Violent Felony Convicted Population	Cumulative Earnings Lost
Mississippi	-0.39	-1.83	-1.31	-0.31	-3.86
North Carolina	-2.15	-2.60	-3.88	-1.44	-10.07
Oklahoma	-0.40	-1.65	-1.77	-0.44	-4.25
South Carolina	-0.61	-2.91	-1.70	-0.58	-5.80
Tennessee	-1.08	-4.05	-2.33	-0.91	-8.37
Texas	-2.44	-13.59	-14.57	-5.80	-36.40
Virginia	-0.75	-2.88	-3.47	-0.95	-8.05
West Virginia	-0.21	-1.07	-0.45	-0.14	-1.86
West	**$12.6 Billion**	**$62.2 Billion**	**$39.4 Billion**	**$14.7 Billion**	**$129.0 Billion**
Alaska	-0.16	-0.89	-0.71	-0.19	-1.94
Arizona	-1.16	-8.37	-4.08	-1.27	-14.88
California	-4.71	-30.00	-21.40	-7.70	-63.82
Colorado	-1.63	-4.32	-2.79	-0.99	-9.73
Hawaii	-0.29	-1.14	-0.61	-0.17	-2.21
Idaho	-0.37	-1.80	-1.14	-0.26	-3.56
Montana	-0.16	-0.75	-0.44	-0.27	-1.61
Nevada	-0.72	-3.99	-1.29	-0.37	-6.37
New Mexico	-0.81	-2.70	-0.91	-0.71	-5.14
Oregon	-0.52	-2.76	-2.02	-1.20	-6.51
Utah	-0.82	-1.99	-0.84	-0.31	-3.96
Washington	-1.06	-2.32	-2.90	-1.14	-7.42
Wyoming	-0.21	-1.18	-0.33	-0.13	-1.84

Policy and Economic Implications

Arrest and conviction records are associated with substantial earnings losses for individuals and large economic losses for the country as a whole.

These losses are present across all record categories, including arrest and non-conviction records, and they increase with record severity. They also vary by race and sex, underscoring that the economic consequences of legal system involvement are not distributed evenly.

> The most important finding is that **records are associated with long-term economic penalties even after accounting for observed differences** between people with records and their peers without records.

Respondents with arrest or non-conviction records experienced an estimated 14.9% reduction in expected annual earnings, equal to roughly $4,400 per year. Respondents with misdemeanor convictions experienced a 17.8% reduction, equal to roughly $6,400 per year. The estimated penalties were even larger for felony convictions, reaching 31.8%, or roughly $9,300 per year, for non-violent felony convictions, and 36.7%, or roughly $9,800 per year, for violent felony convictions.

These results matter for Clean Slate and other record sealing policies because they show that the economic burden of a record extends well beyond the point of contact with the legal system and despite a person remaining crime-free for over a decade. Earnings penalties persist throughout prime working years, affecting people's ability to support themselves, care for their families, build savings and wealth, and participate fully in the economy.

The cumulative individual-level losses are large: $72,000 for people with arrest or non-conviction records, $102,000 for people with misdemeanor conviction records, $150,000 for people with non-violent felony conviction records, and $159,000 for people with violent felony conviction records between ages 26 and 40. To put these figures in context, $72,000 is comparable to a down payment on a home.[35] Losses exceeding $100,000 approach the cost of a four-year college degree,[36] while losses in the range of $150,000 to $160,000 are comparable to more than a decade of retirement savings.[37]

The Real Cost of Earnings Losses

For people with records, the impact of earnings lost adds up.

$72k
is comparable to a down payment on a home.

$100k
approaches the cost of a four-year college degree.

$150-160k
is comparable to more than a decade of retirement savings.

Cumulative earnings losses are large, and represent real economic mobility opportunities lost.

> These **penalties are likely to continue accumulating beyond age 40** and may have long-term consequences.

Reduced earnings during a person's prime working years limit their ability to accumulate assets, invest in education or training, build retirement savings, and support family members. These effects can compound over time, extending beyond the individual and shaping family stability and generational wealth.

Arrest and non-conviction records are especially important. These are records where a person may not have been convicted of any crime, yet the estimated earnings penalty remains large. **This supports the need for policies that address the sealing of non-conviction records, not only conviction records. In states where non-conviction records remain publicly accessible or appear on background checks, people may continue to face economic barriers even when they were not found guilty.**

The findings also suggest that misdemeanor records deserve serious policy attention. Misdemeanor charges are legally classified as lower-level offenses; however, they can have real and harmful effects on peoples' lives. Misdemeanor offenses account for more than three-quarters of all cases filed annually in the United States,[38] meaning they represent one of the most common points of contact between the public and the legal system. Despite typically carrying lighter formal penalties than felonies, such as fines, fees, probation, or short jail sentences, misdemeanor convictions can still result in lasting records that restrict access to employment, housing, education, and professional licensing.

Misdemeanor offenses account for more than

75%

of all cases filed annually in the United States[37]

Because of their prevalence, **misdemeanor conviction records account for the largest single category of national lost earnings, with an estimated $187.7 billion in annual losses**. This means that even modest individual-level penalties can become enormous when applied to a large affected population. The Clean Slate Initiative's policy minimums require misdemeanor conviction records to be eligible for sealing to be considered a true Clean Slate policy. All states considering automated record sealing policies should ensure misdemeanors are eligible for sealing because they likely cover the largest population of directly impacted individuals and are associated with substantial economic hardship despite the lower severity of the underlying charge.

Felony conviction records carry the largest individual-level penalties. Although violent felony convictions are often excluded from automated record sealing policies, the findings show that both violent and non-violent felony conviction records are associated with severe, persistent, and worsening earnings losses throughout peoples' prime working years. Non-violent felony convictions are especially important from a policy perspective because they are more common than violent felony convictions and account for a larger share of aggregate felony-related earnings losses.

While including felony convictions is not part of The Clean Slate Initiative's policy minimums beyond a strong recommendation, making at least some felony convictions eligible for automated sealing could produce meaningful economic benefits for individuals and state economies. From a per-person perspective, people with felony convictions face the harshest barriers to employment and earnings penalties, meaning that relief for eligible felony records may have especially large individual-level effects.

Nearly all of the 15 states or jurisdictions with Clean Slate policies include some felony convictions for sealing, with the exception of Utah, Virginia, and D.C.[39]

As more research is published on the economic impacts of Clean Slate post implementation, it will be important to examine whether states that exclude felony convictions experience smaller economic gains than states with broader eligibility.

The race and sex findings highlight that record-related earnings penalties intersect with broader labor-market inequality. Black respondents with conviction records experienced especially large earnings penalties, but Black respondents without records also earned less than white respondents with records. This demonstrates that record-related penalties operate on top of existing racial inequalities. For women, the findings suggest that records may deepen existing sex-based earnings gaps. Female respondents with conviction records earned roughly half as much as male respondents with the same record type.

Clean Slate should be viewed as one element of a larger strategy for economic justice, a perspective supported by these results.

> Record sealing can reduce barriers to employment and economic mobility, but **it cannot fully eliminate the structural disadvantages that shape labor-market outcomes.**

People with records exist within a network of systems and institutions, including courts, supervision systems, employers, housing markets, licensing agencies, health systems, and educational institutions. Any one of these systems can limit opportunity, especially for people of color and women.

For that reason, we should not expect Clean Slate legislation to recover all of the estimated lost earnings presented in this report. Some portion of these losses may reflect barriers directly tied to public record visibility and background checks, while other portions may reflect incarceration, court debt, disrupted education, weakened work history, health challenges, discrimination, or broader structural inequality. However, Clean Slate legislation remains an important piece of the puzzle. By reducing unnecessary access to old records, automated record sealing can help people who have remained crime-free move forward with fewer barriers to employment, housing, education, and economic stability. In doing so, these policies may help people begin to regain some of the tens to hundreds of thousands of dollars in earnings associated with justice involvement and arrest or conviction records.

> **Clean Slate policies may help people begin to regain some of the tens to hundreds of thousands of dollars** in earnings associated with justice involvement and arrest or conviction records.

At the same time, the full economic impact of Clean Slate will take time to understand. Because these policies are relatively new and economic outcomes unfold over the course of years, the evidence base on earnings and economic mobility is still developing. The Clean Slate Initiative is actively building this evidence through a portfolio of funded research examining long-term outcomes, with many results expected by the end of 2028. In the near term, survey research and qualitative studies will provide earlier insight into how people experience record sealing and whether it changes behavior, such as applying for jobs or housing. Over time, rigorous causal research using administrative data will help clarify impacts on employment, earnings, and broader measures of well-being. Together, this research agenda will allow us to better understand not only whether Clean Slate improves economic outcomes, but how, for whom, and under what conditions those gains are realized.

Conclusion

This report demonstrates that arrest and conviction records are associated with substantial and enduring economic losses for individuals and the nation as a whole.

These losses persist long after initial legal system contact, affecting people during their prime working years and limiting their ability to build financial stability, support their families, and contribute fully to the economy.

The evidence shows that these penalties are not confined to the most severe records. While felony conviction records carry the largest individual-level impacts, misdemeanor conviction records, because of their scale, drive the largest share of aggregate economic loss. Importantly, even arrest and non-conviction records are associated with meaningful earnings reductions, reinforcing that legal system contact alone can have long-term economic consequences.

At a national level, the estimated $440 billion in lost annual earnings represents a significant drag on economic growth. These losses ripple outward, reducing consumer spending, weakening local economies, and likely diminishing tax revenue. In an environment where labor force participation remains constrained and employers face persistent hiring challenges, excluding millions of working-age individuals from full participation in the labor market represents a major inefficiency.

The findings also make clear that the economic burden of records is not experienced equally. Disparities by race and sex indicate that record-related penalties intersect with broader structural inequalities, deepening existing gaps in earnings and opportunity. As a result, policies aimed at reducing the impact of records should be understood not only as workforce interventions, but also as tools for advancing economic equity.

Clean Slate automated record sealing policies offer a practical and evidence-based approach to reducing unnecessary barriers to employment.

By limiting access to old records for individuals who have remained crime-free, these policies can help reconnect people to the labor market and allow them to recover some of the earnings losses documented in this report. Expanding eligibility, particularly to include felony convictions, has the potential to generate meaningful economic benefits at both the individual and societal levels. However, record sealing alone is not a complete solution.

The economic consequences associated with legal system involvement reflect a broader set of challenges, including disrupted education, limited work experience, health impacts, and systemic discrimination. Addressing these issues will require a comprehensive approach that spans multiple systems and institutions.

Even so, the scale of the losses identified in this analysis makes one conclusion clear: reducing barriers associated with arrest and conviction records is not only a matter of fairness, it is an economic imperative. By enabling more people to participate fully in the workforce, policymakers can support stronger economic growth, more stable communities, and a more inclusive labor market.

This study estimates the economic losses associated with arrest and conviction records using data from the National Longitudinal Survey of Youth 1997 (NLSY97), a rare resource for its comprehensive timespan and rich detail on individuals' involvement with the legal system.

The survey follows the lives of a nationally representative sample of approximately 9,000 youths. Initially surveyed in 1997 at ages 12 to 17, the respondents were 36 to 41 years old in 2021, the most recent survey year at the time of this analysis.

The NLSY97 covers several topic areas, including education, employment, demographics, income, crime and substance use, among others. The *Crime and Substance Use* topic provides detailed information on respondents' contact with the legal system, which allows for categorization of individuals into relevant treatment groups, individuals with an arrest or conviction record who can be compared to a group of individuals with no reported legal system involvement, based on the most serious arrest or conviction they reported. This study uses the public-use NLSY97 dataset, and we did not have access to state-level geographic data. Our Methodology section, "Incorporating The Clean Slate Initiative's Data Model to Estimate Economic Losses" (page 34) describes how we used regional indicators to estimate state-level earnings loss due to legal system exposure.

We employed Propensity Score Matching (PSM) to measure earning differences associated with the various levels of arrest and conviction records. This involved identifying statistically similar respondents who had no reported involvement in the legal system, allowing for a comparison of earnings between the two groups while minimizing observable selection bias.

This framework enables us to estimate the economic losses associated with records for individuals across the country and within specific states who may be eligible for Clean Slate policies.

CLASSIFYING SURVEY RESPONDENTS' MOST SERIOUS RECORD

Of the numerous topics covered in the NLSY97, the section on *Crime and Substance Use* is the primary focus of this research, given its relevance to Clean Slate policies. Each survey year, respondents provide information on their arrests, convictions, and incarceration or other post-conviction sentencing.[40]

The influence of legal system involvement on an individual's employment prospects and income may vary according to the severity of the offense, as well as other factors. Offense severity, as it is typically referred to in criminal-legal context, can be generally defined hierarchically, with violent felony convictions considered most severe, followed by non-violent felony convictions, misdemeanor convictions, and arrests without conviction.

To quantify the earning differences associated with records of each severity level, in alignment with CSI's Data Model,[41] we classify survey respondents according to the most severe offense they reported in an arrest and/or conviction. These categorizations will be referred to as *treatment groups* moving forward. Any survey respondent with no reported involvement in the legal system will be included in our potential *comparison group*.

We utilized arrest and incarceration summary measures, offense-specific arrest and conviction flags, and post-conviction sentencing information where applicable to determine the most severe conviction type a survey respondent self-reported before the age of 26. Doing so ensures that treatment status is established before the earnings outcome window used in the main analysis, which begins at age 26. It also enables us to measure earning differences over a fourteen year follow-up period, capturing longer-term trends if they exist.

The NLSY97 asks respondents whether they have been arrested and/or convicted of twelve specific offenses, including assault, robbery, burglary, theft, destruction of property, possession or use of illicit drugs, sale or trafficking of illicit drugs, major traffic offenses, public order offenses, and other offenses. These offense categories provide the foundation for classifying respondents into broader arrest or conviction record severity groups.

Offenses Covered in the National Longitudinal Survey of Youth 1997

ASSAULT
An attack with a weapon or your hands, such as battery, rape, aggravated assault, or manslaughter

ROBBERY
Taking something from someone using a weapon or force

BURGLARY
Burglary and breaking and entering: breaking into private property without permission in order to steal

THEFT
Stealing something without the use of force, such as auto theft, larceny, or shoplifting

DESTRUCTION OF PROPERTY
Vandalism, arson, malicious destruction, or shoplifting

OTHER PROPERTY CRIME
Fencing, receiving, possessing, or selling stolen property

POSSESSION OF ILLEGAL DRUGS
Possession or use of illicit drugs

SELLING ILLEGAL DRUGS
Sale or trafficking of illicit drugs

MAJOR TRAFFIC OFFENSE
Driving under the influence of alcohol or other drugs, reckless driving, or driving without a license

PUBLIC ORDER OFFENSE
Drinking or purchasing alcohol while under the legal age, disorderly conduct, or a sex offense

OTHER OFFENSE
Any other offense not mentioned in the survey

PAROLE/PROBATION VIOLATION
Not directly defined in the survey. For our analysis we excluded these violations as they do not constitute a new conviction.

CSI's Data Model categorizes convictions for Driving Under the Influence (DUI) and simple assault as hard-to-clear misdemeanor convictions. Based on the definitions of the offenses provided in the NLSY97, we cannot definitively determine which respondents were convicted of these specific offenses. Therefore, we do not include a separate category for hard-to-clear misdemeanor convictions in this analysis. **The final categories constructed from the survey data are arrest or non-conviction, misdemeanor conviction, non-violent felony conviction, and violent felony conviction records.**

The motivation behind breaking out violent and non-violent felony convictions is that violent felony offenses are typically not eligible for automated sealing under Clean Slate policies, and we assume that the impact of having a violent felony conviction on employability and earnings potential would be meaningfully different from a non-violent felony conviction.

Offenses involving force or threat against a person, including assault and robbery, were classified as violent felonies. Serious property and drug offenses without interpersonal violence were classified as non-violent felonies. Lower-level property offenses, major traffic offenses, public order offenses, and other offenses were classified as misdemeanors.

In cases where respondents reported multiple offenses, classification was based on the single most severe arrest or conviction observed before age 26, regardless of offense frequency.

Definitions of Treatment Groups Using Offense-level Conviction Flags

Arrest or Non-Conviction Only

Reported an arrest for any offense type, but no subsequent conviction or incarceration.

Misdemeanor Conviction

Reported a conviction for either:

- Major traffic offense
- Other property crime
- Public order offense
- Other offense

and no convictions were reported for felony offenses, violent or non-violent.

Non-Violent Felony Conviction

Reported a conviction for either:

- Burglary
- Theft
- Destruction of property
- Selling illegal drugs
- Possession of illegal drugs

and no convictions were reported for violent felony offenses.

Violent Felony Conviction

Reported an assault or robbery conviction (regardless of post-conviction sentencing data).

In addition to classifying the most severe offense a person self-reported before age 26, several other critical conditions must be controlled for before conducting Propensity Score Matching.

1. To isolate the contribution of the treatment on one's earnings, we must **ensure that later legal system involvement does not obscure the relationship between earlier record status and future earnings**. New involvement with the legal system could further depress future earnings and make it difficult to interpret the association between the original arrest or conviction record and subsequent earnings.

2. **Exclude legal system activity that would likely be charged in juvenile court**, as most states seal juvenile records from public view. For arrests and misdemeanor convictions, we excluded activity that occurred before the respondent was 18 years of age. For felony convictions, we allowed for some juvenile activity due to transfer laws and automatic charging as an adult for more serious offenses.[42]

3. For a valid comparison with our matched comparison group, **a survey respondent must have provided data from before their initial involvement with the legal system**. This pre-intervention data is essential for comparing attributes between the treatment and comparison groups without relying on characteristics measured after treatment occurred.

Data that were inconsistent or incomplete were excluded from both the treatment and comparison groups. All covariates used in the propensity score models are drawn from survey rounds preceding the respondent's most severe arrest or conviction to avoid post-treatment bias.

FINDING STATISTICALLY SIMILAR PEERS WITHOUT RECORDS FOR COMPARISON

Comparing the earnings of our treatment groups (those with arrest or conviction records) and our comparison group directly would bias the estimation of treatment effects, as involvement in the legal system is not randomly assigned. To address this issue, we used Propensity Score Matching (PSM) to match survey respondents in the treatment groups with statistically similar respondents in the comparison group based on a variety of personal factors. PSM estimates the probability of group membership, also referred to as the propensity score. Those with similar scores are considered statistically comparable and can be matched for analysis.[43]

We establish a relationship between various covariates and the treatment assignment using statistical models to estimate the propensity score. Please refer to Appendix A for more details on model selection and validation.

The final covariates we include in our model are:

- Demographic characteristics (race/ethnicity, gender, and age)
- Educational attainment and general health indicators
- Survey year and region of residence fixed effects

This research aims to quantify the nationwide impact of arrest and conviction records and to provide state-level estimates for all 50 states and the District of Columbia.[44] In every survey year, a respondent is asked to report the U.S. Census region in which they reside: Northeast, North Central, South, or West. A regional analysis can reveal variation in the earnings differences associated with arrest or conviction records as a product of broader labor-market and macroeconomic conditions, including workforce and job-market demands, differences in wages and cost of living, and general perceptions of criminality and people with records attempting to reenter the workforce. **However, a regional analysis cannot capture the differences in practices and policies that may exist across individual states.**

EXAMPLE SURVEY QUESTION:

Census region of the residence as of the survey date

Northeast
CT, ME, MA, NH, NJ, NY, PA, RI, VT

North Central
IL, IN, IA, KS, MI, MN, MO, NE, OH, ND, SD, WI

South
AL, AR, DE, DC, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV

West
AK, AZ, CA, CO, HI, ID, MT, NV, NM, OR, UT, WA, WY

Although region was included as a covariate in the national propensity score model, that does not guarantee that a good match nationally is necessarily a good match within every region. We therefore examined whether regional matching could be conducted using the same methodological decisions applied in the national estimates. Ultimately, we did not continue with separate regional models because small sample sizes produced unstable outcomes. Instead, we segmented the matched treatment and comparison respondents from the national model by region of residence in the survey years following treatment to estimate regional earning differences.

This approach reflects the fact that while treatment occurred at a specific place and point in time, earnings losses may accrue in the labor markets where individuals reside and work in subsequent years, which can differ from where the arrest or conviction occurred. **A person's employment and earnings may be influenced by their arrest or conviction record irrespective of where the record originated, primarily due to the widespread use of commercial background checks and the ability of credit reporting agencies to connect records across jurisdictions.**

These regional estimates should therefore be interpreted as reflecting the earning differences experienced by individuals residing in a given region, rather than the effect of a region's policies or practices. Our national estimates serve as the primary benchmark and provide the overall average earning differences associated with each treatment group.

To estimate disparities in earnings across treated individuals by race and sex, we also estimated separate PSM models for Black, white, Hispanic, male, and female respondents. These subgroup analyses follow the same general methodological framework but should be interpreted with appropriate caution because splitting the sample by race or sex reduces sample size and may increase statistical uncertainty.

MEASURING DIFFERENCES IN EARNINGS AND EMPLOYMENT

Once suitable treatment-comparison pairs were identified, we retrieved earnings and employment data from the *Income* and *Employment* topic areas of the survey. The NLSY97 collects detailed information on respondents' pre-tax annual income and weekly employment histories across all survey waves.

In cases where respondents did not report an exact income amount, the survey administrators collected bracketed income responses, ranging from $1–$5,000 to $100,001–$250,000 and "more than $250,000." For these observations, we imputed income using the midpoint of the reported bracket. For the highest open-ended income category, we assigned a fixed top value of $250,000. This approach preserves sample size while minimizing the loss of information from missing exact income values.

Reported income values from the survey are not adjusted for inflation. We therefore adjusted all income measures to 2021 dollars using Consumer Price Index (CPI) data obtained from the Bureau of Labor Statistics (BLS) Data API. Because this study uses the public-use NLSY97 data, inflation adjustment was conducted using the available year and regional information rather than state-level price indices.

EXAMPLE SURVEY QUESTION:

Gross Income

During 1996, how much income did you receive from wages, salary, commissions, or tips from all jobs, before deductions for taxes or anything else?

Weekly employment status arrays were used to determine whether a respondent was employed in a given survey year and to compute the total number of weeks worked during that year. A respondent was classified as employed if they reported employment activity and positive earnings during the survey year.

We used a two-step modeling approach to more accurately estimate how arrest and conviction records are associated with earnings over time. Instead of looking only at reported earnings, which can be misleading because many people have zero earnings in a given year. We separated the process into two parts: first, estimating the likelihood that someone is employed at all, and second, estimating how much they earn if they are employed. We then combined these two pieces to calculate an overall expected level of annual earnings for every individual, including those who are not working. This approach allows us to avoid bias that would come from ignoring non-workers or mixing together different economic processes.[45] The final earnings differences we report reflect both reduced access to employment and lower wages among those who do work, providing a more complete picture of the economic impact of having a record.

All analyses were restricted to respondents observed between ages 26 and 40. We restrict our analysis to these ages to ensure that earnings are measured after treatment status has been established and to maintain sufficient longitudinal earnings data across the survey period.

INCORPORATING THE CLEAN SLATE INITIATIVE'S DATA MODEL TO ESTIMATE ECONOMIC LOSSES

To translate individual-level earning differences into aggregate economic impacts, we combine our estimated differences in earnings by record severity with population estimates provided by CSI's Data Model.[46]

To estimate national annual earnings losses, we multiply the average earnings penalty, the baseline earnings from the comparison group, and the nationwide population with an arrest or conviction record provided by CSI's Data Model.

To estimate state losses, we multiply the average regional earnings penalties, the regional baseline earnings from the comparison group, and the state population with an arrest or conviction record provided by CSI's Data Model. For additional information on regional earnings losses, refer to Appendix B.

This approach assumes that the earnings penalty associated with arrest or conviction record severity is broadly similar across states within the same Census region. It does not assume that state policies, labor markets, or record-sealing practices are identical. Rather, regional estimates are used because they are the most granular geographic estimates available from the public-use NLSY97 data. State-level estimates should therefore be interpreted as modeled estimates of economic losses among state residents with records, not as direct causal estimates from state-level survey data.

Appendix

Appendix A. Additional Methodology and Considerations

Throughout this analysis, we evaluated several alternative methodological approaches before selecting the final classification and modeling strategy described in the Methodology section of this report.

This Appendix summarizes the additional analyses, variable construction decisions, and validation checks considered during the research process, particularly those related to offense classification, treatment group construction, and propensity score model optimization.

POST-CONVICTION SENTENCING DATA IN THE NLSY97

Before deciding to rely solely on offense types to determine record severity and treatment groups, we attempted using post-conviction sentencing information to help determine the likely severity of the offense, particularly for those that could be classified as either a misdemeanor or felony.[47] We relied on research from the National Conference of State Legislators,[48,49] and the Pew Research Center[50] to determine appropriate sentencing length for each offense severity.

Generally speaking, we determined that a misdemeanor offense has a maximum imprisonment sentence of one year and a probation sentence of two years,[51] anything longer than these guidelines is deemed a felony conviction. The survey collects data on the month and year a respondent began and ended correctional and/or probation supervision, in addition to having monthly incarceration arrays. We used both of these data points to construct the supervision length as accurately as possible.

Additionally, misdemeanor convictions can be sentenced to community service, and fines and fees (which is not explicitly asked by the NLSY97).[52] Therefore, if an offense category was ambiguous, a respondent did not report any correctional or community supervision, but did indicate a community service or other sentence, we attempted to categorize these convictions as likely misdemeanors.

However, we found that post-conviction sentencing trends were inconsistent across offense types, see Appendix Tables 1 and 2. For example, based on the definition of violent felony offenses in the NLSY97, we expected almost all respondents to have reported a prison sentence due to the interpersonal violence associated with these charges. However, we see a decent percent of violent felony convictions being sentenced to lower-level sentences like jail, community service, or other sentences not directly defined in the survey.

Additionally, we have little understanding of how respondents are differentiating jail from prison sentences. As the question is posed in the NLSY97 documentation, we assume that recorded jail sentences are not for pre-trial detention. However, given the high number of occurrences reporting jail sentences across all offense types, we remain unsure whether respondents or survey administrators fully understand the differences between jail and prison sentences. Based on these inconsistent trends, we decided to rely on offense types alone when creating our treatment groups and not post-conviction sentences information.

EXAMPLE SURVEY QUESTION:

Sentenced to corrections institute or perform community service?

Were you sentenced to spend time in a corrections institution, like a jail, prison or a youth institution like juvenile hall or reform school or training school or to perform community service?

UNIVERSE: Respondent has been arrested; has been charged with offense by police; went to juvenile/adult court; was convicted of or plead guilty to charges; convicted of 1 or more offense(s).

Response Options

- Not sentenced to a corrections institution
- Jail
- Adult corrections institution
- Juvenile corrections institution
- Reform school or training school
- Community service
- Other

Appendix Table 1. Distribution of Sentencing Types by Conviction Offense in Early Adulthood

Offense Category	Adult Prison[53]	Jail	Probation	Community Service	Juvenile Corrections	Reform or Training School	Other Sentence Not Explicitly Included in Question	Number of Conviction Events Reported by Respondents
Violent Felony Offenses	16.8%	25.6%	9.3%	9.1%	13.9%	1.1%	21.4%	992
Burglary	19.1%	21.3%	6.0%	9.2%	15.1%	2.2%	23.8%	403
Theft	8.6%	23.3%	8.6%	12.7%	14.5%	2.1%	25.1%	661
Destruction of Property	3.1%	22.1%	12.7%	11.8%	16.3%	3.6%	26.6%	417
Possession of Illegal Drugs	15.8%	28.2%	12.8%	9.7%	4.8%	1.1%	21.9%	1,023
Selling Illegal Drugs	28.2%	25.0%	8.4%	4.2%	6.1%	1.3%	18.7%	380
Major Traffic Offense	7.5%	29.6%	18.3%	11.5%	3.1%	0.6%	25.2%	1,149
Public Order Offense	8.1%	17.9%	28.2%	13.3%	6.9%	0.6%	21.8%	670
Other Property Crimes	15.4%	18.8%	8.1%	9.0%	19.2%	2.6%	23.9%	234
Other Offense	14.4%	25.2%	16.3%	9.9%	7.9%	1.1%	20.8%	1,426

**Note that the percentages presented in the above table may not add up to 100% across offense categories because of survey respondent non-responsiveness.*

Appendix Table 2. Distribution of Sentencing Types by Most Severe Conviction during Early Adulthood

Offense Category	Adult Prison[54]	Jail	Probation	Community Service	Juvenile Corrections	Reform or Training School	Other Sentence Not Explicitly Included in Question	Number of Conviction Events Reported by Respondents
Violent Felony Conviction	17.3%	26.3%	20.6%	9.3%	13.3%	1.0%	9.7%	874
Non-violent Felony Conviction	16.2%	26.2%	23.1%	11.7%	3.8%	1.2%	11.8%	1,384
Misdemeanor Conviction	9.3%	23.9%	22.5%	12.0%	2.4%	0.3%	24.1%	1,658

**Note that the percentages presented in the above table may not add up to 100% across offense categories because of survey respondent non-responsiveness*

PROPENSITY SCORE MODEL SELECTION AND VALIDATION

We initially tested three reputable models for our Propensity Score Matching (PSM) procedure: Logistic Regression, Random Forest, and Boosted Regression. Logistic Regression models tend to be used most often in these methods, mainly because of their linear and interpretable outputs. However, compared to the other two models, Logistic Regression can be less able to capture non-linear relationships. Random Forest and Boosted Regression are both ensemble methods that are better suited to identifying complex and non-linear patterns but can be more challenging to interpret.

We conducted feature importance review, parameter tuning, and model evaluation using metrics such as ROC-AUC and out-of-sample accuracy. However, given that treatment is rare in our dataset, predictive accuracy alone does not necessarily indicate the success of identifying treatment; it may instead indicate the success of identifying comparison respondents. Ultimately, the deciding factors for model selection were the post-match sample size and post-match balance, as measured by the Standardized Mean Difference (SMD).

SMD is a statistical metric used to measure the difference between two groups in standardized units. Consistent with prior literature, SMDs of 0.2, 0.5, and 0.8 are commonly interpreted as small, medium, and large imbalances, respectively.[55] Negative SMDs indicate that the comparison group has a higher concentration or average value for a given variable, while positive SMDs indicate the treatment group has a higher concentration or average value.

After each model computed propensity scores for respondents in our sample, we measured similarity using nearest-neighbor matching with a caliper to exclude poorly matched treatment-comparison pairs. We implemented nearest-neighbor matching with replacement, allowing comparison respondents to be matched to multiple treated respondents to preserve sample size in smaller treatment groups. The final model was selected because it provided the best balance of predictive performance, post-match balance, and retained sample size. See Appendix Table 3 for SMDs and sample sizes across all models and treatment groups.

After evaluating multiple propensity score models and optimizing jointly on sample retention and post-match balance, we selected the Random Forest model to estimate propensity scores for all matching analysis. Random Forest models were used in our race and sex specific PSM procedures as well. We conducted the same testing and validation procedures described above for these PSMs as we did for the national model.

Below are tables outlining the SMDs and post-match treatment sample sizes used to determine the optimal matching model (Appendix Table 3), the balance across key covariates from the three main models for each treatment group (Appendix Tables 4 - 7), final post-match balance across key covariates (Appendix Tables 8 - 11), and post-match sample descriptive statistics (Appendix Table 12).

Appendix Table 3. Model Outputs for Propensity Score Matching Across All Treatment Groups

Model	Treatment Group	Average Absolute Standard Mean Difference (SMD)		Post-Match Treatment Sample Sizes
		Pre-Match	Post-Match	
Logistic Regression (Without Replacement)	Non-conviction	0.2419	0.0817	963
	Misdemeanor Conviction	0.2418	0.0554	447
	Non-violent Felony Conviction	0.3319	0.1406	472
	Violent Felony Conviction	0.3999	0.1944	378
Random Forest (With Replacement) FINAL SELECTED MODEL	Non-conviction	0.2419	0.0415	735
	Misdemeanor Conviction	0.2418	0.0388	375
	Non-violent Felony Conviction	0.3319	0.0537	306
	Violent Felony Conviction	0.3999	0.0574	198
Boosted Regression (With Replacement)	Non-conviction	0.2419	0.0598	734
	Misdemeanor Conviction	0.2418	0.0496	373
	Non-violent Felony Conviction	0.3319	0.0891	306
	Violent Felony Conviction	0.3999	0.0907	198

Appendix Table 4. Arrest or Non-conviction Treatment Group Post-Match SMD Across Three Tested Models

Variable	Standardized Mean Difference (SMD)		
	Logistic Regression	Random Forest	Boosted Regression
Age (Months)	-0.2584	0.0300	0.1366
Highest Grade Completed	0.1301	0.0501	0.1037
Excellent Health	0.0990	-0.0450	0.0045
Very Good Health	-0.0247	-0.0068	-0.0161
Good Health	-0.0113	0.0165	-0.0172
Fair Health	-0.0823	0.0519	0.0339
Poor Health	-0.0489	0.0349	0.0666
White	0.0804	0.0802	0.0220
Black	-0.1000	-0.0502	-0.0629
Hispanic	0.0128	-0.0405	0.0136
Other	0.0170	0.0080	0.0767
North Central	-0.0077	0.0766	0.0702
Northeast	0.0836	-0.0536	-0.0154
South	-0.0960	-0.0561	-0.1181
West	0.0532	0.0474	0.0981
Female	-0.0153	-0.0064	0.0736
Male	0.0153	0.0064	-0.0736
Average Absolute SMD	**0.0817**	**0.0415**	**0.0598**

Appendix Table 5. Misdemeanor Conviction Treatment Group Post-Match SMD Across Three Tested Models

Variable	Standardized Mean Difference (SMD)		
	Logistic Regression	Random Forest	Boosted Regression
Age (Months)	-0.1510	0.0866	0.1161
Highest Grade Completed	-0.0079	0.0427	0.0319
Excellent Health	0.0047	-0.0026	0.0226
Very Good Health	-0.0428	-0.0376	-0.0190
Good Health	0.0633	-0.0085	0.0085
Fair Health	-0.0389	0.0837	-0.0270
Poor Health	0.0669	-0.0050	0.0732
White	-0.0691	0.0225	-0.1222
Black	0.0752	0.0261	0.0382
Hispanic	0.0056	-0.0361	0.0872
Other	0.0277	-0.0331	0.0716
North Central	-0.0482	0.0414	-0.0601
Northeast	-0.0064	0.0084	0.0890
South	0.0096	-0.0548	0.0091
West	0.0484	0.0091	-0.0149
Female	0.0409	0.0610	0.0919
Male	-0.0409	-0.0610	-0.0919
Average Absolute SMD	**0.0554**	**0.0388**	**0.0496**

Appendix Table 6. Non-violent Conviction Treatment Group Post-Match SMD Across Three Tested Models

Variable	Standardized Mean Difference (SMD)		
	Logistic Regression	Random Forest	Boosted Regression
Age (Months)	-0.4964	0.0294	0.0986
Highest Grade Completed	0.2056	0.0203	0.0775
Excellent Health	0.0944	-0.0885	-0.0403
Very Good Health	-0.0839	0.0060	-0.0902
Good Health	0.0199	0.0005	0.0387
Fair Health	0.0139	0.1018	0.0877
Poor Health	-0.1056	0.0497	0.1820
White	0.1754	0.0332	0.0204
Black	-0.1739	0.0054	-0.0568
Hispanic	0.0111	-0.0318	-0.0106
Other	-0.0483	-0.0334	0.1615
North Central	0.1182	0.0446	0.0448
Northeast	0.0555	-0.0510	0.0361
South	-0.1879	-0.0168	-0.0253
West	0.0629	0.0190	-0.0475
Female	0.0421	0.1707	0.2375
Male	-0.0421	-0.1707	-0.2375
Average Absolute SMD	**0.1406**	**0.0537**	**0.0891**

Appendix Table 7. Violent Felony Conviction Treatment Group Post-Match SMD Across Three Tested Models

Variable	Standardized Mean Difference (SMD)		
	Logistic Regression	Random Forest	Boosted Regression
Age (Months)	-0.8984	0.1658	0.1738
Highest Grade Completed	0.3680	-0.0127	-0.1128
Excellent Health	0.1752	-0.0440	0.0769
Very Good Health	-0.0809	0.0148	0.0119
Good Health	-0.0627	0.1191	-0.0802
Fair Health	0.0612	-0.0952	-0.0556
Poor Health	-0.1865	-0.0505	0.1750
White	0.0494	0.0631	-0.0533
Black	-0.0217	-0.0061	-0.0941
Hispanic	-0.0504	-0.0341	0.1433
Other	0.0569	-0.0852	0.1750
North Central	0.0997	0.0117	-0.0344
Northeast	0.0280	-0.0053	0.1126
South	-0.0544	-0.0030	-0.0433
West	-0.0551	-0.0032	-0.0051
Female	-0.0800	0.0793	0.1126
Male	0.0800	-0.0793	-0.1126
Average Absolute SMD	**0.1944**	**0.0574**	**0.0907**

Appendix Table 8. Arrest or Non-conviction Treatment Group

Pre- and Post-Match SMD using RandomForest Model

Variable	Standardized Mean Difference (SMD)	
	Pre-Match	Post-Match
Demographic Characteristics		
Black	0.1908	-0.0502
White	-0.1605	0.0802
Hispanic	-0.0005	-0.0405
Other Race	-0.0303	0.0080
Male	0.5357	0.0064
Female	-0.5357	-0.0064
Age (Months)	-0.8374	0.0300
School & Health		
Excellent Health	-0.0518	-0.0450
Very Good Health	-0.1281	-0.0068
Good Health	0.1305	0.0165
Fair Health	0.1018	0.0519
Poor Health	0.0116	0.0349
Highest Grade Completed	-0.9033	0.0501
Regional Indicators		
North Central	-0.0372	0.0766
Northeast	-0.0090	-0.0536
South	0.1007	-0.0561
West	-0.0752	0.0474
Average Absolute SMD	**0.2419**	**0.0415**

Appendix Table 9. Misdemeanor Conviction Treatment Group

Pre- and Post-Match SMD using RandomForest Model

Variable	Standardized Mean Difference (SMD)	
	Pre-Match	Post-Match
Demographic Characteristics		
Black	-0.1996	0.0261
White	0.2032	0.0225
Hispanic	-0.0183	-0.0361
Other Race	-0.0710	-0.0331
Male	0.7027	-0.0610
Female	-0.7027	0.0610
Age (Months)	-0.6171	0.0866
School & Health		
Excellent Health	0.0274	-0.0026
Very Good Health	-0.0942	-0.0376
Good Health	0.0097	-0.0085
Fair Health	0.1162	0.0837
Poor Health	-0.0375	-0.0050
Highest Grade Completed	-0.7574	0.0427
Regional Indicators		
North Central	0.1906	0.0414
Northeast	-0.0781	0.0084
South	-0.1224	-0.0548
West	0.0083	0.0091
Average Absolute SMD	**0.2418**	**0.0388**

SMD Thresholds: Medium 0.5 - 0.8 Large > 0.8

Appendix Table 10. Non-violent Treatment Group

Pre- and Post-Match SMD using RandomForest Model

Variable	Standardized Mean Difference (SMD)	
	Pre-Match	Post-Match
Demographic Characteristics		
Black	0.1434	0.0054
White	-0.0366	0.0332
Hispanic	-0.0780	-0.0318
Other Race	-0.0801	-0.0334
Male	0.8470	-0.1707
Female	-0.8470	0.1707
Age (Months)	-1.0444	0.0294
School & Health		
Excellent Health	-0.0660	-0.0885
Very Good Health	-0.0536	0.0060
Good Health	-0.0061	0.0005
Fair Health	0.1864	0.1018
Poor Health	0.0904	0.0497
Highest Grade Completed	-1.3387	0.0203
Regional Indicators		
North Central	0.1222	0.0446
Northeast	-0.0695	-0.0510
South	0.0537	-0.0168
West	-0.1329	0.0190
Average Absolute SMD	**0.3319**	**0.0537**

Appendix Table 11. Violent Conviction Treatment Group

Pre- and Post-Match SMD using RandomForest Model

Variable	Standardized Mean Difference (SMD)	
	Pre-Match	Post-Match
Demographic Characteristics		
Black	0.3108	-0.0061
White	-0.2650	0.0631
Hispanic	0.0161	-0.0341
Other Race	-0.1064	-0.0852
Male	0.8095	-0.0793
Female	-0.8095	0.0793
Age (Months)	-1.3290	0.1658
School & Health		
Excellent Health	-0.0837	-0.0440
Very Good Health	-0.1821	0.0148
Good Health	0.1381	0.1191
Fair Health	0.2036	-0.0952
Poor Health	0.0698	-0.0505
Highest Grade Completed	-1.6667	-0.0127
Regional Indicators		
North Central	-0.0152	0.0117
Northeast	0.0120	-0.0053
South	-0.0154	-0.0030
West	0.0217	-0.0032
Average Absolute SMD	**0.3999**	**0.0574**

SMD Thresholds: Medium 0.5 - 0.8 Large > 0.8

Appendix Table 12. Post-Match Descriptive Descriptive Statistics of Comparison & Treatment Groups

Variable	Arrest & Non-conviction		Misdemeanor Conviction		Non-violent Felony Conviction		Violent Felony Conviction	
	Comparison	Treatment	Comparison	Treatment	Comparison	Treatment	Comparison	Treatment
Demographic Characteristics								
Black	35.9%	33.5%	15.8%	16.8%	35.7%	36.0%	41.2%	40.9%
White	37.4%	41.4%	58.4%	59.5%	39.2%	40.9%	31.9%	34.9%
Hispanic	22.9%	21.2%	22.3%	20.8%	21.6%	20.3%	24.2%	22.7%
Other Race	3.8%	3.9%	3.5%	2.9%	3.5%	2.9%	2.7%	1.5%
Male	64.5%	64.8%	76.2%	73.6%	85.9%	79.4%	82.4%	79.3%
Female	35.5%	35.2%	23.8%	26.4%	14.1%	20.6%	17.6%	20.7%
Age (Months)	247.5	248.2	249.4	251.4	247.4	248.1	243.5	247.1
School & Health								
Excellent Health	31.6%	29.5%	33.7%	33.6%	32.5%	28.4%	31.3%	29.3%
Very Good Health	32.7%	32.4%	34.3%	32.5%	31.1%	31.4%	29.1%	29.8%
Good Health	28.0%	28.7%	24.6%	24.3%	25.8%	25.8%	23.1%	28.3%
Fair Health	7.4%	8.8%	7.0%	9.3%	9.5%	12.7%	14.3%	11.1%
Poor Health	0.3%	0.5%	0.3%	0.3%	1.1%	1.6%	2.2%	1.5%
Highest Grade Completed	11.6	11.7	11.7	11.8	11.1	11.2	10.9	10.8
Regional Indicators								
North Central	17.5%	20.5%	29.0%	30.9%	22.6%	24.5%	19.2%	19.7%
Northeast	18.6%	16.6%	14.4%	14.7%	14.8%	13.1%	14.8%	14.6%
South	46.6%	43.8%	34.3%	31.7%	45.9%	45.1%	45.6%	45.5%
West	17.2%	19.0%	22.3%	22.7%	16.6%	17.3%	20.3%	20.2%

TWO-STEP MODEL AND CONFIDENCE INTERVALS

To estimate differences in earnings between treatment and comparison groups, we implemented a two-part (hurdle) modeling approach that explicitly separates the employment decision from the earnings process. This approach is well-suited to outcomes like annual earnings that contain a large mass at zero and a continuous distribution among positive values.[56] We did not rely solely on observed earnings because restricting the analysis to individuals with positive earnings would introduce selection bias, while including zero earners without adjustment would conflate two distinct processes: whether an individual is employed at all and how much they earn conditional on employment.

Our approach proceeds in two steps. First, we estimate a binary model predicting the probability of being employed in a given year for all respondents. Second, we estimate a continuous earnings model restricted to respondents with positive earnings, capturing expected earnings conditional on employment. Predicted probabilities of employment from the first stage and predicted conditional earnings from the second stage were combined to compute unconditional expected annual earnings for each individual. This yields an estimate of expected earnings for all respondents, including those not employed.

Differences in these expected values between treatment and comparison groups represent the average annual earnings penalty associated with criminal record status, incorporating both reduced likelihood of employment and lower earnings among those employed.

Unlike the Heckman selection model, a technique used by other researchers to mitigate selection bias,[57] the hurdle model does not impose a correction term based on an instrumental variable. This was a limitation of not having the restricted access data that includes state-level identifiers needed for establishing a strong instrument that could capture variance in economic indicators.

Confidence intervals for workforce participation metrics were calculated directly from the matched treatment-comparison sample. For employment rates and weeks worked, we estimated confidence intervals around treatment and comparison group averages and around the average matched-pair difference between each treatment group and its matched peers. Statistical significance is based on matched-pair tests of whether the average treatment-comparison difference differs from zero. These results are available in Appendix B, Tables 23 and 24.

For expected annual earnings, which combine estimates from both the employment and earnings models, we used a non-parametric bootstrap procedure. Specifically, we repeatedly resampled respondents, re-estimated the relevant earnings quantities, and recomputed expected earnings differences. This allowed us to construct confidence intervals that reflect uncertainty from both stages of the modeling process. The results with confidence intervals are presented in Appendix B, Tables 25 - 28.

Appendix B. Additional Results

This appendix provides additional methodological detail, descriptive analyses, and supporting results referenced throughout the main report.

It is intended to supplement the primary findings by documenting key analytical decisions, evaluating the performance of the matching procedures, and presenting additional subgroup and regional analyses not included in the main text.

SURVEY SAMPLE BEFORE MATCHING

This subsection presents descriptive statistics for the analytic sample prior to matching. The observed differences reflect both pre-existing disparities across groups that may be related to one's likelihood of interacting with the legal system and differences that emerge following legal system involvement. While the pre-match descriptive statistics are useful for illustrating how individuals with legal system involvement differ from the broader survey population, they do not represent the matched sample used to estimate earnings differences. The propensity score matching (PSM) procedure identifies a subset of individuals who are comparable to those in the treatment groups based on observable characteristics. As a result, the composition of the comparison group changes after matching. The post-match descriptive statistics therefore provide a more accurate representation of the analytic sample used in the earnings estimation.

Out of the 8,984 respondents surveyed in the NLSY97, we identified 3,212 respondents with some form of legal system involvement that would likely create an arrest or conviction record in alignment with our treatment definitions and 5,747 respondents with no reported involvement. After applying the constraints described in the methodology section, the final pre-match treatment sample included 963 respondents with arrest or non-conviction records, 447 respondents with misdemeanor conviction records, 472 respondents with non-violent felony conviction records, and 387 respondents with violent felony conviction records.

Arrest or non-conviction records was the largest treatment group, while each conviction record treatment group included roughly 400 to 500 respondents after constraints were applied. Extending the analysis to smaller subgroups, such as Census regions, race groups, or sex groups, further reduces effective sample sizes, particularly for felony conviction record categories.[58]

Appendix Table 13. Treatment Group Sample Sizes, National

Treatment Group	Average Absolute Standardized Mean Difference (SMD)	
	Pre-Match	Post-Match
Arrest or non-conviction	0.2419	0.0415
Misdemeanor Conviction	0.2418	0.0388
Non-violent Felony Conviction	0.3319	0.0537
Violent Felony Conviction	0.3999	0.0574

We conducted an exploratory analysis to determine which attributes may be more or less similar across the comparison and treatment groups before matching. At the aggregate level, individuals with arrest or conviction records differ from the comparison group along several key dimensions. A higher proportion of those with records are male, and they have lower levels of educational attainment prior to treatment (see Appendix Table 14 below). For example, the share of males is 40.5% in the comparison group, but nearly 80% in felony conviction groups, while average educational attainment was more than three grade levels lower among the violent felony conviction groups versus the comparison group.

At the same time, the racial composition shifts in non-uniform ways across treatment categories. Black respondents are overrepresented in more severe categories (e.g., 38.6% in violent felony compared to 24.3% in comparison), while white respondents are underrepresented in more severe record categories.

These aggregate patterns show that legal system involvement is not randomly distributed across the survey population and **underscore the importance of PSM as a way to reduce observable selection bias** before estimating earnings differences.

The ages of each treatment group also varied by design. As described in the methodology section, we excluded legal system activity that would likely be handled in juvenile court. For arrests and misdemeanor convictions, we excluded activity that occurred before the respondent was 18 years of age. For felony convictions, we allowed for some juvenile activity due to transfer laws and the possibility of more serious offenses being charged in adult court. As a result, treatment age differs somewhat across the four treatment categories. This imbalance is important to document before matching because age and educational attainment are closely related during early adulthood.

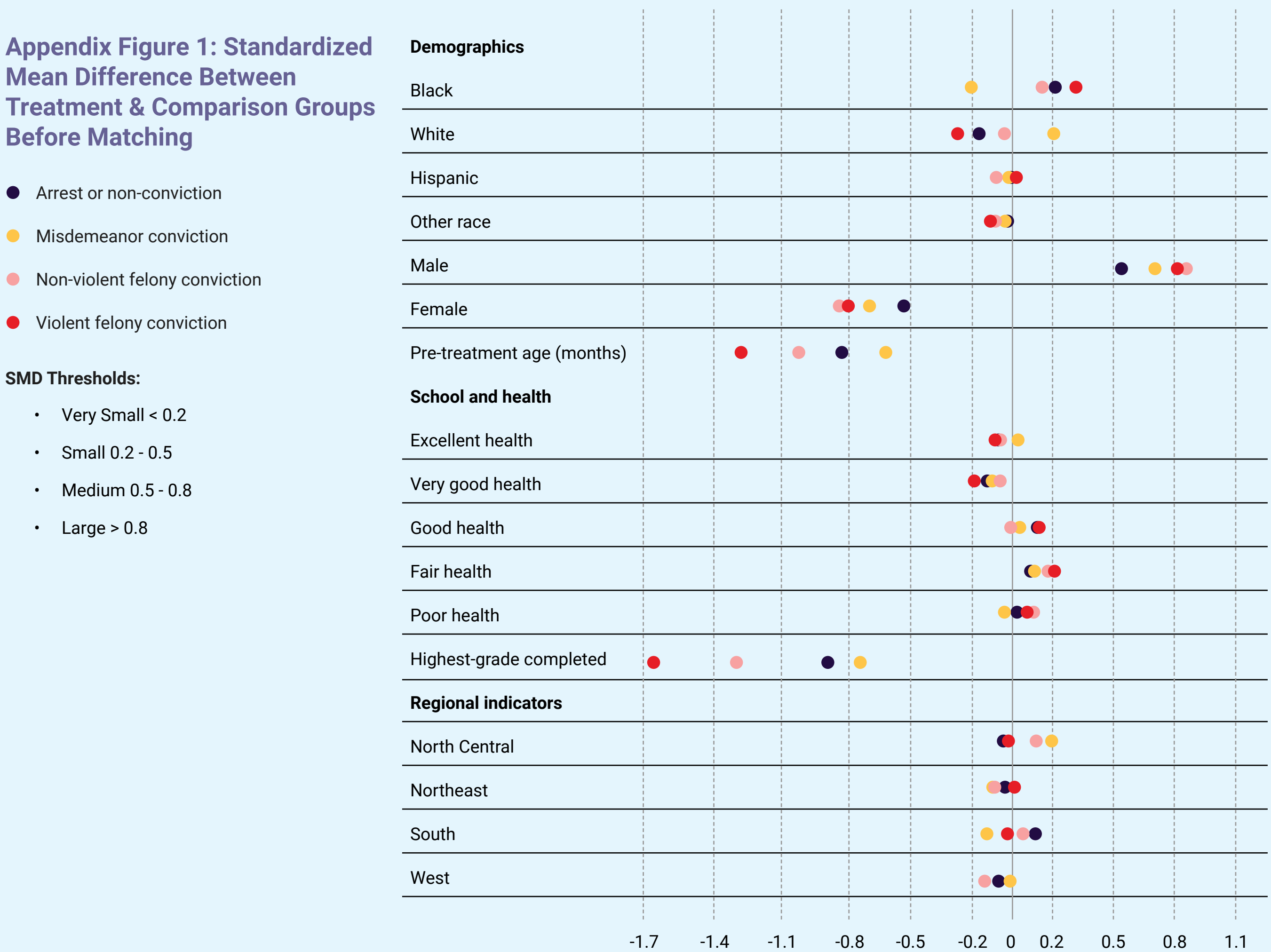
Appendix Table 14. Descriptive Statistics of Comparison & Treatment Groups Before Matching

Variable	Comparison	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Demographic Characteristics					
Black	24.3%	32.9%	16.3%	30.7%	38.6%
White	50.3%	42.4%	60.4%	48.5%	37.3%
Hispanic	21.1%	21.1%	20.4%	18.0%	21.8%
Other Race	4.2%	3.6%	2.9%	2.8%	2.3%
Male	40.5%	66.4%	73.4%	78.8%	77.5%
Female	59.5%	33.6%	26.6%	21.2%	22.5%
Pre-treatment Age (Months)	260.7	238.1	243.8	230.5	219.8
Pre-treatment School & Health					
Excellent Health	33.2%	30.7%	34.5%	30.1%	29.3%
Very Good Health	36.5%	30.4%	32.0%	33.9%	28.0%
Good Health	24.4%	30.2%	24.8%	24.2%	30.6%
Fair Health	5.5%	8.1%	8.5%	10.6%	11.1%
Poor Health	0.4%	0.5%	0.2%	1.3%	1.0%
Highest Grade Completed	12.9	11.2	11.5	10.4	9.6
Pre-treatment Regional Indicators					
North Central	21.9%	20.4%	30.2%	27.1%	21.2%
Northeast	17.2%	16.8%	14.3%	14.6%	17.6%
South	38.1%	43.0%	32.2%	40.7%	37.3%
West	22.9%	19.8%	23.3%	17.6%	23.8%

We assess covariate balance prior to matching using standardized mean differences (SMDs), where values closer to zero indicate better balance. Consistent with prior literature, SMDs of 0.2, 0.5, and 0.8 are commonly interpreted as small, medium, and large imbalances, respectively.[59] Negative SMDs indicate that the comparison group has a higher concentration or average value for a given variable, while positive SMDs indicate the treatment group has a higher concentration or average value.

As anticipated from the descriptive statistics in Appendix Table 14, the most imbalanced covariates prior to matching include sex, highest grade completed, age, and in some cases race. These findings indicate that a simple comparison of treated and untreated respondents would likely overstate or misstate earnings differences because treatment and comparison respondents differ in ways that may also be related to employment and earnings.

Appendix Figure 1: Standardized Mean Difference Between Treatment & Comparison Groups Before Matching



Examining these patterns across racial groups highlights disparities that are not apparent in aggregate comparisons alone. Similar to the aggregate statistics, treated respondents of all racial groups are more likely to be male, and have lower levels of educational attainment than their respective comparison groups. However, the extent of these differences varies considerably. Male representation was especially high among Black and Hispanic respondents in more severe conviction categories. Educational attainment also declined with record severity across racial groups, with respondents in felony conviction groups reporting lower average schooling than respondents in the comparison group.

Health differences are more modest than differences in sex, age, and education, but some patterns still emerge. In several treatment categories, respondents reported fair or poor health at higher rates than their respective comparison groups. These differences were especially relevant because health can affect both employment stability and earnings.[60]

Appendix Table 15. Descriptive Statistics of Comparison & Treatment Groups Before Matching, By Race

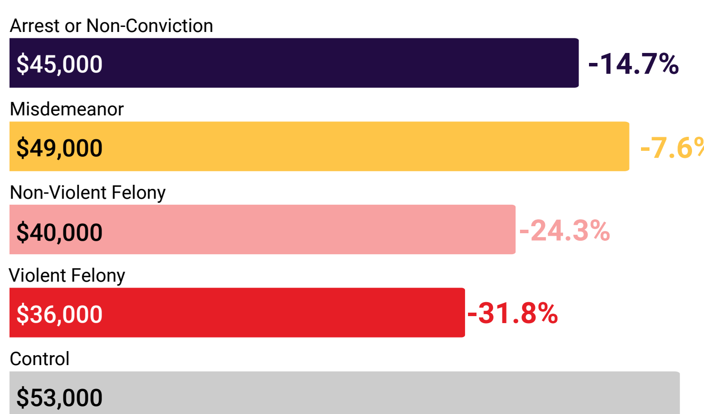
Variable	Comparison	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Sex: Male					
Black	34.4%	68.5%	75.3%	84.1%	78.5%
White	43.9%	61.0%	71.9%	75.5%	75.7%
Hispanic	38.8%	72.9%	76.9%	82.4%	81.0%
Pre-treatment Highest Grade Completed					
Black	12.6	11	11.2	10.5	9.7
White	13.3	11.4	11.6	10.4	9.8
Hispanic	12.4	11.1	12.3	10.4	9.4
Pre-treatment General Health: Fair or Poor					
Black	8.9%	10.7%	6.8%	11.7%	16.1%
White	4.2%	7.8%	8.1%	10.9%	9.7%
Hispanic	6.9%	7.9%	12.1%	12.9%	9.5%

Taken together, these pre-match findings point to a two-layered pattern in the data. First, rather than reflecting a random cross-section of the population, legal system involvement follows systematic patterns across key demographic characteristics.[61] Second, these patterns are not uniform across racial groups. Within each group, treatment respondents differ from their respective comparison populations along dimensions such as sex, educational attainment, and health, but the magnitude of these differences varies. In other words, while aggregate statistics show that individuals with legal system involvement differ from the broader population, the subgroup analysis highlights that these differences are more pronounced for some groups than others, and reveals that this disadvantage is systematically deeper and more concentrated for non-white populations.

Prior to matching, individuals with legal system involvement earn substantially less than their counterparts with no recorded involvement, see Appendix Figure 2. As one might expect, those with violent and non-violent felony conviction records have the lowest and second lowest average annual earnings, respectively. However, the arrest or non-conviction treatment group earn less than those with misdemeanor convictions, even though misdemeanor convictions are a more severe record category. This pattern likely reflects differences in the composition of the groups, including the higher share of white respondents and higher average educational attainment in the misdemeanor conviction group.

Average post-treatment earnings are 7% to 32% lower across treatment groups in the pre-match sample, with the largest raw gaps observed for respondents with violent and non-violent felony conviction records. These raw earnings differences suggest that individuals with arrest and conviction records face meaningful economic burdens. However, they do not account for underlying differences in demographic and socioeconomic characteristics, nor do they fully account for respondents with no reported earnings. For that reason, the raw differences should be interpreted as descriptive evidence rather than estimated earnings penalties.

Appendix Figure 2. Average Annual Post-treatment Earnings for Pre-matched Sample



PROPENSITY SCORE MATCHING PERFORMANCE AND SURVEY SAMPLE AFTER MATCHING

Pre-match statistics describe how treatment is distributed in the broader population, while post-match statistics define the sample used to estimate earnings differences. The pre-match descriptive analysis highlights substantial differences between respondents with arrest or conviction records and those without reported legal system involvement, underscoring that legal system involvement is non-random and that matching is necessary to improve comparability.

After evaluating multiple propensity score models and optimizing jointly on sample retention and post-match balance, we selected a Random Forest model to estimate propensity scores for all matching analysis. Before matching, the average absolute SMD across key covariates ranged from 0.2418 to 0.3999 across treatment groups. The arrest or non-conviction and misdemeanor conviction groups had lower overall imbalance, while the non-violent and violent felony conviction groups exhibited more pronounced pre-match imbalance. Even in the lower-imbalance groups, several key covariates, including sex, age, and highest grade completed, exhibited medium to large imbalances.

Appendix Table 16. Final Pre- and Post-Match Average Differences Across Key Covariates

Treatment Group	Average Absolute Standardized Mean Difference (SMD)	
	Pre-Match	Post-Match
Arrest or non-conviction	0.2419	0.0415
Misdemeanor Conviction	0.2418	0.0388
Non-violent Felony Conviction	0.3319	0.0537
Violent Felony Conviction	0.3999	0.0574

Following matching, the differences between treatment and comparison groups were substantially reduced across all observed characteristics (Appendix Tables 8 - 11), indicating that the PSM procedure is effective in constructing comparable groups. Within each treatment category, the distribution of race, gender, age, education, and health is closely aligned between treated and matched comparison respondents. For example, the share of males, average age, and average educational attainment are nearly identical across treatment and comparison groups within each category (Appendix Table 12). This suggests that the remaining differences in outcomes are less likely to be driven by observable demographic or socioeconomic factors and instead reflect differences associated with treatment status.

The final matched treatment samples also provide a clearer picture of the population used to estimate earnings differences. In our final matched treatment groups, we see,

- **More Black respondents within the highest severity treatment group than whites** (40.9% vs. 34.9%). There are higher proportions of white respondents in arrest, misdemeanor, and non-violent felony conviction treatment groups.
- **More male respondents in all treatment groups and overrepresentation worsens as severity of treatment increases**; ranging from 64.8% male to over 79% male.
- **The percent of respondents indicating fair or poor health prior to treatment is higher among those with more severe conviction records**; ranging from 9.3% for those with an arrest or conviction record to 12.6% - 14.3% for those with felony convictions.

Appendix Table 17. Descriptive Statistics of Treatment Groups Post Matching

Variable	Arrest & Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Demographic Characteristics				
Black	33.5%	16.8%	36.0%	40.9%
White	41.4%	59.5%	40.9%	34.9%
Hispanic	21.2%	20.8%	20.3%	22.7%
Other Race	3.9%	2.9%	2.9%	1.5%
Male	64.8%	73.6%	79.4%	79.3%
Female	35.2%	26.4%	20.6%	20.7%
Pre-treatment Age (Months)	248.2	251.4	248.1	247.1
Pre-treatment School & Health				
Excellent Health	29.5%	33.6%	28.4%	29.3%
Very Good Health	32.4%	32.5%	31.4%	29.8%
Good Health	28.7%	24.3%	25.8%	28.3%
Fair Health	8.8%	9.3%	12.7%	11.1%
Poor Health	0.5%	0.3%	1.6%	1.5%
Highest Grade Completed	11.7	11.8	11.2	10.8
Pre-treatment Regional Indicators				
North Central	20.5%	30.9%	24.5%	19.7%
Northeast	16.6%	14.7%	13.1%	14.6%
South	43.8%	31.7%	45.1%	45.5%
West	19.0%	22.7%	17.3%	20.2%

Appendix Table 18. Treatment-Comparison Group Sample Sizes, National PSM

Offense Severity	Treatment Pre-Match	Treatment Post-Match	Comparison Post-Match
Arrest or Non-conviction	963	735	554
Misdemeanor Conviction	447	375	315
Non-violent Felony Conviction	472	306	252
Violent Felony Conviction	387	198	165

RACE AND SEX SAMPLES

We ran separate PSM analyses for each race and sex group with sufficient sample sizes.[62] The purpose of these subgroup analyses was to measure disparities that would not otherwise be apparent in the overall national matching method.

After matching comparison and treatment respondents within these demographic groups, we begin to see trends across our treatment samples that were not otherwise apparent. Black and Hispanic respondents tended to have higher male representation and, in some cases, slightly lower average educational attainment than white respondents within the same record category. Differences in health also remained in certain cases, such as higher shares reporting fair or poor health among Black respondents in more severe conviction categories. These patterns indicate that while matching improves comparability between treated and comparison respondents within each analysis, it does not eliminate differences across racial groups within treatment categories. In other words, matching helps ensure that Black respondents with records are compared to similar Black respondents without records, and white respondents with records are compared to similar white respondents without records. It does not mean that Black, white, and Hispanic respondents with the same type of record have the same demographic, health, educational, or labor-market profile.

Appendix Table 19. Descriptive Statistics of Treatment Groups Post Matching, By Race

Variable	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Sex: Male				
Black	59.5%	72.2%	74.4%	74.4%
White	66.3%	78.5%	85.5%	79.6%
Hispanic	71.8%	74.4%	79.0%	82.2%
Pre-treatment Highest Grade Completed				
Black	11.9	12.0	11.4	11.1
White	11.4	11.4	11.0	10.8
Hispanic	11.6	11.6	11.0	10.4
Pre-treatment General Health: Fair or Poor				
Black	8.2%	9.4%	13.2%	8.5%
White	11.8%	4.6%	13.6%	14.0%
Hispanic	9.0%	12.8%	14.5%	11.1%

Segmenting our sample by sex also highlights differences that are not visible in the aggregate analysis. Female respondents in the arrest and misdemeanor conviction treatment groups reported fair or poor health prior to treatment at more than twice the rate of male respondents in the same groups. This difference was also present among respondents with non-violent felony convictions, though it was somewhat less pronounced. Among respondents with violent felony convictions, male and female respondents reported fair or poor health at more similar rates, but fewer female respondents reported excellent or very good health (42.6% of female respondents compared to 65.9% of male respondents).

These subgroup differences are important because they help explain why the economic consequences associated with arrest and conviction records may not be experienced evenly across communities.

Appendix Table 20. Descriptive Statistics of Treatment Groups Post Matching, By Sex

Variable	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Pre-treatment Highest Grade Completed				
Male	11.7	11.8	11.1	10.7
Female	11.7	11.8	11.4	10.9
Pre-treatment General Health: Fair or Poor				
Male	6.3%	6.5%	12.6%	11.7%
Female	15.1%	18.2%	20.6%	10.6%

As mentioned throughout the main report, the sample sizes for Hispanic and female respondents in our survey are small, especially as the severity of one's record increases. This likely causes some estimates to be less stable, and therefore results should be interpreted with caution and should not be overgeneralized without further analysis. The standard errors and confidence intervals reported in the following section are an indication of the variability in outcomes particularly for these two demographic groups. However, even with confidence intervals, these final reported workforce and earnings findings likely don't fully capture the lived experience of these groups and should be investigated further.

Appendix Table 21. Number of Respondents by Treatment Group, Race and Sex

Offense Severity	Final Sample Size				
	Black	White	Hispanic	Female	Male
Arrest or Non-conviction	246	304	156	259	476
Misdemeanor Conviction	65	223	78	99	278
Non-violent Felony Conviction	110	129	62	63	246
Violent Felony Conviction	93	82	45	47	179

WORKFORCE PARTICIPATION ESTIMATES, CONFIDENCE INTERVALS, AND STATISTICAL SIGNIFICANCE

In the main report, employment rates and weeks worked are presented as simple averages for people with arrest or conviction records and their matched peers without records. These averages are intended to describe the overall level of workforce participation observed in each group during prime working years.

For statistical inference, we also estimated differences across each matched treatment-comparison pair. This approach compares each person with an arrest or conviction record to their matched peer at the same age, then averages those pair-level differences. Because this estimator gives equal weight to matched treatment-comparison observations, it can differ from the simple difference between the two group averages shown in the main report. This is especially likely because comparison individuals are reused across matches and therefore treatment and comparison individuals have different numbers of observed person-years.

As a result, the "difference" shown in the main report should be interpreted as the difference between the overall treatment and comparison group averages. The matched-pair confidence intervals and p-values shown here should be interpreted as tests of whether the average within-pair difference is statistically distinguishable from zero.

For employment, the matched-pair results indicate that people with felony conviction records had statistically significantly lower annual employment rates than their matched peers. The gap was largest for people with violent felony convictions, followed by people with non-violent felony convictions. Smaller but statistically significant matched-pair differences were also observed for people with arrests or non-convictions and misdemeanor convictions.

For weeks worked, the estimates should be interpreted as differences among people with employment during the year. In other words, this measure captures the number of weeks worked conditional on having any employment, while the employment rate captures whether someone worked at all during the year. Together, these measures describe two related but distinct dimensions of workforce participation: attachment to employment and intensity of work among those employed.

Appendix Table 22. Annual Employment Rates and Matched-Pair Differences During Prime Working Years

Offense Severity	Employment rate	95% CI	Comparison Employment Rate	95% CI	Difference in group averages	Matched-pair difference	95% CI for matched-pair difference
Arrest or Non-conviction	84.2%	83.3%, 85.1%	85.7%	84.7%, 86.7%	-1.5 pps	-1.9 pps***	-3.4, -0.5 pps
Misdemeanor Conviction	90.4%	89.4%, 91.4%	90.5%	89.3%, 91.6%	-0.1 pps	-1.9 pps**	-3.6, -0.3 pps
Non-violent Felony Conviction	80.4%	78.8%, 81.9%	86.7%	85.3%, 88.2%	-6.4 pps	-6.9 pps***	-9.2, -4.5 pps
Violent Felony Conviction	77.7%	75.7%, 79.7%	86.0%	84.2%, 87.9%	-8.3 pps	-9.9 pps***	-12.9, -6.8 pps

*p < 0.10, **p < 0.05, ***p < 0.01

Appendix Table 23. Weeks Worked Among Employed People During Prime Working Years

Offense Severity	Weeks Worked	95% CI	Comparison Weeks Worked	95% CI	Difference in group averages	Matched-pair difference	95% CI for matched-pair difference
Arrest or Non-conviction	39.9	39.4, 40.4	43.7	43.2, 44.2	-3.8	-4.3 pps***	-5.1, -3.5 pps
Misdemeanor Conviction	40.6	40.0, 41.3	43.7	43.1, 44.4	-3.1	-2.4 pps***	-3.6, -1.3 pps
Non-violent Felony Conviction	38.5	37.7, 39.3	44.3	43.6, 45.0	-5.8	-4.7 pps***	-6.0, -3.3 pps
Violent Felony Conviction	36.7	35.7, 37.8	43.4	42.5, 44.3	-6.7	-5.0 pps***	-6.8, -3.3 pps

*p < 0.10, **p < 0.05, ***p < 0.01

BOOTSTRAPPED AVERAGES AND CONFIDENCE INTERVALS FOR EARNINGS LOSSES AND PENALTIES

As previously mentioned in Appendix A, we constructed confidence intervals for expected annual earnings by using a non-parametric bootstrap procedure. Specifically, we repeatedly resampled respondents, re-estimated the relevant earnings quantities, and recomputed expected earnings differences. This allowed us to construct confidence intervals that reflect uncertainty from both stages of the modeling process, employment and earnings.

These results show that earnings losses across all treatment groups can be more or less severe than the averages included in the main report, however, nearly all differences are negative. Even in the best case scenario, people with an arrest or conviction record are losing thousands of dollars in a given year, and tens of thousands across prime working years, compared to their peers without records.

Appendix Table 24. Expected Earning Differences

Bootstrapped Averages and Confidence Intervals

Offense Severity	Average	95% CI Lower Bound	95% CI Upper Bound
Arrest or Non-conviction	-$4,400	-$7,200	-$1,600
Misdemeanor Conviction	-$6,400	-$11,000	-$2,400
Non-violent Felony Conviction	-$9,300	-$13,000	-$5,600
Violent Felony Conviction	-$9,800	-$14,000	-$5,200

Appendix Table 25. Expected Earnings Penalties

Bootstrapped Averages and Confidence Intervals

Offense Severity	Average	95% CI Lower Bound	95% CI Upper Bound
Arrest or Non-conviction	-14.9%	-23.0%	-5.8%
Misdemeanor Conviction	-17.8%	-27.5%	-7.1%
Non-violent Felony Conviction	-31.8%	-41.8%	-21.0%
Violent Felony Conviction	-36.7%	-49.5%	-21.8%

Appendix Table 26. Cumulative Earnings Losses in Prime Working Years

Bootstrapped Averages and Confidence Intervals

Offense Severity	Average	95% CI Lower Bound	95% CI Upper Bound
Arrest or Non-conviction	-$72,000	-$116,000	-$27,000
Misdemeanor Conviction	-$102,000	-$169,000	-$37,000
Non-violent Felony Conviction	-$150,000	-$210,000	-$90,000
Violent Felony Conviction	-$159,000	-$232,000	-$84,000

Appendix Table 27. Expected Earnings Penalties, by Race and Sex

Bootstrapped Averages and Confidence Intervals

	Arrest & Non-conviction			Misdemeanor Conviction			Non-violent Felony Conviction			Violent Felony Conviction		
	Avg	CI Lower	CI Upper	Avg	CI Lower	CI Upper	Avg	CI Lower	CI Upper	Avg	CI Lower	CI Upper
Black	-18.7%	-33.8%	0	-39.0%	-55.9%	-18.0%	-43.1%	-59.3%	-22.9%	-51.8%	-66.5%	-33.2%
White	-27.5%	-38.0%	-15.5%	-23.9%	-35.2%	-11.3%	-31.9%	-45.4%	-15.7%	-30.6%	-50.6%	-5.6%
Hispanic	-9.1%	-25.6%	+10.9%	+2.1%	-21.1%	+30.2%	-22.2%	-43.4%	+4.4%	-35.3%	-58.2%	-6.5%
Male	-15.6%	-25.5%	-4.2%	-13.4%	-24.5%	-1.2%	-33.6%	-44.2%	-21.7%	-31.0%	-46.2%	-12.8%
Female	-24.7%	-37.5%	-9.8%	-33.0%	-48.9%	-11.9%	-49.1%	-65.0%	-27.9%	-49.4%	-69.1%	-21.4%

REGIONAL EARNINGS PENALTIES AND ECONOMIC LOSSES

Finally, we found that annual earnings penalties vary slightly across regions. People living in the North Central region experience the smallest earnings penalties across all treatment groups and regions. People living in the Northeast, South, and West experience the larger earnings penalties, though the pattern varied by treatment group.

For arrest or non-conviction records, the largest estimated penalties were in the Northeast and South, both at 15.9%. For misdemeanor convictions, the largest penalties were in the South at 19.4%, followed by the West at 18.9%. For felony convictions, the Northeast had the largest estimated penalties among respondents with non-violent felony convictions, while the Northeast and West showed especially large penalties among respondents with violent felony convictions.

Although percentage penalties were somewhat similar across regions, dollar losses differed because average earnings levels also vary by region. Respondents in the West experienced some of the largest annual dollar losses, ranging from $5,700 to $12,200 depending on record severity. People in the Northeast and South lose comparable annual earnings, however, people in the Northeast tend to lose more particularly for arrest and non-conviction records and non-violent felony convictions.

These trends suggest that there is meaningful regional variation in earnings losses associated with arrest and conviction records. These differences may reflect a combination of regional labor-market conditions, dominant industries, wage levels, cost of living, and employer practices. However, because the public-use NLSY97 does not include state identifiers, this analysis cannot determine which state or local policies may be driving regional differences.

Appendix Table 28. Expected Earnings Penalties of Matched Respondents, by Region of Residence

	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Northeast	-15.9%	-17.9%	-33.0%	-38.8%
North Central	-15.1%	-17.6%	-30.4%	-35.9%
South	-15.9%	-19.4%	-32.6%	-37.5%
West	-15.3%	-18.9%	-31.2%	-38.0%
Average Earnings Penalty	**-14.9%**	**-17.8%**	**-31.8%**	**-36.7%**

Appendix Table 29. Average Expected Earnings for Peers without Records, by Region of Residence

	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Northeast	$34,000	$36,000	$32,000	$28,000
North Central	$30,000	$39,000	$27,000	$25,000
South	$28,000	$33,000	$30,000	$28,000
West	$39,000	$44,000	$39,000	$32,000
Average Earnings Penalty	**$30,000**	**$36,000**	**$29,000**	**$27,000**

Aggregating CSI's state-level population estimates to Census regions reveals substantial geographic concentration in the economic losses associated with arrest and conviction.

The South contains the largest share of adults with an arrest or conviction record, with approximately 41% of the national justice-impacted population. The West has the second-largest share, followed by the North Central region and the Northeast.

Appendix Table 30. Percent Share of Population with Arrest or Conviction Records

Treatment Group	Northeast	North Central	South	West
Arrest or non-conviction	17.0%	19.5%	38.8%	24.7%
Misdemeanor Conviction	17.6%	17.9%	36.8%	27.7%
Non-violent Felony Conviction	11.6%	19.8%	46.7%	21.9%
Violent Felony Conviction	14.0%	20.0%	44.6%	21.4%
Any Arrest or Conviction	**15.4%**	**19.1%**	**40.9%**	**24.6%**

Appendix Table 31. Regional Annual Earnings Lost from Arrest or Conviction Records

	Arrest or Non-conviction	Misdemeanor Conviction	Non-violent Felony Conviction	Violent Felony Conviction
Northeast	$8.4 Billion	$30.5 Billion	$18.0 Billion	$8.5 Billion
North Central	$8.2 Billion	$32.7 Billion	$23.8 Billion	$10.3 Billion
South	$15.5 Billion	$62.2 Billion	$67.0 Billion	$26.6 Billion
West	$12.6 Billion	$62.2 Billion	$39.4 Billion	$14.7 Billion
Nationwide Cumulative Earnings Lost	**$44.7 Billion**	**$187.7 Billion**	**$148.2 Billion**	**$60.0 Billion**

The regional economic losses are not driven by regional earnings penalties alone. Regional annual earnings losses is a function of the regional earnings penalties, earnings of those in our survey sample without a record, and the population with an arrest or conviction record. Consequently, in regions where both earnings penalties and wages are high for those without records, the resulting losses to the regional economy can be significantly larger than a region with high earnings penalties but lower wages among those without records.

The South accounts for the largest share of aggregate losses, despite the region's lower earnings among individuals without records, it has the largest legal system–impacted population and substantial earnings penalties across offense types. However, while the South contains nearly 41% of people with records, it accounts for approximately 39% of national annual lost earnings. This means that the South's large share of total losses is driven mostly by its large affected population, rather than disproportionately large per-person penalties.

The estimated annual earnings losses across the four Census regions are:

$75.0B
17.0% of national total
North Central

$65.3B
14.8% of national total
Northeast

$129.0B
29.3% of national total
West

$171.3B
38.9% of national total
South

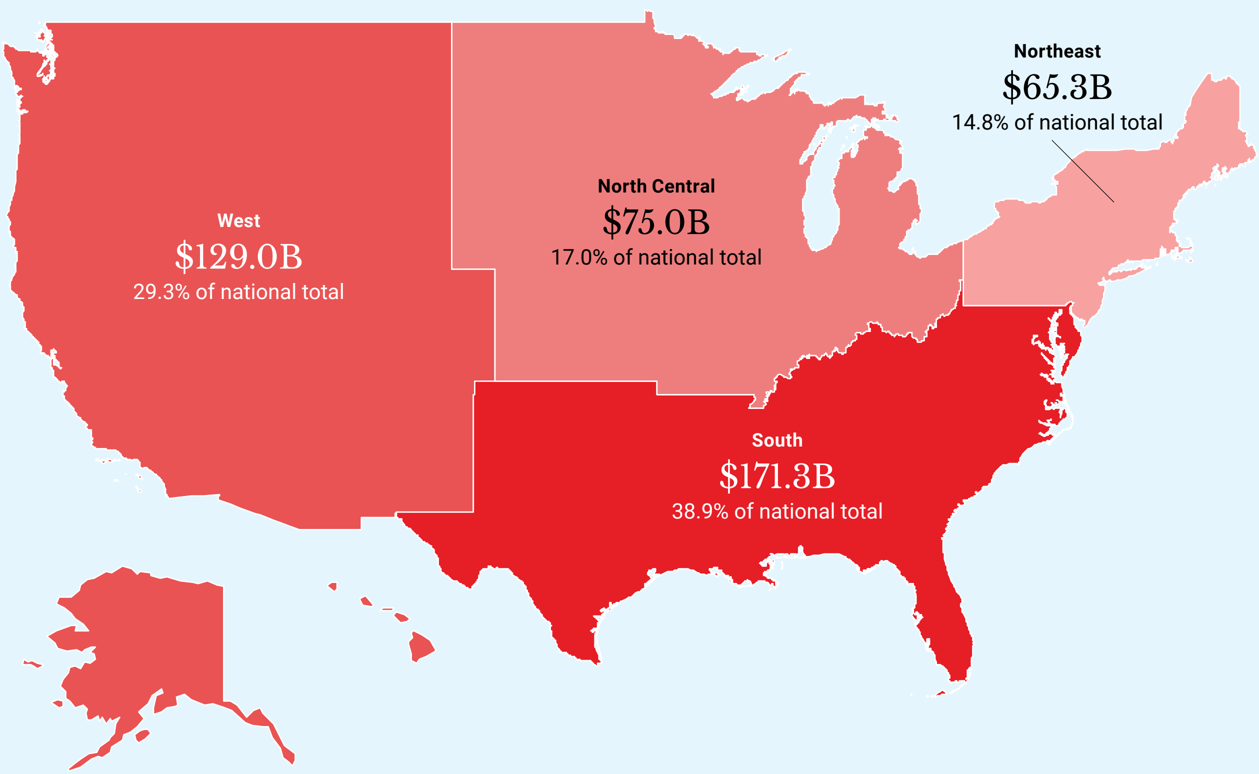
The Northeast and North Central regions contribute less to national lost earnings than their population shares might suggest. In the Northeast, higher per-person penalties are partly offset by smaller population size, resulting in a somewhat proportional contribution to national losses. In the North Central region, lower earnings penalties and lower comparison group earnings contribute to lower aggregate losses.

The West is the only region that contributes more to national lost earnings than its share of the justice-impacted population. This appears to be driven by higher earnings levels among matched peers, which means that a similar percentage penalty translates into a larger dollar loss.

Appendix Table 32. Percent Share of National Annual Lost Earnings from Arrest and Conviction Record

Treatment Group	Northeast	North Central	South	West
Arrest or non-conviction	18.7%	18.3%	34.7%	28.3%
Misdemeanor Conviction	16.3%	17.4%	33.2%	33.1%
Non-violent Felony Conviction	12.1%	16.1%	45.2%	26.6%
Violent Felony Conviction	14.1%	17.1%	44.3%	24.5%
Any Arrest or Conviction	**14.8%**	**17.0%**	**38.9%**	**29.3%**

Appendix Figure 3. Annual Earnings Lost from Arrest and Conviction Records, by Region



Appendix C. Limitations

Several limitations should be considered when interpreting these findings. The analysis uses public-use NLSY97 data, which provides rich longitudinal information but does not include state-level geographic identifiers.

As a result, state-level estimates are constructed by applying regional earnings penalties to state-level population estimates from CSI's Data Model. This approach is necessary given the available data, but it means that state estimates should be interpreted as modeled estimates rather than direct state-level measurements. These estimates do not capture differences in state policy, employer practices, court systems, implementation of record sealing, or local labor-market conditions within Census regions.

> **The analysis is observational and does not establish a causal relationship** between arrest or conviction records and later earnings outcomes.

While propensity score matching helps improve comparability between respondents with and without records by reducing observable selection bias, matching cannot account for unobserved differences between respondents with and without records or fully isolate the independent effect of a record itself. Characteristics not measured in the survey, or not observed before treatment, may still affect both legal system involvement and later earnings. As a result, the findings should be interpreted as estimated earnings penalties associated with records, not definitive proof that the record alone caused the full difference in earnings.

The two-part earnings model helps account for the fact that annual earnings reflect both employment and earnings conditional on employment. However, the model does not fully eliminate selection concerns. People with zero earnings may be out of the labor force for many reasons, including unemployment, caregiving, schooling, disability, incarceration, health limitations, or other personal circumstances. The model improves measurement by separating employment from earnings among workers, but it cannot perfectly identify every reason why someone did or did not report earnings.

Another limitation is that sample sizes become smaller when the analysis is divided by region, race, sex, or record severity. This is especially important for Hispanic respondents and for felony conviction subgroups. Some subgroup estimates are less stable and should be interpreted cautiously. The Hispanic subgroup results, in particular, were less consistent than the Black and white subgroup results and should not be operationalized without further investigation.

The record categories are also based on broad NLSY97 offense categories, which do not perfectly map onto state criminal statutes. Some offenses can be charged as either misdemeanors or felonies depending on state law, offense circumstances, prior record, and judicial discretion. The analysis uses the most consistent classification possible given the survey data, but some misclassification is possible.

Finally, the aggregate national, regional, and state estimates depend on CSI Data Model population estimates. The confidence intervals in the earnings analysis reflect uncertainty in the estimated earnings penalties, but the aggregate estimates do not fully incorporate uncertainty in the population counts themselves.

Appendix D. Areas for Further Research

Several areas deserve further research. One important area is the economic consequences of arrest and non-conviction records.

This analysis finds substantial earnings penalties for people without convictions, but the available data cannot determine how much of that penalty is driven by background checks, pretrial detention, court costs, interrupted employment, stigma, or other mechanisms. Better understanding these pathways would help clarify why non-conviction records are associated with such large economic losses and which policy interventions may be most effective.

Additional research is also needed on racial disparities, particularly among Hispanic communities. The Hispanic subgroup results were less consistent than those for Black and white respondents, likely in part because of smaller sample sizes. Future work should examine employment rates, weeks worked, geography, immigration-related labor-market factors where measurable, and industry differences to better understand these patterns.

Future research should also examine intersectional groups, particularly Black women and Latina women. This report separately examines race and sex, but it does not fully estimate how the association between records and earnings differs across combined race-sex groups. Given the large sex-based penalties and the large penalties observed for Black respondents with conviction records, intersectional analysis is an important next step.

Future research should also explore the extent to which estimated earnings penalties reflect causal effects of records themselves versus broader structural and socioeconomic disadvantages associated with legal system involvement. Quasi-experimental and longitudinal approaches, including analyses of record sealing, expungement, automatic clearance policies, or changes in employer screening practices, could help isolate the mechanisms through which records affect employment and earnings outcomes. Better causal evidence would strengthen understanding of which policies are most effective at reducing long-term economic harms.

There is also room to examine outcomes beyond earnings. Arrest and conviction records can affect employment, housing, education, health, family relationships, self-esteem, wealth accumulation, and long-term financial stability. The NLSY97 contains rich longitudinal information that could be used to study some of these outcomes in future publications.

Future work should also investigate intergenerational consequences. Parental legal system involvement may affect children's economic, educational, and health outcomes. Understanding the generational scarring associated with records would provide a fuller picture of the long-term consequences of legal system involvement and the potential benefits of record sealing policies.

An important next step is building a stronger evidence base on the long-term impacts of record sealing policies. Because many Clean Slate laws have only recently taken effect, there is still limited longitudinal evidence on how these policies shape employment, earnings, economic mobility, housing stability, and other life outcomes over time. In addition, over half of directly impacted respondents in states that have begun implementing Clean Slate reported that they had not heard of their state's Clean Slate law.[63] Effective implementation coupled with awareness are necessary to ensure individuals fully benefit from Clean Slate policies.[64] To help address these gaps in the evidence, The Clean Slate Initiative supports a broad research agenda examining the implementation and effects of record sealing policies across multiple states and populations.

> **Effective implementation coupled with awareness are necessary** to ensure individuals fully benefit from Clean Slate policies.

This work includes both near-term and long-term research approaches. Survey research and qualitative studies provide earlier insight into how people experience record sealing and whether it changes behavior, such as applying for jobs, housing, or educational opportunities. Longer-term research with administrative data and quasi-experimental designs will help identify the causal effects of Clean Slate policies on employment, earnings, and broader measures of well-being. Together, this growing body of research will help clarify the effects of record sealing across populations, labor markets, and policy environments.

Acknowledgements

We extend our sincere gratitude to the members of CSI's Research Advisory Committee (RAC) including **Ames Grawert**, Senior Counsel and John L. Neu Justice Counsel in the Brennan Center's Justice Program, **Le'Ann Duran**, Senior Director and Co-Lead of the Justice and Mobility Fund at Blue Meridian Partners, and **Akua Amaning**, Director of Public Safety for the Center for American Progress, as well as **Mindy Tarlow**, Senior Fellow and Research Professor directing the SCALE + Lab at the New York University Marron Institute of Urban Management and CSI Board Member, and **Dr. Nyron Crawford**, Associate Professor of Political Science at Temple University, for their guidance, insight, and continued support throughout this project. Their expertise helped shape the direction and rigor of this work.

We are especially grateful to **Dr. Terry-Ann Craigie**, Associate Professor of Economics at Smith College and economics fellow in the Brennan Center's Justice Program for her careful review of the methodology and report, and to **Dr. JJ Prescott**, Henry King Ransom Professor of Law at the University of Michigan, for offering substantive comments on an early version of the analysis. Their thoughtful feedback has undoubtedly strengthened this research in meaningful ways.

Endnotes

1. Research has found that young adults (age 18 to 25) have the highest rates of incarceration and recidivism (see: Griffin, Kelley, January 14, 2025, "More Support, Less Punishment: Getting Young People on a Better Path, National Conference of State Legislatures, available at: https://www.ncsl.org/state-legislatures-news/details/more-support-less-punishment-getting-young-people-on-a-better-path). In addition, the "age-crime curve" has been widely studied in the field of criminology, with criminal offending consistently found to peak during the teenage and early adulthood years (For example, see: Tuttle, James, 2025, "The age-crime curve is dead, long live the age-crime curve! An examination of the FBI's 2024 arrest data," Journal of Crime and Justice, 49(2).

2. Matthew Friedman. "Just Facts: As Many Americans Have Criminal Records as College Diplomas" Brennan Center for Justice. (2015). Available at: https://www.brennancenter.org/our-work/analysis-opinion/just-facts-many-americans-have-criminal-records-college-diplomas

3. According to the National Consumer Law Center (2019), around 94% of employees and 90% of landlords use background checks to screen potential employees and tenants. See: Ariel Nelson (December 10, 2019). "Broken Records Redux: How Errors by Criminal Background Check Companies Continue to Harm Consumers Seeking Jobs and Housing." National Consumer Law Center. Available at: https://www.nclc.org/resources/report-broken-records-redux/

4. Ericka B. Adams, Elsa Y. Chen, and Sarah E. Lageson (2025). "The Symbiotic Harm of a Criminal Record." Criminal Justice and Behavior, 53(3). https://doi.org/10.1177/00938548251379861

5. Amanda Y. Agan, Andrew Garin, Dmitri K. Koustas, Alexandre Mas, and Crystal Yang, "Can you Erase the Mark of a Criminal Record? Labor Market Impacts of Criminal Record Remediation," NBER Working Paper 32394 (2024), https://doi.org/10.3386/w32394.

6. Pager D, Western B, Sugie N. Sequencing Disadvantage: Barriers to Employment Facing Young Black and White Men with Criminal Records. Ann Am Acad Pol Soc Sci. 2009 May;623(1):195-213. doi: 10.1177/0002716208330793. PMID: 23459367; PMCID: PMC3583356.

7. Adams, E. B., Chen, E. Y., & Lageson, S. E. (2025). The symbiotic harm of a criminal record. Criminal Justice and Behavior. Advance online publication. https://doi.org/10.1177/00938548251379861

8. U.S. Chamber of Commerce. (2026, February 20). America works data center: The U.S. workforce by the numbers. https://www.uschamber.com/workforce/america-works-data-center

9. Bucknor, C., & Barber, A. (2016, June). The price we pay: Economic costs of barriers to employment for former prisoners and people convicted of felonies. Center for Economic and Policy Research. https://cepr.net/images/stories/reports/employment-prisoners-felonies-2016-06.pdf

10. Craigie, T.-A., Grawert, A., & Kimble, C. (2020, September 15). Conviction, imprisonment, and lost earnings: How involvement with the criminal justice system deepens inequality. Brennan Center for Justice. https://www.brennancenter.org/our-work/research-reports/conviction-imprisonment-and-lost-earnings-how-involvement-criminal

11. For more information, see: https://www.cleanslateinitiative.org/states

12. Generally, early adulthood is between the ages of 18 and 25. However, for people with felony convictions, especially violent felonies, we captured data from younger populations due to transfer laws that exist in nearly all states. For more information on this, please refer to the Methodology section.

For Pennsylvania: Pennsylvania State Police cited in SEARCH (2025, March). Navigating Clean Slate Implementation: Appendix, p. J-11. For Michigan: Michigan State Police cited in Sandiford, K. and John S. Cooper (2024, April 11). Clean Slate Year 3: The First Year of Automatic Expungments - Looking Back & Looking Ahead. Safe and Just Michigan. Available at: https://safeandjustmi.org/wp-content/uploads/2024/04/Clean_Slate_Year_3_Report.pdf

13. We defined annual employment as having worked for at least one week out of the year.

14. National Inventory of Collateral Consequences of Conviction. (n.d.). Collateral consequences inventory. Retrieved May 13, 2026, from https://niccc.nationalreentryresourcecenter.org/consequences

15. This finding is statistically significant at the 0.05 level (95% confidence interval; p = .026).

16. United States Sentencing Commission. (2023, November). Demographic differences in federal sentencing. Available at: https://www.ussc.gov/research/research-reports/2023-demographic-differences-federal-sentencing

17. Refer to Appendix B for sample sizes of record types by race and sex.

18. U.S. Bureau of Labor Statistics. (2026, March 3). Women in the Labor Force. https://www.bls.gov/cps/demographics/women-labor-force.htm

19. We use a two-step model to calculate expected annual earnings. The details of this model and how we capture likelihood of employment and earnings conditional on employment are included in the methodology section of this report.

20. Love, M. (2026). 50-State Comparison: Expungement, Sealing & Other Record Relief. Collateral Consequences Resource Center. https://ccresourcecenter.org/state-restoration-profiles/50-state-comparisonjudicial-expungement-sealing-and-set-aside-2-2/

21. Again, because we did not have access to state-level indicators for this analysis, we could not see how these earning differences varied for states with and without these protections.

22. Amanda Y. Agan, Andrew Garin, Dmitri K. Koustas, Alexandre Mas, and Crystal Yang, "Can you Erase the Mark of a Criminal Record? Labor Market Impacts of Criminal Record Remediation," NBER Working Paper 32394 (2024), https://doi.org/10.3386/w32394.

23. In addition, most legal involvement involves a cost in terms of legal representation, bail, and/or court fees, all of which can affect economic mobility later in life.

24. For more information on how confidence intervals were constructed, please refer to the Methodology section. For the final confidence intervals for our earning findings, please refer to Appendix B.

25. Pager, D. (2003). The Mark of a Criminal Record. American Journal of Sociology, 108(5), 937–975. https://doi.org/10.1086/374403

26. Refer to Appendix B for sample sizes of record types by race and sex.

27. Pager D, Western B, Sugie N. Sequencing Disadvantage: Barriers to Employment Facing Young Black and White Men with Criminal Records. Ann Am Acad Pol Soc Sci. 2009 May;623(1):195-213. doi: 10.1177/0002716208330793. PMID: 23459367; PMCID: PMC3583356.

28. Our analysis did not account for post-conviction sentencing factors such as probation or imprisonment. These specific outcomes, particularly active incarceration, can significantly influence an individual's earnings following legal system involvement. However, since the respondents in our sample remained crime-free throughout the 14-year post-treatment observation period, any ongoing incarceration at that stage would likely be the result of a more serious offense, such as a violent felony.

29. Richard Frey and Carolina Aragao (March 4, 2025). "Gender Pay Gap in U.S. Has Narrowed Slightly Over 2 Decades." Pew Research Center. Available at: https://www.pewresearch.org/short-reads/2025/03/04/gender-pay-gap-in-us-has-narrowed-slightly-over-2-decades/

30. Again, it is important to note that the Hispanic sample in this study is small and should be interpreted with caution and should not be overgeneralized without further analysis. Refer to Appendix B for sample sizes of record types by race and sex.

31. Like the Hispanic survey sample in this study, the sample of female respondents with violent felony convictions is small. These findings should be interpreted with caution and should not be overgeneralized without further analysis. Refer to Appendix B for sample sizes of record types by race and sex.

32. For more information on the Data Model, please refer to our methodology document available at our website at www.cleanslateinitiative.org/data. State-level population estimates are available to the public on our Data Dashboard.

33. For additional information on regional earnings losses, refer to Appendix B.

34. This estimate assumes a 20% down payment on the median home value in 2021, obtained from the Federal Reserve Bank of St. Louis, which is available at: https://fred.stlouisfed.org/series/MSPUS

35. The average cost of 4-year public college in 2022-2023 was around $108,000 in total, over 4 years. See: https://educationdata.org/average-cost-of-college

36. The average 401K balance for those ages 45-49 is $152,000 according to a report by Fidelity. See: "How Do Your Retirement Savings Stack Up?" Available at: https://www.fidelity.com/learning-center/personal-finance/average-retirement-savings

37. Mayson, Sandra Gabriel and Stevenson, Megan, Misdemeanors by the Numbers (March 2020). Boston College Law Review, Vol 61, Issue 3, Available at SSRN: https://ssrn.com/abstract=3374571 or http://dx.doi.org/10.2139/ssrn.3374571

38. Oklahoma's Clean Slate law makes a narrow set of felony records eligible, involving convictions to an offense that has now been reclassified as a misdemeanor.

39. See: https://www.nlsinfo.org/content/cohorts/nlsy97/topical-guide/crime/introduction

40. Arrest or non-conviction, non-violent misdemeanor conviction, hard-to-clear misdemeanor conviction (DUI and simple assault offenses), non-violent felony conviction, and violent felony conviction. See: www.cleanslateinitiative.org/data for more information about CSI's Data Model and methodology.

41. Many states have different age limits and transfer ages for juveniles to be tried as adults. Given the information reported by the National Conference of State Legislators, and the Interstate Commission for Juveniles, we excluded any non-violent felony convictions occurring before a respondent was sixteen years of age and we did not exclude any violent felony convictions.

42. Austin PC. An Introduction to Propensity Score Methods for Reducing the Effects of Confounding in Observational Studies. Multivariate Behav Res. 2011 May;46(3):399-424. doi: 10.1080/00273171.2011.568786. Epub 2011 Jun 8. PMID: 21818162; PMCID: PMC3144483.

43. We did not utilize the restricted-access survey data that contains state-level geographic information. However, even with restricted-access data, extending the analysis to the state level would likely be difficult due to the small sample sizes available across treatment groups. Therefore, we used regional indicators to estimate state-level earnings losses associated with various arrest and conviction records.

44. Specifically, we separate whether someone is working at all (employment) from how much they earn if they are working (wages). These are each driven by different factors. For example, a record might make it harder to get a job in the first place, but it could also affect the type of job someone gets and how much it pays. If you just look at overall earnings without separating these, you can't tell whether lower earnings are due to people not working, earning less when they do work, or both.

45. See: https://www.cleanslateinitiative.org/data

46. We classified theft, destruction of property, drug crimes, and other offense categories as offenses that could be either misdemeanor or felony offenses depending on sentencing and criminal history information.

47. National Conference of State Legislators. (2019, January 19). Misdemeanor Sentencing Trends. https://www.ncsl.org/civil-and-criminal-justice/misdemeanor-sentencing-trends

48. National Conference of State Legislators. (2019, July 16). Misdemeanor Justice: Statutory Guidance for Sentencing. https://www.ncsl.org/civil-and-criminal-justice/misdemeanor-justice-statutory-guidance-for-sentencing

49. Horowitz, J. (2021, April 15). States Can Shorten Probation and Protect Public Safety. https://www.pew.org/en/research-and-analysis/reports/2020/12/states-can-shorten-probation-and-protect-public-safety

50. According to the National Conference of State Legislators (NCSL), six states have a maximum jail term greater than one year and thirty four states have maximum terms for misdemeanor probation between one and three years.

51. According to the National Conference of State Legislators (NCSL), statutes authorize a range of penalties that can be imposed for misdemeanors. These typically include no penalty, time served, a fine with no incarceration, a sentence to probation, incarceration with no fine or a combination of incarceration and a fine. It is the court's discretion which penalty or combination of penalties to order.

52. Adult prison is equivalent to adult correctional institution as referred to by the NLSY97.

53. Adult prison is equivalent to adult correctional institution as referred to by the NLSY97.

54. Andrade C. Mean Difference, Standardized Mean Difference (SMD), and Their Use in Meta-Analysis: As Simple as It Gets. J Clin Psychiatry. 2020 Sep 22;81(5):20f13681. doi: 10.4088/JCP.20f13681. PMID: 32965803.

55. Tran, D. (2026, March 18). Two-stage hurdle models: Predicting Zero-inflated outcomes. Towards Data Science. https://towardsdatascience.com/two-stage-hurdle-models-predicting-zero-inflated-outcomes/

56. Craigie, T.-A., Grawert, A., & Kimble, C. (2020, September 15). Conviction, imprisonment, and lost earnings: How involvement with the criminal justice system deepens inequality. Brennan Center for Justice. https://www.brennancenter.org/our-work/research-reports/conviction-imprisonment-and-lost-earnings-how-involvement-criminal

57. As a result, subgroup and regional estimates should be interpreted with caution.

58. Andrade C. Mean Difference, Standardized Mean Difference (SMD), and Their Use in Meta-Analysis: As Simple as It Gets. J Clin Psychiatry. 2020 Sep 22;81(5):20f13681. doi: 10.4088/JCP.20f13681. PMID: 32965803.

59. Roozbeh Hosseini, Karen Kopecky and Kai Zhao. (January 2021). "How Important is Health Inequality for Lifetime Earnings Inequality?" Federal Reserve Bank of Atlanta Working Paper Series. Working Paper 2021-1.

60. See: Jesse Kelley and Samuel Sinyangwe. (2024). Clean Slate Laws: Pathways to Equity and Justice for Black Communities in America. The Clean Slate Initiative. Available at: https://www.cleanslateinitiative.org/research-data-publications/2024-bhm-policy-short

61. The Other racial group was excluded from the subgroup analysis because of small sample sizes.

62. Chavez, Laura. (2025). Research Brief: Awareness of Clean Slate Laws in Pennsylvania, Utah, and Michigan. The Clean Slate Initiative. Available at: https://www.cleanslateinitiative.org/research-data-publications/awareness-of-clean-slate-laws-pa-ut-mi

63. See: Lageson, Sarah E., Ericka B. Adams, and Elsa Y. Chen (2026). Automating Administrative Burden in Algorithmic Criminal Record Expungement. Law and Society Review. Available at: https://www.cambridge.org/core/journals/law-and-society-review/article/automating-administrative-burden-in-algorithmic-criminal-record-expungement/9F9388FA046EA425F4A5362DE590C63D

64. For more information about the RAC, see: https://www.cleanslateinitiative.org/research-data

THE
CLEAN SLATE
INITIATIVE

CLEANSLATEINITIATIVE.ORG